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NOTICES

OF THE

PROCEEDINGS

AT THE

MEETINGS OF THE MEMBERS

OF THE

Royal Institution of Great Britain,

WITH

ABSTRACTS OF THE DISCOURSES

DELIVERED AT

THE EVENING MEETINGS.

VOLUME XV.
1896—1898.

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Royal Institution of Great Britain.

WEEKLY EVENING MEETING,

Friday, January 17, 1896.

SIR FREDERICK ABEL, Bart. K.C.B. D.C.L. LL.D. F.R.S. Vice-President, in the Chair.

THE RIGHT HON. LORD RAYLEIGH, M.A. D.C.L. LL.D. F.R.S. *M.R.I.* Professor of Natural Philosophy R.I.

More about Argon.

(Abstract.)

IN our original paper * are described determinations by Professor Ramsay, of the density of argon prepared with the aid of magnesium. The volume actually weighed was 163 c.c. and the adopted mean result was 19·941, referred to $O_2 = 16$. At that time a satisfactory conclusion as to the density of argon prepared by the oxygen method of Cavendish had not been reached, although a preliminary result (19·7) obtained from a mixture of argon and oxygen † went far to show that the densities of the gases prepared by the two methods were the same. In order further to test the identity of the gases, it was thought desirable to pursue the question of density; and I determined, as the event proved, somewhat rashly, to attempt large scale weighings of pure argon with the globe of 1800 c.c. capacity employed in former weighings of gases ‡ which could be obtained in quantity.

The accumulation of the 3 litres of argon, required for convenient working, involved the absorption of some 300 litres of nitrogen, or about 800 litres of the mixture with oxygen. This was effected at the Royal Institution with the apparatus already described,§ and which is capable of absorbing the mixture at the rate of about 7 litres per hour. The operations extended themselves over nearly three weeks, after which the residual gases amounting to about 10 litres, still containing oxygen with a considerable quantity of nitrogen, were removed to the country and transferred to a special apparatus where it could be prepared for weighing.

For this purpose the purifying vessel had to be arranged somewhat differently from that employed in the preliminary absorption

* Rayleigh and Ramsay, Phil. Trans. vol. 186 A, pp. 221, 238, 1895.

† Loc. cit. p. 221.

‡ Roy. Soc. Proc. February 1888; February 1892; March 1893.

§ Phil. Trans. loc. cit. p. 219.

of nitrogen. When the gas is withdrawn for weighing, the space left vacant must be filled up with liquid, and afterwards when the gas is brought back for repurification, the liquid must be removed. In order to effect this the working vessel (Fig. 7*) communicates by means of a siphon with a 10-litre "aspirating bottle," the ends of the siphon being situated in both cases near the bottom of the liquid. In this way the alkaline solution may be made to pass backwards and forwards, in correspondence with the desired displacements of gas.

There is, however, one objection to this arrangement which requires to be met. If the reserve alkali in the aspirating bottle were allowed to come into contact with air, it would inevitably dissolve nitrogen, and this nitrogen would be partially liberated again in the working vessel, and so render impossible a complete elimination of that gas from the mixture of argon and oxygen. By means of two more aspirating bottles an atmosphere of *oxygen* was maintained in the first bottle, and the outermost bottle, connected with the second by a rubber hose, gave the necessary control over the pressure.

Five glass tubes in all were carried through the large rubber cork by which the neck of the working vessel was closed. Two of these convey the electrodes: one is the siphon for the supply of alkali, while the fourth and fifth are for the withdrawal and introduction of the gas, the former being bent up internally, so as to allow almost the whole of the gaseous contents to be removed. The fifth tube, by which the gas is returned, communicates with the fall-tube of the Töpler pump, provision being made for the overflow of mercury. In this way the gas, after weighing, could be returned to the working vessel at the same time that the globe was exhausted. It would be tedious to describe in detail the minor arrangements. Advantage was frequently taken of the fact that *oxygen* could always be added with impunity, its presence in the working vessel being a necessity in any case.

When the nitrogen had been so far removed that it was thought desirable to execute a weighing, the gas on its way to the globe had to be freed from oxygen and moisture. The purifying tubes contained copper and copper oxide maintained at a red heat, caustic soda, and phosphoric anhydride. In all other respects the arrangements were as described in the memoir on the densities of the principal gases,† the weighing globe being filled at 0°, and at the pressure of the manometer gauge.

The process of purification with the means at my command proved to be extremely slow. The gas contained more nitrogen than had been expected, and the contraction went on from day to day until I almost despaired of reaching a conclusion. But at last the visible contraction ceased, and soon afterwards the yellow line of nitrogen

* Phil. Trans. loc. cit. p. 218.

† Roy. Soc. Proc. vol. 53, p. 134, 1893.

disappeared from the spectrum of the jar discharge.* After a little more sparking, a satisfactory weighing was obtained on May 22, 1895; but, in attempting to repeat, a breakage occurred, by which a litre of air entered, and the whole process of purification had to be recommenced. The object in view was to effect, if possible, a *series* of weighings with intermediate sparkings, so as to obtain evidence that the purification had really reached a limit. The second attempt was scarcely more successful, another accident occurring when two weighings only had been completed. Ultimately a series of four weighings were successfully executed, from which a satisfactory conclusion can be arrived at.

May 22	3·2710	
June 4	3·2617	
June 7	3·2727	
June 13	3·2652	
June 18	3·2750	3·2746
June 25	3·2748	
July 2	3·2741	

The results here recorded are derived from the comparison of the weighings of the globe "full" with the mean of the preceding and following weighings "empty," and they are corrected for the errors of the weights and for the shrinkage of the globe when exhausted, as explained in former papers. In the last series, the experiment of June 13 gave a result already known to be too low. The gas was accordingly sparked for fourteen hours more. Between the weighings of June 18 and June 25 there was nine hours' sparking, and between those of June 25 and July 2 about eight hours' sparking. The mean of the last three, viz. 3·2746, is taken as the definitive result, and it is immediately comparable with 2·6276, the weight under similar circumstances of oxygen.† If we take $O_2 = 16$, we obtain for argon

$$19 \cdot 940,$$

in very close agreement with Professor Ramsay's result.

The conclusion from the spectroscopic evidence that the gases isolated from the atmosphere by magnesium and by oxygen are essentially the same is thus confirmed.

The refractivity of argon was next investigated, in the hope that it might throw some light upon the character of the gas. For this

* Jan. 29.—When the argon is nearly pure, the arc discharge (no jar connected) assumes a peculiar purplish colour, quite distinct from the greenish hue apparent while the oxidation of nitrogen is in progress and from the sky blue observed when the residue consists mainly of oxygen.

† Roy. Soc. Proc. vol. 53, p. 144, 1893.

purpose absolute measurements were not required. It sufficed to compare the pressures necessary in two columns of air and argon of equal lengths, in order to balance the retardations undergone by light in traversing them.

The arrangement was a modification of one investigated by Fraunhofer, depending upon the interference of light transmitted through two parallel vertical slits placed in front of the object-glass of a telescope. If there be only one slit, and if the original source, either a distant point or a vertical line of light, be in focus, the field is of a certain width, due to "diffraction," and inversely as the width of the slit. If there be two equal parallel slits whose distance apart is a considerable multiple of the width of either, the field is traversed by bands of width inversely as the distance between the slits. If from any cause one of the portions of light be retarded relatively to the other, the bands are displaced in the usual manner, and can be brought back to the original position only by abolishing the relative retardation.

When the object is merely to see the interference bands in full perfection, the use of a telescope is not required. The function of the telescope is really to magnify the slit system,* and this is necessary when, as here, it is desired to operate separately upon the two portions of light. The apparatus is, however, extremely simple, the principal objection to it being the high magnifying power required, leading under ordinary arrangements to a great attenuation of light. I have found that this objection may be almost entirely overcome by the substitution of cylindrical lenses, magnifying in the horizontal direction only, for the spherical lenses of ordinary eye-pieces. For many purposes a single lens suffices, but it must be of high power. In the measurements about to be described most of the magnifying was done by a lens of home manufacture. It consisted simply of a round rod, about $\frac{1}{6}$ inch (4 mm.) in diameter, cut by Mr. Gordon from a piece of plate glass.† This could be used alone; but as at first it was thought necessary to have a web, serving as a fixed mark to which the bands could be referred, the rod was treated as the object-glass of a compound cylindrical microscope, the eye-piece being a commercial cylindrical lens of $1\frac{1}{4}$ inch (31 mm.) focus. Both lenses were mounted on adjustable stands, so that the cylindrical axes could be made accurately vertical, or, rather, accurately parallel to the length of the original slit. The light from an ordinary paraffin lamp now sufficed, although the magnification was such as to allow the error of setting to be less than 1/20 of a band interval. It is to be remembered that with this arrangement the various parts of the length of a band correspond, not to the various parts of the original slit, but rather to the various parts of the object-glass. This

* Brit. Assoc. Report, 1893, p. 703.

† Preliminary experiments had been made with ordinary glass cane and with tubes charged with water.

departure from the operation of a spherical eyepiece is an advantage, inasmuch as optical defects show themselves by deformation of the bands instead of by a more injurious encroachment upon the distinction between the dark and bright parts.

The collimating lens A (Fig. 1) is situated 20 feet (7 metres) from the source of light. B C are the tubes, one containing dry air, the other the gas to be experimented upon. They are 1 foot (30·5 cm.) long, and of ½ inch (1·3 cm.) bore, and they are closed at the ends with small plates of parallel glass cut from the same strip. E is the object-glass of the telescope, about 3 inches (7·6 cm.) in diameter. It is fitted with a cap D, perforated by two parallel slits. Each slit is ¼ inch (6 mm.) wide, and the distance between the middle lines of the slits is 1½ inches (38 mm.).

The arrangements for changing the tubes and varying the pressures of the gases are sketched in Fig. 2. A gas pipette, D E, communicates with the tube C, so that by motion of the reservoir E and consequent flow of mercury through the connecting hose, part of the gas may be transferred. The pressure was measured by a U-shaped

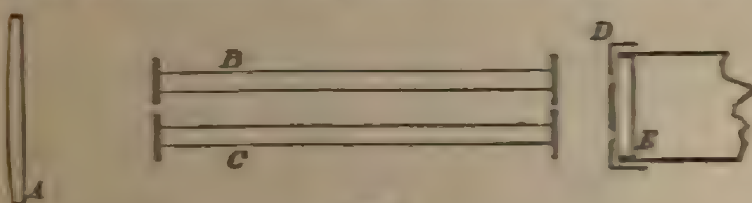

Fig. 1.

manometer F, containing mercury. This was fitted below with a short length of stout rubber tubing G, to which was applied a squeezer H. The object of this attachment was to cause a rise of mercury in both limbs immediately before a reading, and thus to avoid the capillary errors that would otherwise have entered. A similar pipette and manometer were connected with the air tube B. In order to be able, if desired, to follow with the eye a particular band during the changes of pressure (effected by small steps and alternately in the two tubes), diminutive windlasses were provided by which the motions of the reservoirs (E) could be made smooth and slow. In this way all doubt was obviated as to the identity of a band; but after a little experience the precaution was found to be unnecessary.

The manner of experimenting will now be evident. By adjustment of pressures the centre of the middle band was brought to a definite position, determined by the web or otherwise, and the pressures were measured. Both pressures were then altered and adjusted until the band was brought back precisely to its original position. The ratio of the changes of pressure in the inverse ratio of the refractivities

($\mu = 1$) of the gases. The process may be repeated backwards and forwards any number of times, so as to eliminate in great degree err ors of the settings and of the pressure readings.

During these observations a curious effect was noticed, made possible by the independent action of the parts of the object-glass situated at various levels, as already referred to. When the bands were stationary, they appeared straight, or nearly so, but when in motion, owing to changes of pressure, they became curved, even in passing the fiducial position, and always in such a manner that the

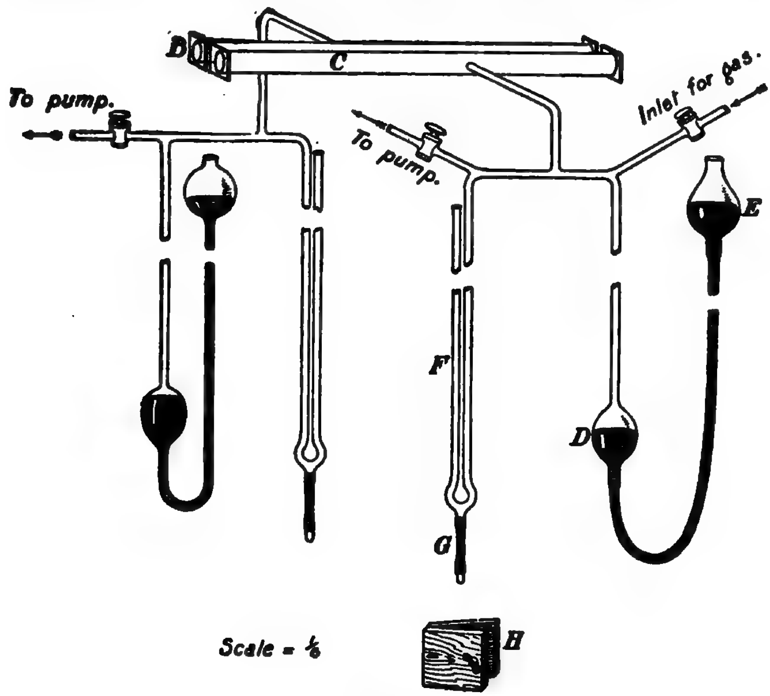

FIG. 2.

ends led. The explanation is readily seen to depend upon the temporary changes of temperature which accompany compression or rarefaction. The full effect of a compression, for example, would not be attained until the gas had cooled back to its normal temperature, and this recovery of temperature would occur more quickly at the top and bottom, where the gas is in proximity to the metal, than in the central part of the tube.

The success of the measures evidently requires that there should be no apparent movement of the bands apart from real retardations

in the tubes. As the apparatus was at first arranged, this condition was insufficiently satisfied. Although all the parts were carried upon the walls of the room, frequent and somewhat sudden displacements of the bands relatively to the web were seen to occur, probably in consequence of the use of wood in some of the supports. The observations could easily be arranged in such a manner that no systematic error could thence enter, but the agreement of individual measures was impaired. Subsequently a remedy was found in the use of a second system of bands, formed by light which passed just above the tubes, to which, instead of to the web, the movable bands were referred. The coincidence of the two systems could be observed with accuracy, and was found to be maintained in spite of movements of both relatively to the web.

In the comparisons of argon and air (with nearly the same refractivities) the changes of pressure employed were about 8 inches (20 cm.), being deductions from the atmospheric pressure. In one observation of July 26, the numbers, representing suctions in inches of mercury, stood

Argon.	Air.
8·54	99·6
0·01	1·77
8·53	8·19

$$\text{Ratio} = 0\cdot961,$$

signifying that 8·53 inches of argon balanced 8·19 inches of dry air. Four sets, during which the air and argon (from the globe as last filled for weighing) were changed, taken on July 17, 18, 19, 26, gave respectively for the final ratio 0·962, 0·961, 0·961, 0·960, or as the mean

$$\frac{\text{Refractivity of argon}}{\text{Refractivity of air}} = 0\cdot961.$$

The evidence from the refractivities, as well as from the weights, is very unfavourable to the view that argon is an allotropic form of nitrogen such as would be denoted by N_3.

The above measurements, having been made with lamp-light, refer to the most luminous region of the spectrum, say in the neighbourhood of D. But since no change in the appearance of the bands at the two settings could be detected, the inference is that the dispersions of the two gases are approximately the same, so that the above ratio would not be much changed, even if another part of the spectrum were chosen. It may be remarked that the displacement actually compensated in the above experiments amounted to about forty bands, each band corresponding to about $\frac{1}{5}$ inch (5 mm.) pressure of mercury.

Similar comparisons have been made between air and helium.

The latter gas, prepared by Professor Ramsay, was brought from London by Mr. W. Randall, who further gave valuable assistance in the manipulations. It appeared at once that the refractivity of helium was remarkably low, 13 inches pressure of the gas being balanced by less than 2 inches pressure of air. The ratios given by single comparisons on July 29 were 0·147, 0·146, 0·145, 0·146, mean 0·146; and on July 30, 0·147, 0·147, 0·145, 0·145, mean 0·146. The observations were not made under ideal conditions, on account of the smallness of the changes of air pressure; but we may conclude that with considerable approximation

$$\frac{\text{Refractivity of helium}}{\text{Refractivity of air}} = 0\cdot146.$$

The lowest refractivity previously known is that of hydrogen, nearly 0·5 of that of air.

The viscosity was investigated by the method of passage through capillary tubes. The approximate formula has been investigated by O. Meyer,* on the basis of Stokes' theory for incompressible fluids. If the driving pressure $(p_1 - p_2)$ is not too great, the volume V_2 delivered in time t through a tube of radius R and length λ is given by

$$V_2 = \pi t \frac{p_1^2 - p_2^2}{2p_2} \frac{R^4}{8\eta\lambda},$$

the volume being measured at the lower pressure p_2, and η denoting the viscosity of the gas. In the comparison of different gases V_2, p_1, p_2, R, λ may be the same, and then η is proportional to t.

In the apparatus employed two gas pipettes and manometers, somewhat similar to those shown in Fig. 2, were connected by a capillary tube of very small bore and about 1 metre long. The volume V_2 was about 100 c.c. and was caused to pass by a pressure of a few centimetres of mercury, maintained as uniform as possible by means of the pipettes. There was a difficulty, almost inherent in the use of mercury, in securing the right pressures during the first few seconds of an experiment; but this was not of much importance as the whole time t amounted to several minutes. The apparatus was tested upon hydrogen, and was found to give the received numbers with sufficient accuracy. The results, referred to dry air, were for helium 0·96; and for argon 1·21, somewhat higher than for oxygen which at present stands at the head of the list of the principal gases.

In the original memoir upon argon † results were given of weighings of the residue from the Bath gas after removal of oxygen, carbonic anhydride, and moisture, from which it appeared that the

* Pogg. Ann. vol. 127, p. 270, 1866.

† Rayleigh and Ramsay, Phil. Trans. A, vol. 186, p. 227, 1895.

proportion of argon was only one-half of that contained in the residue, after similar treatment from the atmosphere. After the discovery of helium by Professor Ramsay, the question presented itself as to whether this conclusion might not be disturbed by the presence in the Bath gas of helium, whose lightness would tend to compensate the extra density of argon.

An examination of the gas which had stood in my laboratory more than a year having shown that it still contained no oxygen, it was thought worth while to remove the nitrogen so as to determine the proportion that would refuse oxidation. For this purpose 200 c.c. were worked up with the oxygen until the volume, free from nitrogen, was reduced to 8 c.c. On treatment with pyrogallol and alkali the residue measured 3·3 c.c. representing argon, and helium, if present. On sparking the residue at atmospheric pressure and examining the spectrum, it was seen to be mainly that of argon, but with an unmistakable exhibition of D_3. At atmospheric pressure this line appears very diffuse in a spectroscope of rather high power, but the place was correct.

From another sample of residue from the Bath gas, vacuum tubes were charged by my son, Mr. R. J. Strutt, and some of them showed D_3 sharply defined and precisely coincident with the line of helium in a vacuum tube prepared by Professor Ramsay.

Although the presence of helium in the Bath gas is not doubtful, the quantity seems insufficient to explain the low density found in October 1894. In order to reconcile that density with the proportion of residue ($3·3/200 = 0·016$) found in the experiment just described, it would be necessary to suppose that the helium amounted to 25 per cent. of the whole residue of argon and helium. Experiment, however, proved that a mixture of argon and helium containing 10 per cent. of the latter gas showed D_3 more plainly than did the Bath residue. It is just possible that some of the helium was lost by diffusion during the long interval between the experiments whose results are combined in the above estimate.

Gas from the Buxton springs, kindly collected for me by Mr. A. McDougall, was found to contain no appreciable oxygen. The argon amounted to about 2 per cent. of the volume. When its spectrum was examined, the presence of D_3 was suspected, but the appearance was too feeble to allow of a definite statement being made. The proportion of helium is in any case very much lower than in the Bath gas.

Is helium contained in the atmosphere? Apart from its independent interest, this question is important in connection with the density of atmospheric argon. Since the spectrum of this gas does not show the line D_3, we may probably conclude that the proportion of helium is less than 3 per cent.; so that there would be less than 3×10^{-4} of helium in the atmosphere. The experiment about to be described was an attempt to carry the matter further, and is founded upon the observation by Professor Ramsay, that the solu-

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WEEKLY EVENING MEETING,

Friday, January 24, 1896.

SIR JAMES CRICHTON-BROWNE, M.D. LL.D. F.R.S. Treasurer and Vice-President, in the Chair.

PROFESSOR BURDON SANDERSON, M.D. D.C.L. LL.D. F.R.S.

Ludwig and Modern Physiology.

THE death of any discoverer—of any one who has added largely to the sum of human knowledge, affords a reason for inquiring what his work was and how he accomplished it. This inquiry has interest even when the work has been completed in a few years and has been limited to a single line of investigation—much more when the life has been associated with the origin and development of a new science and has extended over half a century.

The Science of Physiology as we know it came into existence fifty years ago with the beginning of the active life of Ludwig, in the same sense that the other great branch of Biology, the Science of Living Beings, as we now know it, came into existence with the appearance of the 'Origin of Species.' In the order of time Physiology had the advantage, for the new Physiology was accepted some ten years before the Darwinian epoch. Notwithstanding, the content of the Science is relatively so unfamiliar, that before entering on the discussion of the life and work of the man who, as I shall endeavour to show, had a larger share in founding it than any of his contemporaries, it is necessary to define its limits and its relations to other branches of knowledge.

The word Physiology has in modern times changed its meaning. It once comprehended the whole knowledge of Nature. Now it is the name for one of the two Divisions of the Science of Life. In the progress of investigation the study of that Science has inevitably divided itself into two: *Ontology*,* the Science of Living Beings; *Physiology*, the Science of Living Processes, and thus, inasmuch as Life consists in processes, of Life itself. Both strive to understand the complicated relations and endless varieties which present themselves in living Nature, but by different methods. Both refer to general principles, but they are of a different nature.

To the *Ontologist*, the student of Living Beings, Plants or

* I do not forget that this word is ordinarily used in another sense. Its suitability is my excuse for employing it.

Animals, the great fact of Evolution, namely, that from the simplest beginning our own organism, with its infinite complication of parts and powers, no less than that of every animal and plant, unfolds the plan of its existence—taken with the observation that that small beginning was, in all excepting the lowest forms, itself derived from two parents, equally from each—is the basis from which his study and knowledge of the world of living beings takes its departure. For on Evolution and Descent the explorer of the forms, distribution and habits of animals and plants has, since the Darwinian epoch, relied with an ever-increasing certainty, and has found in them the explanation of every phenomenon, the solution of every problem relating to the subject of his inquiry. Nor could he wish for a more secure basis. Whatever doubts or misgivings exist in the minds of "non-biologists" in relation to it, may be attributed partly to the association with the doctrine of Evolution of questions which the true naturalist regards as transcendental; partly to the perversion or weakening of meaning which the term has suffered in consequence of its introduction into the language of common life, and particularly to the habit of applying it to any kind of progress or improvement, anything which from small beginnings *gradually* increases. But, provided we limit the term to its original sense—the Evolution of a living being from its germ by a *continuous* not a gradual process, there is no conception which is more free from doubt either as to its meaning or reality. It is inseparable from that of Life itself, which is but the *unfolding* of a predestined harmony, of a prearranged consensus and synergy of parts.

The other branch of Biology, that with which Ludwig's name is associated, deals with the same facts in a different way. While Ontology regards animals and plants as individuals and in relation to other individuals, Physiology considers the processes themselves of which life is a complex. This is the most obvious distinction, but it is subordinate to the fundamental one, namely, that while Ontology has for its basis laws which are in force only in its own province, those of Evolution, Descent, and Adaptation, we Physiologists, while accepting these as true, found nothing upon them, using them only as guides to discovery, not for the purpose of explanation. Purposive Adaptation, for example, serves as a clue, by which we are constantly guided in our exploration of the tangled labyrinth of vital processes. But when it becomes our business to explain these processes—to say how they are brought about—we refer them not to biological principles of any kind, but to the Universal Laws of Nature. Hence it happens that with reference to each of these processes, our inquiry is rather how it occurs than why it occurs.

It has been well said that the Natural Sciences are the children of necessity. Just as the other Natural Sciences owed their origin to the necessity of acquiring that control over the forces of Nature without which life would scarcely be worth living, so Physiology

arose out of human suffering and the necessity of relieving it. It sprang indeed out of Pathology. It was suffering that led us to know, as regards our own bodies, that we had internal as well as external organs; and probably one of the first generalisations which arose out of this knowledge was, that "if one member suffer all the members suffer with it"—that all work together for the good of the whole. In earlier times the *good* which was thus indicated was associated in men's minds with human welfare exclusively. But it was eventually seen that Nature has no less consideration for the welfare of those of her products which to us seem hideous or mischievous, than for those which we regard as most useful to man or most deserving of his admiration. It thus became apparent that the good in question could not be human exclusively, but as regards each animal *its own good*—and that in the organised world the existence and life of every species is brought into subordination to one purpose—its own success in the struggle for existence.*

From what has preceded it may be readily understood that in Physiology, Adaptation takes a more prominent part than Evolution or Descent. In the prescientific period adaptation was everything. The observation that any structure or arrangement exhibited marks of adaptation to a useful purpose was accepted not merely as a guide in research, but as a full and final explanation. Of an organism or organ which perfectly fulfilled in its structure and working the end of its existence, nothing further required to be said or known. Physiologists of the present day recognise as fully as their predecessors that perfection of contrivance which displays itself in all living structures, the more exquisitely the more minutely they are examined. No one, for example, has written more emphatically on this point than did Ludwig. In one of his discourses, after showing how Nature exceeds the highest standard of human attainment—how she fashions as it were out of nothing and without tools, instruments of a perfection which the human artificer cannot reach, though provided with every suitable material—wood, brass, glass, india-rubber—he gives the organ of sight as a single example, referring among its other perfections to the rapidity with which the eye can be fixed on numerous objects in succession, and the instantaneous and unconscious estimates which we are able to form of the distances of objects, each estimate involving a process of arithmetic which no

* I am aware that in thus stating the relation between adaptation and the struggle for existence, I may seem to be reversing the order followed by Mr. Darwin, inasmuch as he regarded the survival of organisms which are fittest for their place in Nature, and of parts which are fittest for their place in the organism, as the agency by which adaptedness is brought about. However this may be expressed, it cannot be doubted that fitness is an essential property of organisms. Living beings are the only things in Nature which by virtue of evolution and descent are able to adapt themselves to their surroundings. It is therefore only so far as *organism* (with all its attributes) is presupposed, that the dependence of adaptation on survival is intelligible.

calculating machine would effect in the time.* In another discourse—that given at Leipzig when he entered on his professorship in 1865, he remarks that when in our researches into the inner mechanism of an organ we at last come to understand it, we are humbled by the recognition "that the human inventor is but a bungler compared with the unknown Master of the animal creation."†

Some readers will perhaps remember how one of the most brilliant of philosophical writers, in a discourse to the British Association delivered a quarter of a century ago, averred on the authority of a great Physiologist that the eye, regarded as an optical instrument, was so inferior a production that if it were the work of a mechanician it would be unsaleable. Without criticising or endeavouring to explain this paradox, I may refer to it as having given the countenance of a distinguished name to a misconception which I know exists in the minds of many persons, to the effect that the scientific Physiologist is more or less blind to the evidence of design in creation. On the contrary, the view taken by Ludwig, as expressed in the words I have quoted, is that of all Physiologists. The disuse of the teleological expressions which were formerly current does not imply that the indications of contrivance are less appreciated, for, on the contrary, we regard them as more characteristic of organism as it presents itself to our observation than any other of its endowments. But, if I may be permitted to repeat what has been already said, we use the evidences of adaptation differently. We found no explanation on this or any other biological principle, but refer all the phenomena by which these manifest themselves, to the simpler and more certain Physical Laws of the Universe.

Why must we take this position? First, because it is a general rule in investigations of all kinds, to explain the more complex by the more simple. The material Universe is manifestly divided into two parts, the living and the non-living. We may, if we like, take the living as our Norma, and say to the Physicists, you must come to us for Laws, you must account for the play of energies in universal nature by referring them to Evolution, Descent, Adaptation. Or we may take these words as true expressions of the mutual relations between the phenomena and processes peculiar to living beings, using for the explanation of the processes themselves the same methods which we should employ if we were engaged in the investigation of analogous processes going on independently of life. Between these two courses there seems to me to be no third alternative, unless we

* I summarise here from a very interesting lecture entitled "Leid und Freude in der Naturforschung" published in the 'Gartenlaube' (Nos. 22 and 23) in 1870.

† The sentence, of which the words in inverted commas form a part, is as follows: "Wenn uns endlich die Palme gereicht wird, wenn wir ein Organ in seinem Zusammenhang begreifen, so wird unser stolzes Gattungsbewusstsein durch die Erkenntniss niedergedrückt, dass der menschlicher Erfinder ein Stümper gegen den unbekannten Meister der thierischen Schöpfung sei."

suppose that there are two material Universes, one to which the material of our bodies belongs, the other comprising everything that is not either plant or animal.

The second reason is a practical one. We should have to go back to the time which I have ventured to call prescientific, when the world of life and organisation was supposed to be governed exclusively by its own Laws. The work of the past fifty years has been done on the opposite principle, and has brought light and clearness where there was before obscurity and confusion. All this progress we should have to repudiate, but this would not be all. We should have to forego the prospect of future advance. Whereas by holding on our present course, gradually proceeding from the more simple to the more complex, from the physical to the vital, we may confidently look forward to extending our knowledge considerably beyond its present limits.

A no less brilliant writer than the one already referred to, who is also no longer with us, asserted that mind was a secretion of the brain in the same sense that bile is a secretion of the liver, or urine that of the kidney; and many people have imagined this to be the necessary outcome of a too mechanical way of looking at vital phenomena, and that Physiologists, by a habit of adhering strictly to their own method, have failed to see that the organism presents problems to which this method is not applicable, such e.g. as the origin of the organism itself, or the origin and development in it of the mental faculty. The answer to this suggestion is that these questions are approached by Physiologists only in so far as they are approachable. We are well aware that our business is with the unknown knowable, not with the transcendental.

During the last twenty years there has been a considerable forward movement in Physiology in the psychological direction, partly dependent on discoveries as to the localisation of the higher functions of the nervous system, partly on the application of methods of measurement to the concomitant phenomena of psychical processes. And these researches have brought us to the very edge of a region which cannot be explored by our methods—where measurements of time or of space are no longer possible. In approaching this limit, the Physiologist is liable to fall into two mistakes—on the one hand that of passing into the transcendental without knowing it; on the other, that of assuming that what he does not know is not knowledge. The former of these risks seems to me of little moment; first, because the limits of natural knowledge in the psychological direction have been well defined by the best writers, as e.g. by du Bois-Reymond in his well-known essay "On the Limits of Natural Knowledge," * but chiefly because the investigator who knows what he is about is arrested *in limine* by the impossibility of applying the experimental

* 'Ueber die Grenzen des Naturerkennens.' Reden, Leipzig, 1886.

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Ludwig as Investigator and Teacher

The biographical history of Ludwig's life—how early he began his investigation of the anatomy and function of the kidneys, how he became, less than fifty years ago, Extraordinary Professor at Marburg, in the small University of his native State, Hesse Cassel, how in 1849 he removed to Zürich as ordinary Professor and afterwards married; how he was six years later promoted to Vienna, has already been admirably related by Dr. Stirling.* In 1865, after twenty years of professional experience, but still in the prime of life and, as it seemed, with many years of activity still before him, he accepted the Chair of Physiology at Leipzig. His transition to that great University was by far the most important occurrence in his Life, for the liberality of the Saxon Government, and particularly the energetic support which he received from the enlightened Minister, v. Falkenstein, enabled him to accomplish for Physiology what had never before been attempted on an adequate scale. No sooner had he been appointed than he set himself to create what was then essential to the progress of the Science—a great Observatory, arranged not as a Museum, but much more like a physical and chemical Laboratory, provided with all that was needed for the application of exact methods of research to the investigation of the processes of Life. The idea which he had ever in view, and which he carried into effect during the last thirty years of his life with signal success, was to unite his life-work as an investigator with the highest kind of teaching. Even at Marburg and at Zürich he had begun to form a *School*; for already men nearly of his own age had rallied round him. Attracted in the first instance by his early discoveries, they were held by the force of his character, and became permanently associated with him in his work as his loyal friends and followers—in the highest sense his *scholars*. If, therefore, we speak of Ludwig as one of the greatest *teachers* of Science the world has seen, we have in mind his relation to the men who ranged themselves under his leadership in the building up of the Science of Physiology, without reference to his function as an ordinary academical teacher.

Of this relation we can best judge by the careful perusal of the numerous biographical memoirs which have appeared since his death,

* See 'Science Progress,' vol. iv. Nov. 1895.

more particularly those of Professor His * (Leipzig); of Professor Kronecker † (Bern), who was for many years his coadjutor in the Institute; of Professor v. Fick ‡ (Würzburg); of Professor v. Kries § (Freiburg); of Professor Mosso ‖ (Turin); of Professor Fano ¶ (Florence); of Professor Tigerstedt ** (Upsala); of Professor Stirling,†† in England. With the exception of Fick, whose relations with Ludwig were of an earlier date, and of his colleague in the Chair of Anatomy, all of these distinguished teachers were at one time workers in the Leipzig Institute. All testify their love and veneration for the master, and each contributes some striking touches to the picture of his character.

All Ludwig's investigations were carried out with his scholars. He possessed a wonderful faculty of setting each man to work at a problem suited to his talent and previous training, and this he carried into effect by associating him with himself in some research which he had either in progress or in view. During the early years of the Leipzig period, all the work done under his direction was published in the well-known volumes of the 'Arbeiten,' and subsequently in the 'Archiv für Anat. und Physiologie' of du Bois-Reymond. Each "Arbeit" of the laboratory appeared in print under the name of the scholar who co-operated with his master in its production, but the scholar's part in the work done varied according to its nature and his ability. Sometimes, as v. Kries says, he sat on the window-sill while Ludwig, with the efficient help of his laboratory assistant Salvenmoser, did the whole of the work. In all cases Ludwig not only formulated the problem, but indicated the course to be followed in each step of the investigation, calling the worker, of course, into counsel. In the final working up of the results he always took a principal part, and often wrote the whole paper. But whether he did little or much, he handed over the whole credit of the performance to his coadjutor. This method of publication has no doubt the disadvantage that it leaves it uncertain what part each had taken: but it is to be remembered that this drawback is unavoidable whenever master and scholar work together, and is outweighted by the many advantages which arise from this mode of co-operation. The instances in which any uncertainty can exist in

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* His, "Karl Ludwig und Karl Thiersch. Akademische Gedächtnissrede," Leipzig, 1895.

† Kronecker, "Carl Friedrich Wilhelm Ludwig." 'Berliner klin. Wochensch.' 1895, no. 21.

‡ A. Fick, "Karl Ludwig. Nachruf." 'Biographische Blätter,' Berlin, vol. i. pt. 3.

§ v. Kries, "Carl Ludwig." Freiburg i. B. 1895.

‖ Mosso, "Karl Ludwig." 'Die Nation,' Berlin, nos. 38, 39.

¶ Fano, "Per Carlo Ludwig Commemorazione." 'Clinica Moderna,' Florence, i. no. 7.

** Tigerstedt, "Karl Ludwig. Denkrede." 'Biographische Blätter,' Berlin, vol. i. pt. 3.

†† Stirling, loc. cit.

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before Ludwig's time, investigated the phenomena of life from the physical side, it was he and the contemporaries who were associated with him who first clearly recognised the importance of the principle that vital phenomena can *only be understood by comparison with their physical counterparts*, and foresaw that in this principle the future of Physiology was contained as in a nutshell. Feeling strongly the fruitlessness and unscientific character of the doctrines which were then current, they were eager to discover chemical and physical relations in the processes of life. In Ludwig's intellectual character this eagerness expressed his dominant motive. Notwithstanding that his own researches had in many instances proved that there are important functions and processes in the animal organism which have no physical or chemical analogues, he never swerved either from the principle or from the method founded upon it.

Although Ludwig was strongly influenced by the rapid progress which was being made in scientific discovery at the time that he entered on his career, he derived little from his immediate predecessors in his own science. He is sometimes placed among the pupils of the great comparative Anatomist and Physiologist, J. Muller. This, however, is a manifest mistake, for Ludwig did not visit Berlin until 1847, when Muller was nearly at the end of his career. At that time he had already published researches of the highest value (those on the Mechanism of the Circulation and on the Physiology of the Kidney), and had set forth the line in which he intended to direct his investigations. The only earlier Physiologist with whose work that of Ludwig can be said to be in real continuity was E. H. Weber, whom he succeeded at Leipzig, and strikingly resembled in his way of working. For Weber, Ludwig expressed his veneration more unreservedly than for any other man excepting perhaps Helmholtz, regarding his researches as the foundation on which he himself desired to build. Of his colleagues at Marburg he was indebted in the first place to the anatomist, Professor Ludwig Fick, in whose department he began his career as Prosector, and to whom he owed facilities without which he could not have carried out his earlier researches; and in an even higher degree to the great Chemist, R. W. Bunsen, from whom he derived that training in the exact sciences which was to be of such inestimable value to him afterwards.

There is reason, however, to believe, that, as so often happens, Ludwig's scientific progress was much more influenced by his contemporaries than by his seniors. In 1847, as we learn on the one hand from du Bois-Reymond, on the other from Ludwig himself, he visited Berlin for the first time. This visit was an important one both for himself and for the future of Science, for he there met three men of his own age, Helmholtz, du Bois-Reymond and Brücke, who were destined to become his life-friends, all of whom attained to the highest distinction, and one of whom is still living. They

[illegible]

[illegible] He regarded the faculty of vivid sensual realisation [illegible] as of special value to the investigator of natural phenomena, and did his best to cultivate it in those who worked with him in the laboratory. In himself, this objective tendency (if I may be permitted the use of a word which, if not correct, seems to express what I mean) might be regarded as almost a defect, for it made him indisposed to appreciate any sort of knowledge which deals with the abstract. He had a disinclination to philosophical speculation which almost amounted to aversion, and, perhaps for a similar reason, avoided the use of mathematical methods even in the discussion of scientific questions which admitted of being treated mathematically—contrasting in this respect with his friend du Bois-Reymond, resembling Brücke. But as a teacher the quality was of immense use to him. His power of vivid realisation was the *substratum* of that many-sidedness which made him, irrespectively of his scientific attainments, so attractive a personality.

I am not sure that it can be generally stated that a keen scientific observer is able to appreciate the artistic aspects of Nature. In Ludwig's case, however, there is reason to think that the æsthetic faculty was as developed as the power of scientific insight. He was a skilful draughtsman, but not a musician; both arts were however a source of enjoyment to him. He was a regular frequenter of the *Gewandhaus* concerts, and it was his greatest pleasure to bring together gifted musicians in his house, where he played the part of an intelligent and appreciative listener. Of painting he knew more than of music, and was a connoisseur whose opinion carried weight. It is related that he was so worried by what he considered bad art,

that after the redecoration of the *Gewandhaus* concert-room, he was for some time deprived of his accustomed pleasure in listening to music.

Ludwig's social characteristics can only be touched on here in so far as they serve to make intelligible his wonderful influence as a teacher. Many of his pupils at Leipzig have referred to the *schöne Gemeinsamkeit* which characterised the life there. The harmonious relation which, as a rule, subsisted between men of different education and different nationalities, could not have been maintained had not Ludwig possessed side by side with that inflexible earnestness which he showed in all matters of work or duty, a certain youthfulness of disposition which made it possible for men much younger than himself to accept his friendship. This sympathetic geniality was, however, not the only or chief reason why Ludwig's pupils were the better for having known him. There were not a few of them who for the first time in their lives came into personal relation with a man who was utterly free from selfish aims and vain ambitions, who was scrupulously conscientious in all that he said and did, who was what he seemed, and seemed what he was, and who had no other aim than the advancement of his science, and in that advancement saw no other end than the increase of human happiness. These qualities displayed themselves in Ludwig's daily active life in the laboratory, where he was to be found whenever work of special interest was going on; but still more when, as happened on Sunday mornings, he was "at home" in the library of the Institute—the corner room in which he ordinarily worked. Many of his "scholars" have put on record their recollections of these occasions; the cordiality of the master's welcome, the wide range and varied interest of his conversation, and the ready appreciation with which he seized on anything that was new or original in the suggestions of those present. Few men live as he did, "*im Ganzen, Guten, Schönen*," and of those still fewer know how to communicate out of their fulness to others.

The Old and the New Vitalism.

Since the middle of the century the progress of Physiology has been continuous. Each year has had its record, and has brought with it new accessions to knowledge. In one respect the rate of progress was more rapid at first than it is now, for in an unexplored country discovery is relatively easy. In another sense it was slower, for there are now scores of investigators for every one that could be counted in 1840 or 1850. Until recently there has been throughout this period no tendency to revert to the old methods—no new departure—no divergence from the principles which Ludwig did so much to enforce and exemplify.

The wonderful revolution which the appearance of the 'Origin of Species' produced in the other branch of Biology, promoted the

progress of Physiology by the new interest which it gave to the study, not only of structure and development, but of all other vital phenomena. It did not, however, in any sensible degree affect our *method* or alter the direction in which Physiologists had been working for two decades. Its most obvious effect was to sever the two subjects from each other. To the Darwinian epoch comparative Anatomy and Physiology were united, but as the new Ontology grew it became evident that each had its own problems and its own methods of dealing with them.

The old vitalism of the first half of the century is easily explained. It was generally believed that, on the whole, things went on in the living body as they do outside of it; but when a difficulty arose in so explaining them the Physiologist was ready at once to call in the aid of a "*vital force*." It must not, however, be forgotten that, as I have already indicated, there were great teachers (such, for example, as Sharpey and Allen Thomson in England, Magendie in France, Weber in Germany) who discarded all vitalistic theories, and concerned themselves only with the study of the time- and place-relations of phenomena; men who were before their time in insight, and were only hindered in their application of chemical and physical principles to the interpretation of the processes of life by the circumstance that chemical and physical knowledge was in itself too little advanced. Comparison was impossible, for the standards were not forthcoming.

Vitalism in its original form gave way to the rapid advance of knowledge as to the correlation of the physical sciences, which took place in the forties. Of the many writers and thinkers who contributed to that result, J. R. Mayer and Helmholtz did so most directly, for the contribution of the former to the establishment of the Doctrine of the Conservation of Energy had physiological considerations for its point of departure; and Helmholtz, at the time he wrote the "*Erhaltung der Kraft*," was still a Physiologist. Consequently when Ludwig's celebrated *Lehrbuch* came out in 1852,—the book which gave the *coup de grâce* to vitalism in the old sense of the word,—his method of setting forth the relations of vital phenomena by comparison with their physical or chemical counterparts, and his assertion that it was the task of Physiology to make out their necessary dependence on elementary conditions, although in violent contrast with current doctrine, were in no way surprising to those who were acquainted with the then recent progress of research. Ludwig's teaching was indeed no more than a general application of principles which had already been applied in particular instances.

The proof of the non-existence of a special "vital force" lies in the demonstration of the adequacy of the known sources of energy in the organism to account for the actual day by day expenditure of heat and work—in other words, on the possibility of setting forth an energy balance sheet, in which the quantity of food which enters the

body in a given period (hour or day) is balanced by an exactly corresponding amount of heat produced or external work done. It is interesting to remember that the work necessary for preparing such a balance sheet (which Mayer had attempted but, from want of sufficient data, failed in) was begun thirty years ago in the laboratory of the Royal Institution by the present Foreign Secretary of the Royal Society. But the determinations made by Dr. Frankland related to one side of the balance sheet, that of income. By his researches in 1866 he gave Physiologists for the first time reliable information as to the heat value (i.e. the amount of heat yielded by the combustion) of different constituents of food. It still remained to apply methods of exact measurement to the expenditure side of the account. Helmholtz had estimated this, as regards man, as best he might; but the technical difficulties of measuring the expenditure of heat of the animal body appeared until lately to be almost insuperable. Now that it has been at last successfully accomplished, we have the experimental proof that in the process of life there is no production or disappearance of energy. It may be said that it was unnecessary to prove what no scientifically sane man doubted. There are, however, reasons why it is of importance to have objective evidence that food is the sole and adequate source of the energy which we day by day or hour by hour disengage, whether in the form of heat or external work.

In the opening paragraph of this section it was observed that *until recently* there had been no tendency to revive the vitalistic notion of two generations ago. In introducing the words in italics I referred to the existence at the present time in Germany of a sort of reaction, which under the term "Neovitalismus" has attracted some attention—so much indeed that at the *Versammlung Deutscher Naturforscher* at Lübeck last September, it was the subject of one of the general addresses. The author of this address (Prof. Rindfleisch) was, I believe, the inventor of the word, but the origin of the movement is usually traced to a work on Physiological Chemistry which an excellent translation by the late Dr. Wooldridge has made familiar to English students. The author of this work owes it to the language he employs in the introduction on "Mechanism and Vitalism," if his position has been misunderstood, for in that introduction he distinctly ranges himself on the vitalistic side. As, however, his vitalism is of such a kind as not to influence his method of dealing with actual problems, it is only in so far of consequence as it may affect the reader. For my own part I feel grateful to Professor Bunge for having produced an interesting and readable book on a dry subject, even though that interest may be partly due to the introduction into the discussion, of a question which, as he presents it, is more speculative than scientific.

As regards other physiological writers to whom vitalistic tendencies have been attributed, it is to be observed that none of them have even suggested that the doctrine of a "vital force" in its old sense

should be revived. Their contention amounts to little more than this, that in certain recent instances improved methods of research appear to have shown that processes, at first regarded as entirely physical or chemical, do not conform so precisely as they were expected to do to chemical and physical laws. As these instances are all essentially analogous, reference to one will serve to explain the bearing of the rest.

Those who have any acquaintance with the structure of the animal body will know that there exists in the higher animals, in addition to the system of veins by which the blood is brought back from all parts to the heart, another less considerable system of branched tubes, the lymphatics, by which, if one may so express it, the leakage of the blood-vessels is collected. Now, without inquiring into the *why* of this system, Ludwig and his pupils made and continued for many years elaborate investigations which were for long the chief sources of our knowledge, their general result being that the efficient cause of the movement of the lymph, like that of the blood, was mechanical. At the Berlin Congress in 1890 new observations by Professor Heidenhain of Breslau made it appear that under certain conditions the process of lymph formation does not go on in strict accordance with the physical laws by which leakage through membranes is regulated; the experimental results being of so unequivocal a kind that, even had they not been confirmed, they must have been received without hesitation. How is such a case as this to be met? The "Neovitalists" answer promptly by reminding us that there are cells, i.e. living individuals, placed at the inlets of the system of drainage without which it would not work, that these let in less or more liquid according to circumstances, and that in doing so they act in obedience, not to physical laws, but to vital ones—to laws which are special to themselves.

Now, it is perfectly true that living cells, like working bees, are both the architects of the hive and the sources of its activity; but if we ask how honey is made, it is no answer to say that the bees make it. We do not require to be told that cells have to do with the making of lymph, as with every process in the animal organism; but what we want to know is *how* they work, and to this we shall never get an answer so long as we content ourselves with merely explaining one unknown thing by another. The action of cells must be explained, if at all, by the same method of comparison with physical or chemical analogues that we employ in the investigation of organs.

Since 1890 the problem of lymph formation has been attacked by a number of able workers—among others in London, by Dr. Starling of Guy's Hospital, who, by sedulously studying the conditions under which the discrepancies between the actual and the expected have arisen, has succeeded in untying several knots. In reference to the whole subject, it is to be noticed that the process by which difficulties are brought into view is the same as that by which they are

eliminated. It is one and the same method throughout, by which, step by step, knowledge perfects itself—at one time by discovering errors, at another by correcting them; and if at certain stages in this progress difficulties seem insuperable, we can gain nothing by calling in, even provisionally, the aid of any sort of *Eidolon*, whether "cell," "protoplasm," or internal principle.

It thus appears to be doubtful whether any of the biological writers who have recently professed vitalistic tendencies are in reality vitalists. The only exception that I know is to be found in the writings of a well-known worker, Hans Driesch,* who has been led by his researches on what is now called the Mechanics of Evolution, to revert to the fundamental conception of vitalism, that the laws which govern vital processes are not physical, but biological—that is, peculiar to the living organism, and limited thereto in their operation. Driesch's researches as to the modifications which can be produced by mechanical interference in the early stages of the process of ontogenesis have enforced upon him considerations which he evidently regards as new, though they are familiar enough to Physiologists. He recognises that although by the observation of the successive stages in the ontogenetic process, one may arrive at a perfect knowledge of the relation of these stages to each other, this leaves the efficient causes of the development unexplained (*führt nicht zu einem Erkenntniss ihrer bewirkenden Ursachen*)—it does not teach us why one form springs out of another. This brings him at once face to face with a momentous question. He has to encounter three possibilities—he may either join the camp of the biological agnostics and say with du Bois-Reymond, not only "*ignoramus*" but "*ignorabimus*"; *or* he content to work on in the hope that the physical laws that underlie and explain organic Evolution may sooner or later be discovered; *or* he may seek for some hitherto hidden Law of Organism, of which the known facts of Ontogenesis are the expression, and which, if accepted as a Law of Nature, would explain everything. Of the three alternatives Driesch prefers the last, which is equivalent to declaring himself an out-and-out vitalist. He trusts by means of his experimental investigations of the Mechanics of Evolution to arrive at "elementary conceptions" on which by "mathematical deduction"† a complete theory of Evolution may be founded.

If this anticipation could be realised, if we could mentally construct with the aid of these new *Principia* the ontogeny of a single living being, the question whether such a result was or was not incon-

* Driesch, 'Entwicklungsmechanische Studien': a Series of ten Papers, of which the first six have appeared in the 'Zeitsch. f. w. Zoologie,' vols. liii. and lv. the rest in the 'Mittheilungen' of the Naples Station.

† "Elementarvorstellungen . . . . die zwar mathematische Deduktion aller Erscheinungen wie sich gestatten möchten." Driesch, "Beiträge zur theoretischen Morphologie." 'Biol. Centralblatt,' vol. xii. p. 539, 1892.

sistent with the uniformity of Nature, would sink into insignificance as compared with the splendour of such a discovery.

But will such a discovery ever be made? It seems to me even more improbable than that of a physical theory of organic evolution. In the meantime it is satisfactory to reflect that the opinion we may be led to entertain on this theoretical question need not affect our estimate of the value of Driesch's fruitful experimental researches.

[J. B. S.]

WEEKLY EVENING MEETING,

Friday, January 31, 1896.

Sir Benjamin Baker, K.C.M.G. LL.D. F.R.S. Manager, in the Chair.

Sidney Lee, Esq. the Editor of the 'Dictionary of National Biography.'

National Biography.

(Abstract.)

Mr. Sidney Lee pointed out that pride in the achievement of one's ancestors is almost as widely distributed a characteristic of mankind as the power of speech. In China, the national religion centres round a worship of progenitors to very remote degrees, and Western nations exhibit the same instinctive desire to do honour to the memories of those who, by character and exploits, have distinguished themselves from the mass of their countrymen. But no memorial can be national and efficient, unless it be at once permanent, public and perspicuous. It should take such a shape as to leave no doubt in the mind of posterity what was the nature of the achievement or characteristics that generated in the nation the desire of commemoration. Monuments in stone or brass preserve bare names, and are not lasting. "The safest way," wrote Thomas Fuller, "to secure a memory from oblivion is by committing the same to writing." The rarity of poetic memorials like Shelley's 'Adonais' or 'The Burial of Sir John Moore,' which are at once permanent, public and perspicuous, compels recourse to the more adaptable machinery of biography. But biography, as it is ordinarily practised, works fitfully and capriciously. If biography is to respond to a whole nation's commemorative aspirations, its bounds must be enlarged and defined, so as to admit with unerring precision every one who has excited the nation's commemorative instinct, while the mode of treatment must be so contrived, so contracted, that the collected results may not overwhelm us by their bulk. Biography working with these aims and on these lines may justly be called national biography. Carlyle's definition of the function of history—"to find out great men, clean the dirt from them and place them on their proper pedestals"—more properly defines the function of national biography. The aims of the historian and biographer are quite distinct. The historian deals with aggregate movements of men, with political events and institutions, with the evolution of society; he looks at mankind through a field-glass; his

purpose is often served if he catch a glimpse, or no glimpse at all, of personages who command the biographer's most earnest attention. The historian barely mentions men like Dr. Johnson, Benvenuto Cellini, Lord Herbert of Cherbury, or Samuel Pepys. The biographer, on the other hand, puts individual men under a magnifying glass and submits them to minute examination; professionally he cares little or nothing for the evolution of society. But while the historian and biographer seek different goals, they can render one another very genuine service on the road. The biographer requires an intelligent knowledge of the historical environment, if he would portray in fitting perspective all the operations of his unit: but his art is to sternly subordinate his scenery to his actors, and never to crowd his stage with upholstery and scenic apparatus that can only distract the spectators' attention from the proper interest of the piece. The historian's debt to the biographer is even greater than the biographer's to the historian. The biographer has to explore many a dismal swamp in which the historian is not called upon to set foot. Parish registers, academic archives, family letters, unprinted memoranda, county histories, genealogical dissertations and pedigrees, are leading features of the country in which the biographer passes his days. But such material may secrete an important historical fact, or throw a welcome light on an obscure step in an historic movement. Macaulay made frequent appeals to biography with excellent effect, but Mr. Froude neglected it. His picture of Queen Mary of England, as a hag-like bigot, might easily have been rectified by an occasional resort to pedestrian biographical sources. Nor will the lack of accessible biography long constitute a sufficient excuse for the historian's neglect of biographic sources. The historian will soon have at his command a completed register of national biography.

The Method of National Biography.—National biography seeks, as Priestley said of science, "to comprise as much knowledge as possible in the smallest compass." Conciseness carried to the furthest limits consistent with the due performance of his commemorative function, is the first law of the national biographer's being. No place can be accorded to rhetoric, voluble enthusiasm, emotion, or loquacious sentiment. The writings of authors, the works of painters or engravers, must be cast into the unexhilarating form of chronological series or catalogues, and the result must be rather like a map or plan than a picture. The result need not necessarily be devoid of literary art, and should give the reader the feeling—one as pleasing as any that art can give—that to him has been imparted all the information for which his commemorative instinct craves. The national biographer must nerve himself to omit much detail, much anecdote that may find a lawful place in individual biography. It is solely in the few careers which exhibit unusual spiritual tendencies or conspicuous deflections from the ordinary standard of morality, that any reference to a man's moral or spiritual experience is justifiable. Such lapses as the marital adventures of Byron,

Nelson or Parnell, Coleridge's indulgence in opium, Porson's indulgence in drink, which vitally affected their careers, must be frankly but judiciously and briefly described. Here, as at every point in his work, the national biographer has to cultivate the judicial temper, for he has not merely to record reputations but to adjust them. He must not exalt Cromwell at Charles I.'s expense, nor Charles I. at Cromwell's. Careers embittered by controversy must be treated with due regard to all the interests involved. Many of these methods of national biography might be adopted without disadvantage by the individual biographer, who is often no expert in the biographic art; no limit is set to his diffuseness, to his indulgence in trivial details, to his partisan tendencies; with the result that the hero's really eminent achievements and distinctive characteristics lie buried under the dust and ashes of special pleading, commonplace gossip or helpless eulogy. The national biographer aims at commemorating all who have excited the commemorative instinct in any appreciable degree in any department of national life; but it is difficult to enunciate any principle of exclusion that shall carry universal conviction. An Aristotelian definition may apply; and it may be suggested that no man's life should be admitted that does not present at least one action that is "serious, complete and of a certain magnitude." Official dignities, except of the rarest and most dignified kind, give in themselves no claim to national commemoration. But national biography must satisfy the commemorative instinct of all sections of the population, and include representatives of varied political or religious movements. The national biographer must, at times, too, correct the working of the nation's commemorative instinct, by noticing those who, having prepared the way for great inventions, have been forgotten, while all the glory has gone to those who have reaped the benefit of preceding efforts. It is obvious that of the aggregate mass of mankind very few are taken. The lecturer's personal experience led him to estimate that from the year 1000 A. D. to the end of the present century, 30,000 persons have achieved in this Kingdom such measure of distinction as to claim the national biographer's attention; i.e. 1 in 5000 of the adult population. Up to the end of the seventeenth century the ratio for adults seems to have been 1 in 6250. Last century it rose almost imperceptibly, viz. to 1 in 6000. In this century, when we include the English speaking inhabitants of our colonies, but exclude the United States, the ratio sensibly rises, viz. to one in 4000, and at the present moment 600 adults in the County of London are qualifying for admission to a complete register of national biography, of whom twenty should be women. The increase of the ratio of distinction in the present century is largely due to the multiplication of intellectual callings, the specialisation of science and art, and the improvement of educational machinery.

Experiments in National Biography.—In conclusion the lecturer briefly described the efforts previously made in this country in

the direction of national biography. After alluding to mediæval collections of lives of saints, popes, kings and others, he reviewed the development of biography during the sixteenth and seventeenth centuries, beginning with Leland, Bale, Pits and Foxe. These collective biographers were religious partisans whose theological prejudices had to be counteracted before national biography could enjoy an adequate measure of impartiality. Later on, biographers like the Scotsmen Dempster and Mackenzie, betrayed an excessive patriotism or racial bias which overruled all other considerations with equally disastrous results. A great advance was seen during the seventeenth century in Naunton's 'Fragmenta Regalia,' Holland's 'Heroologia,' Aubrey's 'Lives,' but above all in Wood's 'Athenæ Oxonienses' and Fuller's 'Worthies of England.' In the eighteenth century the encyclopædic movement gave rise to a genuine attempt at national biography in the work called 'Biographia Britannica.' The first volume appeared in 1747, the seventh and last in 1763. The scheme had grave defects, but they should be treated with the merciful consideration to which the shortcomings of all pioneers are entitled. Moreover, unlike some of its successors, the 'Biographia Britannica' achieved the distinction of reaching the letter *Z.* Eleven years later Dr. Johnson was invited to prepare a second edition. But Dr. Johnson had had one experience in dictionary making and he not unnaturally declined to have a second. The task was undertaken by another (Dr. Kippis), and in 1793 there appeared the fifth and last volume of the second edition of the 'Biographia Britannica.' But though the work had reached its last volume, its final pages had only arrived at the beginning of the letter F. At the article on Sir Thomas Fastolf this undertaking stopped, to remain for ever a magnificent fragment, a melancholy wreck, a fearful example.

> "Checks and disasters
> Grow in the veins of actions highest reared."

Some twenty-one years later. Alexander Chalmers completed in thirty-two volumes his very respectable 'Biographical Dictionary.' Some thirty years later, the Society for the Diffusion of Useful Knowledge, under a committee, of which Lord Brougham was chairman, and Lord Spencer (father of the present earl) was vice-chairman, designed a dictionary of biography which was to combine national with universal biography, on an ambitious scale. But the letter A was only completed in seven volumes, and it is, therefore, not surprising to learn that that venture went no further. A very modest attempt in the same direction followed, in Rose's 'Biographical Dictionary,' but here the first three letters of the alphabet absorbed six volumes, and the remaining twenty-three letters were compressed into another six. There followed a pause in the efforts of collective biography in this country. After the middle of the century, Germany

Austria and Belgium each set on foot a register of national biography under the auspices of state-aided literary academies. At length, a new and very strenuous endeavour was made to supply the defect in our own literature, made under the auspices of no state-aided literary academies, but by the independent and enlightened exertion of one great English publisher. In conclusion the lecturer said: "It does not become me to say much of this last endeavour, with which I am very closely identified. The 'Dictionary of National Biography,' which was begun some thirteen years ago under Mr. Leslie Stephen's editorship, is now nearing completion under my own. Even if the 'Dictionary of National Biography' does not practise at all points those counsels of perfection which I have addressed to you to-night, if it contains errors from which no work of such multiplicity was ever free; yet those who are acquainted with it will admit that it has accomplished much, that the writers who have co-operated in its production have vastly improved upon their predecessors, and finally that it is none the less efficient, and none the less worthy of its mighty theme, because, while it seeks to do the State some service, it is the outcome of private enterprise, and the handiwork of private citizens."

GENERAL MONTHLY MEETING,

Monday, February 3, 1896.

SIR JAMES CRICHTON-BROWNE, M.D. LL.D. F.R.S. Treasurer and Vice-President, in the Chair.

Mrs. Montagu,
Robert R. Tatlock, Esq. F.C.S. F.I.C.
Ernest Westlake, Esq.

were elected Members of the Royal Institution.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Governor-General of India—Geological Survey of India: Records, Vol. XXVIII. Part 4. 8vo. 1895.

The Lords of the Admiralty—Nautical Almanac for 1899. 8vo.

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Hourly Means for 1891. 4to. 1895.

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Agricultural Society of England, Royal—Journal, Vol. VI. Part 4. 8vo. 1895.

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Aristotelian Society—Proceedings, Vol. III. No. 1. 8vo. 1895.

Asiatic Society of Bengal—Proceedings, 1895, Nos. 7, 8. 8vo.

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Asiatic Society of Great Britain, Royal—Journal for Jan. 1896. 8vo.

Astronomical Society, Royal—Memoirs, Vol. LI. 1892-95. 8vo.

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Dawson, George, Esq. (the Author)—Glacial Deposits of South Western Alberta in the vicinity of the Rocky Mountains. 8vo. 1895.
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Athenæum for Dec. 1895 and Jan. 1896. 4to.
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Monist for Jan. 1896. 8vo.
Nature for Dec. 1895 and Jan. 1896. 4to.
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WEEKLY EVENING MEETING,

Friday, February 7, 1896.

Basil Woodd Smith, Esq. F.R.A.S. F.S.A. Vice-President, in the Chair.

The Hon. John Collier.

Portrait Painting in its Historical Aspects.

(Abstract.)

The lecturer began with the consideration of portraiture in classical times.

Although no direct evidence was obtainable until the late and altogether debased portraits found in the Fayoum, yet from indirect evidence we might gather that portraiture amongst the Greeks and Romans was a very dignified and charming art, probably a little tame and lacking in character, but at its best more full of beauty than it has ever been since.

The likenesses of the dead found in the Græco-Roman cemetery of the Fayoum were then discussed. It was pointed out how strangely they resembled the art of another very debased period—the early Victorian.

Portraiture was then shown to have sunk under the burden of an increasing formalism, until in the early middle ages it had practically ceased to exist.

It first reappeared when Italian painting was brought back to life by the genius of Giotto. Reference was made to his great fresco of Paradise, in the lower portion of which is a likeness of Dante walking in procession with his fellow citizens.

The next decided advance was ascribed to Masaccio, the forerunner of the great fifteenth century masters, who were all in the habit of introducing portraits of their friends into their subject pictures.

But the modern practice of having separate portraits of individuals, was shown to have sprung up with the great painters of the Renaissance—who also were the first to utilise the full resources of light and shade, by which the vigour of portraiture was so much enhanced. It also owed a great deal to the introduction of oil painting and the consequent spread of easel pictures.

After alluding to the art of Leonardo and of Raphael, the lecturer referred to Titian as *the* great portrait painter of the Renaissance. He considered that Titian was, on the whole, the greatest painter who had ever lived, but not quite the greatest portrait painter. Both Rembrandt and Velasquez gave more vitality to their likenesses, but

in the rendering of human beauty and dignity Titian surpassed them both.

Titian's female portraits were apt to be stiff; in proof of this his likeness of Catarina Cornaro was thrown on the screen, and it was shown how oppressed the sitter seemed by the over-gorgeousness of her clothes. This tyranny of clothes was said to have hampered the female portraits of all the old masters.

Then Moroni was referred to as the first example of the specialised portrait painter, i.e. one who painted very little else than portraits.

The early Flemish school was then considered as exemplified by the Van Eycks.

It was pointed out how lacking they were in the feeling for beauty which so distinguished the Italian school.

The lecturer then went on to Holbein and the German school.

Holbein was pronounced hard and dry in painting, but so supreme in draughtsmanship that he gave more of the intimate character of his sitter than any other painter.

The lecturer considered that the Dutch school of portraiture was, as a school, the greatest of all. At the head of it stood Rembrandt, but it included a great number of other admirable portrait painters.

As a painter, Franz Hals was pronounced over-rated. His flesh painting was poor, but his gift of animated draughtsmanship could hardly be exceeded.

Van der Helst's great picture of the 'Banquet of the Civic Guard' was thrown on to the screen, and referred to as a supreme example of patient skill.

Rembrandt was bracketed with Velasquez as one of the two greatest portrait painters who have ever lived.

His 'Syndics of the Cloth-workers' Guild' was shown, and was pronounced the finest example known of a simple portrait group.

Then the lecturer discussed Rembrandt's only rival in his own line—Velasquez.

There was no great Spanish school of portraiture. Velasquez stood practically alone. In some respects he was even greater than Rembrandt. Although a master of chiaroscuro he did not play tricks with it as Rembrandt did, and his colouring was less artificial. On the other hand, his portraits were sometimes stiff, which Rembrandt's never were.

The celebrated picture of the 'Surrender of Breda' was shown and discussed. It was said to be something between a portrait piece and a historical painting, and to be of the very highest excellence in either aspect.

The lecturer then returned to the Flemish School as represented by Vandyke—a man of great talent, but who had an unfortunate influence on art. His extravagance led him to turn his studio into a sort of manufactory, in which by the aid of assistants he turned out a great number of mannered and superficial portraits. This mannerism was reproduced with great facility by Sir Joshua Reynolds

who, with Gainsborough and Romney, established for the first time a purely English school of portraiture. The different characteristics of these three men of genius were then discussed.

They were all three pre-eminently successful with women. In their hands, for the first time since the classical epoch, had female portraiture completely freed itself from the tyranny of stiff clothes and stiff attitudes. For female charm and grace their works were quite unrivalled. The male portraits were pronounced less satisfactory. There was an imperfect rendering of form and a general lack of vigorous drawing. The hands especially were very poor. These three painters were all very prolific, and although their finest works were in many ways admirable, their average productions were very slight and very much scamped.

The lecturer summed up his complaint against these men of genius by saying that they allowed their feeling for grace and charm to overcome their love of truth. There was a great lack of sincerity in these courtly painters, and for the highest form of portrait painting sincerity was absolutely essential.

This was the last of the great epochs of portrait painting—Sir Thomas Lawrence, a man of great ability but of false ideals, started a decadence that reached its lowest depths in the early Victorian era. The lecturer preferred not to discuss the burning subject of modern painting. He merely remarked on the excessive love of novelty and of eccentricity that characterised it. He ended up by maintaining, in the teeth of modern art theories, that it was better for a portrait to resemble the person it was meant for, or that if this was too much to expect, that it should at least resemble a human being.

[J. C.]

WEEKLY EVENING MEETING,

Friday, February 14, 1896.

SIR JAMES CRICHTON-BROWNE, M.D. LL.D. F.R.S. Treasurer and Vice-President, in the Chair.

J. J. ARMISTEAD, Esq. Member of the Royal Commission on Tweed and Solway Fisheries.

Fish Culture.

I NEED hardly for a moment dwell upon the importance of the subject upon which I am about to address you this evening. Fish culture has made very rapid strides during the last few years, and its progress and success have given those who are engaged in it opportunities of becoming much more intimately acquainted with some of its advantages, and also with the proper use of the great motive power which has been placed in the hands of man by an all-wise Creator.

Although a knowledge of fish culture seems to have been lost for a long period, yet there is every evidence that it was well known to the ancients. The Chinese at the present day are well acquainted with fish culture, and have been so from time immemorial. They have curious methods of placing bundles of sticks and mats in the rivers, on which the fish deposit their ova, which afterwards become a marketable commodity. There is no doubt whatever that fish culture was well known to the ancient Greeks, and Romans also, but, as their knowledge has not been handed down to the present time, it might as well, so far as we are concerned, have never existed. It is said of Lucullus, that at Tusculum he caused canals to be dug between his fish ponds and the sea, so that when the fish came up from the sea to deposit their eggs in the fresh water, he was enabled to intercept them by placing gratings in these canals, and while their posterity were growing the fish themselves furnished the market. That fish were held in high esteem in the olden time is very evident. They were patronised by the Cæsars. Augustus had a fish engraved on his signet ring, and they appeared upon coins not only during his time but long afterwards, and the coins of Greece were similarly embellished. Towns, islands, ships and taverns were named after them, and from the same source ancient literature is said to have derived some of its prettiest similes, myths and fables. They were also sacrificed to the various deities. But, notwithstanding this, the ancients seem to have set far greater store upon fish as articles of food in most cases, than as objects of worship. "We remember the fish which we did eat in Egypt," was the cry of the Israelites aft-

the Exodus, from which one would infer that they might have preferred fish to freedom.

Coming down to later times, fish culture, or rather the secret of fertilising ova by artificial means, was discovered by a German naturalist, Count Von Golstein, about the year 1758. It also became known to another German naturalist, one Jacobi, a short time afterwards, about the year 1761, and strange to say he not only succeeded in fertilising eggs, but he fertilised the eggs which he took from a dead fish. However, notwithstanding this, no practical use seems to have been made of the knowledge which was obtained till nearly a century afterwards, down so lately as the year 1841, when it fell to the lot of two French peasants to discover the fact that trout ova could be fertilised artificially, and that they could be hatched. These men could never have heard of the scientists who were acquainted with the scientific experiment which had been discovered so long before, but they found from studying the habits of the fish in their native streams that the eggs were deposited in the gravel; and, following out nature's plan, these men collected a quantity of gravel from the stream bottom and fertilised the eggs, and placed them among gravel, and placed this in a perforated tin or zinc vessel, something in shape like a cheese, and put this at the bottom of the stream where the current would percolate through the holes and so keep up a continual supply of water. In due course of time the eggs hatched. But for a long time the thing went no further. People supposed that the gravelly bed of the stream was an absolute necessity for the hatching of the ova of trout. At last, however, the matter was taken up by the Société d'Acclimatation de Paris, and Professor Coste conceived the idea that eggs could not only be fertilised, but could be incubated and hatched, and the little fish reared to maturity, apart from the natural streams, and he proved his assertion by hatching some salmon in a tub. He got a large tub and in it he placed a number of boxes in such a position that the water flowed from one to the other round the tub. In these boxes he placed his ova, and in due course of time they hatched and produced fish. This was about the year 1850. Then I come down to a later time in the history of fish culture, and one which I cannot but remember with feelings partly of regret at the fact that the operator is no longer with us. I refer to the late lamented Frank Buckland, who some thirty-three years ago stood upon the platform which I have the honour to occupy to-night. Buckland said of fish culture that it promised "to be eventually the origin of increase of revenue to private individuals, a source of national wealth, and certainly a great boon to the public in general." This was thirty years ago, and how do we stand to-day? The first part of that prophecy has been amply fulfilled, and the last part of it has been and is being fulfilled in many places. The third part of it, which comes in the middle, is to be fulfilled as soon as Government will take the matter up, for that alone can make the subject become a source of national wealth. In Germany, fish culture

has been very largely taken up, and all those who are familiar with it are well acquainted with the names of Herr Max von dem Borne and others, who have experimented largely and carried the work to great perfection.

In America, also, a great deal has been done, and the American Government some time ago started a United States Fish Commission which is carried on under Government auspices and devotes attention not only to the stocking of the rivers and the lakes, but to what is more important, the study of the fish themselves, of the animals upon which they feed, of the plants surrounding them in the waters in which the fish live, and of anything else of importance in connection with them. A great deal of work and very important work has been done, and much of our knowledge at the present time has come from the United States and from Canada.

The principle of the artificial incubation of ova is a current of water. It may be a current flowing or rising up perpendicularly or flowing horizontally. In nature we find the eggs deposited—I am alluding now to those of the salmonidæ—in the gravel at the bottom of streams, and we find where they are deposited that the water comes welling up from below through the gravel, and that the eggs obtain thus a sufficient supply of oxygen, and in due course of time hatch. This was followed out for many years by fish culturists, a current of water being caused to flow into the hatching apparatus at the bottom and to flow out at the top, so that it rose up amongst the eggs; and practically this has been carried out with more or less modification until the present time.

The hatching apparatus which is used now chiefly in this country consists of a long box, the water flowing in at one end protected by a water board or breakwater, which is simply to break the current and prevent it from washing away the eggs which are placed in the box. It also diverts the current and sends it down to the bottom of the box. The water passes underneath and passes out at a higher level, where we have a screen of perforated metal to prevent the escape of the little fish, and in this box is placed the hatching apparatus proper, that is, the trays or grilles upon which the ova are deposited. The grilles now in use are made of glass. We found after trying a variety of substances, that glass is the best of anything. It gives off nothing. Wood and metal we know corrode in water, and in some waters some metals corrode very much, and a great deal of loss has been suffered by some who have used metallic trays for the purposes of incubation. The Americans like to do things as we know on a wholesale scale, and, not content with putting a layer of eggs upon the apparatus, they fill a basket, as they call it, half full of eggs. Then they send a current of water welling up from underneath, and of course the effect is that it flows through amongst the eggs, and they find that in due course of time they hatch. I have made very careful inquiries with regard to the result of the hatching of ova in this way, and I have found that the Americans are quite prepared

to admit that they had a larger percentage of mortality in their metal baskets or trays than they had when they used glass grilles. They said "we have discarded glass grilles long ago. They are too expensive"; and they made use of other excuses. But, however, we find in practice that we can get far better results from these glass grilles, because, as I have said, there is nothing to contaminate the ova or do them any injury. The trout eggs absorb any metallic matter which may be in the water, and become so saturated with it in course of time as to be very seriously injured. They may not be absolutely killed at the time, but it has been found that, although there is only a slightly increased mortality in hatching upon the metal, there is a greater mortality amongst the fish afterwards. They do not live to grow up in the same way as they do when they are hatched on the glass. I have here certain little implements which are used in the hatchery for working amongst the ova and the little fish. There is a dipping tube which is used for picking up a fish for examination in the hatching boxes. These are some young trout which I have in here, and they are called "alevins." They are easily picked up in these tubes, which are of different shapes. For all these different appliances, and a great many others, we require a house of considerable dimensions in which to put them. I will show you now a view in one of my hatcheries (Fig. 1).

First of all the water enters the building, and flows along a distributing tank. There are two of these tanks, one containing spring water and the other containing river water. The spring water we find very much the best of the two for incubation, and the river water much the best for growing the fish, so that we can turn on which we like, to suit circumstances as the process goes on. There are pipes by which the water is conducted to the hatching boxes. The hatching boxes are covered with lids in order to keep the fish in the dark. In the natural stream the eggs are buried in the gravel, and we find that light is decidedly injurious to the little embryo trout after they hatch; so we keep them in the dark.

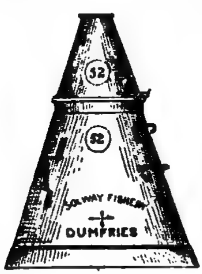

Fig. 2.

These are fish-carriers used for sending away the fish after they have grown (Fig. 2). We put ice in the upper, and the fish in the lower part, and there is a screen of perforated zinc which prevents the ice tumbling in, and as it melts it drips down and keeps the water cool.

There is another view in another hatchery, where we have a tank which is used for spawning purposes, the fish being thrown in after they are spawned, the spawning operations being conducted alongside. I am very sorry that they were not going on at the time that the photograph was taken. But the fish, after having the ova stripped from them, are put into the tank for a short time until they can be taken away.

Fig. 1.—View of Hatchery.

FIG. 3.—SPAWNING SCENE.

In the tank are the bowls or dishes which are used in taking the ova. The eggs are expressed into these dishes. The milt is expressed upon them, and the two mingled together, and after a while they are washed, and the eggs laid down in the hatching boxes. In order to have purity of water—I do not mean chemical purity, but freedom from matter held in suspension—we have to use a system of filtration, and one of the first processes is to filter the water as it comes from the stream itself, and for a long time we had a great deal of trouble in doing this, because the screens which we use choke up and require a great deal of attention, and sometimes cause disaster by being overlooked. We have now got a system which works for a whole season without the slightest attention. Once put it in order, it regulates itself. If we imagine this model to represent the bed of the stream—the sheet of perforated zinc here, and the stream flowing through this box—you can see that the water passing through leaves behind on the zinc anything in the shape of leaves and small pieces of stick and other matters which are floating in it. We found that by setting this at a certain angle if we had twice as much water flowing over it as we had going through the zinc it never stopped; and so, applying this principle, we are able now to run the whole year through without the slightest trouble. The water passes through the zinc into the box, and passes out at the hole at the end, and is drawn off to supply the hatchery.

There is a tank house or place where the water is filtered. Here we have some concrete tanks in which the water is allowed to settle. They are settling tanks in fact. After settling, the water flows from these tanks into a filter box, which is full of wooden screens covered with flannel through which the water passes. This takes away any sediment which may still remain, and the water comes out perfectly pure, passing on into the hatchery.

Having got the hatchery in order, we have to take the eggs from the fish, and this is done first of all by netting them, and then sorting the different kinds into different vessels, and taking them when they are ripe; that is, when they are ready to yield their ova, and by gentle pressure the eggs are quite easily stripped from them. In America this is done with large fish, where a great many have to be done, by putting them into a wooden box by which the head is locked so that it cannot move, and the eggs are taken from it. In this way a large number of fish, like salmon, can be manipulated in a very short time. Here we have a sort of spawning tub. The fish have been taken from a store pond, and are now in the net. Here are the tubs and receptacles into which they are about to be put and then sorted (Fig. 3).

Another photograph will show the next process: a lot of fish being taken and put into tubs. There are the spawning tubs all ready, and the spawning table used in this operation is also shown. The eggs are carried down to the hatcheries and laid down in the hatching boxes. There they remain for a period of something like three months, the

incubation going on meanwhile; and I do not know that there is a much more interesting sight than to watch the development of the embryos. First of all, a short time after laying the eggs down, we find the process of segmentation setting in. There is first a cell, and then a division into two, then into four, then into eight, and sixteen, and so on; and so the process goes on till at last we can detect the chorda dorsalis, or notochord; and at last we see two little black specks which are the eyes of the fish, and when we see this we know that the eggs are almost in a state to bear packing for New Zealand or Australia. We have sent a great many eggs out to New Zealand and Australia, and a great deal of trouble was occasioned in the early days of fish culture by not knowing the exact time at which to pack them. We have found that very soon after the eye-spots appear there is a perceptible motion of the tail of the fish, and also the first appearance of red blood. When we see that, we know that the eggs are fit to be packed, and they travel beautifully on the long voyage to the Antipodes. Here we have the tubs and the operator, and the fish ready to spawn. In due course of time the eggs hatch. The little fish does not look very much like a fish at first. They are very lively and very interesting creatures. Some of the bottles contain ova of trout. One bottle has the ova of salmon in it. The salmon eggs are marked, and the trout eggs are not, so that the mark on the bottle shows which sort it is. There are the little fish in what we call the alevin stage, with the umbilical sac attached (Fig. 4). Through

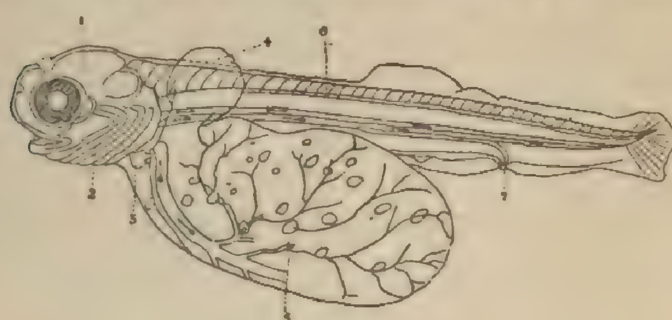

Fig. 4.—Alevin.

a microscope you get a most interesting sight by looking at these little fellows. You can see the circulation of the blood, and the sight is an exceedingly interesting one. Very naturally, delicate little things like these require a great deal of care. Notwithstanding, we have worked the thing to such a point now that we have very little trouble with them during this stage of their existence, if the hatchery apparatus be kept clean. The little pectoral fins are continually moving, and cause currents of water which are passing through the gills, so that the little fish get a supply of oxygen. If we keep the boxes free

from sediment and pollution, we find that we have no trouble with the fish in this stage. A little later on, however, the fish-culturist's troubles begin. The fish begin to feed. The umbilical sac is almost absorbed, and we find the fish rising in the water. Hitherto they have remained pretty much on the bottom, but now we find them rising in the water, heading the current, and to all intents and purposes looking out for food, showing that they are hungry. When we see this we have to begin to feed them. Naturally they have very little mouths, and the difficulty is to find food which is sufficiently small for the little fish to swallow. We have managed to get a good many substances in the shape of artificial food upon which they can be fed, but we find that if we go to nature and take a leaf from her book we can get very much better food in the shape of entomostraca, which can be grown in very large numbers, and upon which the fish thrive very much better than they do on the artificial foods.

It is very natural that with such delicate beings there should be great losses when left to nature, and here is one of the great advantages of fish culture. We can save 95 per cent. of the eggs laid down, whereas if left to nature probably not more than 25 per cent. would ever hatch. Frank Buckland estimated that one egg, or "not one egg," I think he said, in every thousand produced a mature fish, and I do not think that he was far off the mark; so that we see that there is an enormous loss continually taking place in our rivers and streams. It is called a "loss," but I would rather say that these little fish are disposed of by natural means. There is no real loss. We do not recognise such a thing as "loss" in nature. The fish are disposed of by natural means. Nature has arranged so that the enormous numbers of eggs which are deposited should not hatch. We can see that if they hatched the result would be that there would be far more fish in the rivers than the rivers could possibly contain, and therefore there is this great destruction of the ova of the fish in their early stages; whereas, by artificial fish culture, we can save a very large percentage, so that by cultivating the water and making it capable of holding a larger quantity of fish than nature would allow, a great deal may be done, and the supply of fish may be largely increased.

What happens to the salmonidæ of which I have been speaking, happens on a much larger scale to a great many of our marine fishes, and man has a power given him of counteracting this great loss. We have now some marine hatcheries, and a very good work is being begun in those hatcheries. I was at one at Dunbar a little while ago, and saw the work which is being carried on there by Captain Dannevig. He has a series of boxes for hatching ova, and, unlike the boxes which I have here for hatching ova which require to be kept perfectly still, these pelagic ova, accustomed to the motion of the waves, would not do when they were kept in boxes in a state of quiescence, and therefore by means of machinery the boxes are made to move up and down, and the eggs are constantly being slightly

agitated, and you get a motion which is very akin to the motion produced by the waves of the sea, and the results have been found perfect. Before this was obtained a great many difficulties were in the way. The eggs refused to live, and they got matted together, and the modes that were used were to a certain extent unsuccessful. Captain Dannevig has got over the difficulty; and so I believe every difficulty that we have to contend with in fish culture may be got over if we only persevere and strive to overcome these hindrances.

The way in which the loss may be counteracted with regard to our fresh-water fishes is evidently by taking care of the eggs. It is amongst the ova and the fish in its embryonic stage that the great loss occurs, as I have said; so, by making artificial ova beds and laying the eggs down in them in places where the enemies of the fish cannot get in, the eggs can rest there in perfect peace, and can be allowed to hatch. The little fish after they come out can be cared for and protected from their enemies until they have grown to such a size that they can care for themselves; and it is astonishing to see how soon nature teaches them to do this, and how soon they get into the way of finding out shallows, and finding out eddies, and getting behind stones and under cover, and keeping away from their chief enemies, which, I am sorry to say, are often their own parents, or, anyhow, fish of their own species.

These ova beds are constructed just on the same principle that the hatching boxes are constructed in the hatchery, with this difference, that the eggs are hatched among gravel instead of glass. We place some perforated zinc a little way from the bottom of the box, and on that some gravel, and place the eggs among it. The water flowing down to the bottom of the box wells up through the gravel, and so the eggs are incubated successfully. In this way enormous numbers of ova can be hatched, and this plan has been already tried on some of our streams, and has been found to be most successful. The cost is very trifling, and, altogether, fish culture promises in future to do a great deal for many of our rivers.

I have spoken about the young fish beginning to feed. When they begin to feed their troubles really begin. The artificial foods upon which they are fed very naturally give them indigestion, and they suffer from this and from a number of other complaints; and the consequence is that we lose a great many of them. At the present time, if we succeed in rearing one-half of the fish that are hatched, we consider that we are doing very good work. A little while ago, the percentage was less than this. It was about one-third, or 33 per cent. of the fish that were hatched, and this was considered very good work. I believe that we shall very soon get on to raise the percentage to 70 or 80.

Here we have some fry ponds for rearing the fry (Fig. 5). After the latter have begun to feed, they are left in the hatching boxes a short time, just to get accustomed to it. Then they are taken out and put into these narrow ponds, and we have a current of water running through the ponds, and the young fish thrive there, and are fed four

Fig. 5.—Nursery Ponds.

Fig. 6.—Nursery Ponds.

or five times daily. The feeding requires a great deal of skill and experience, and it is thus no light matter. It would take a man the whole of his time to look after a series of ponds like this, and to attend duly to the fish in them, without doing anything else.

This is another series of fry ponds on a piece of level ground (Fig. 6). There we have them rather on a hillside, with a good fall from one to the other, and we find the benefit of that in growing the little fish. Some do very much better than they do when the water has not much fall. The ponds are very much of the same description as the others. We have here at each end a screen to prevent the little fish getting out, and the water flows in at one end and out at another, and then on to the next pond, and so on.

Then the little fish in due time grow to the size which we call yearlings. They are not really a year old, but it seems to be the best name to give them for distinction, and as they are yearlings when they are really a year old and some time after, it seems quite fair to call them yearlings before they have actually lived twelve months. The time that they pass from the fry stage to the yearling stage may be said to be the time during the summer months, when the weather is too warm and the temperature too high to send them very long journeys for stocking rivers and lakes. As soon as the cold weather comes, at the end of August or September, then the fish can travel by rail and otherwise, and they rejoice in the name of yearlings.

The scene here represents the preparation of the yearling fish for a journey (Fig. 7). They cannot be taken out of the pond and sent away at once. We had great losses some years ago in doing this. The fish were put into the carrying tanks and sent off, and we had to make elaborate arrangements for changing the water during transit, which has been found since to be one of the very worst things that can be done, and now we never change the water except as a very last resource, in case of some unlooked-for emergency. The fish are taken out of the pond and confined in these tanks with water running through them for a considerable time—two or three days at least—and in there they are not fed. We find that they travel very much better on empty stomachs than they do after a meal, and, as it does not seem to do them any harm to starve them a little, we do not feed them before sending them away, and we find that the result is perfectly satisfactory. These are the cans which I described before for putting them in. They have the ice on the top, and the fish in the cavity below.

Now, what is the outcome of all this? We have cultivated fish now for thirty years or more, and we have got to know a good deal more about them than we knew at the beginning of that time. Well, we find on looking round that a great many, in fact a large majority, of the streams of this country are in their present state almost worthless. They will not hold trout of any size, and it is very difficult indeed to get good fishing. Little worthless brooks have, in cases where they have been dealt with, been made to produce tons of [illegible] and one—a brook which practically would not produce fish at

all, naturally, and the trout in which were so insignificant in size as hardly to be worth noticing—from a fisherman's point of view I am speaking now—was made to produce tons of fish. One pond alone produced several times over, upwards of fifteen hundredweight of fish. The pond was only ninety feet long by thirty feet wide. Of course the fish had to be largely fed on artificial food, but by using the artificial food twice a day the ponds produced a large quantity. This shows what water may be made to do; and when we hear about the over-crowding of fish in our rivers and lakes, it strikes a fish-culturist sometimes as being the height of absurdity. We find, however, in our streams that there is often little or no water, and that the fish are run back into the pools and have to wait there a considerable time until a flood comes, or until a shower comes which causes the stream to rise, and during this time they get very little food. The food supply in the streams, owing to the lowness of the water, is almost destroyed, and the animals which inhabit the streams, like the fish, suffer very much from the lowness of the water, which is caused very largely by the surface or hill drainage which has been carried on for thirty or forty years in this country.

Now, all this can be counteracted, I believe, very easily. No doubt we have a great deal to learn about it yet, but we are on the right tack, and I think that after a while we shall be able to remedy this state of things to a large extent. We find that from this state of lowness of water we suddenly drift into a state of heavy flood. The rains come down, and the water comes down from the hills in heavy floods—far heavier than came down before the hills were drained. These floods carry everything before them, sometimes washing away bridges, and doing more or less damage to property. Now this water must be put under control, and when we get it under control we find that it is, indeed, a most controllable thing. We find that we can do with it what we did not anticipate but a few years ago. At those times of the year when the water supply is naturally deficient, it must be gently increased, and I need hardly point out, that by caring for it even to this extent, one of the natural consequences will be an increase in the quantity of that class of food which is produced in the stream itself, or in its immediate surroundings or accessories. The fish, too, will at once have a better range, and so will feed more freely than they do when confined in a pool where starvation has become a necessity on the one hand, and escape a practical impossibility on the other. In addition to having become possessed of more roomy quarters, the whole tone of their surroundings has become better. The water in which they live, and on which their very existence depends, has become fresher and contains more oxygen. The fish feel and enjoy a freedom which before they were unacquainted with; and, in addition to this, if a sufficiency of proper food be forthcoming, they will at once begin to put on flesh and grow in a surprising manner.

The water supply can very easily be managed by impounding, and by making reservoirs on the streams so that compensation water can

be let off during dry weather. In this way the streams can be kept up to their proper limits. They need never run so low as they have been accustomed to do. But we find that by impounding water the floods are lessened, and therefore that great scouring process which goes on in the streams, destroying both animal and vegetable life, is to a great extent lessened, and everything living in the water has a very much better chance of existence than it had before.

The desired result cannot be obtained by making one simple dam upon a stream. Take a river for instance: if we make a dam, as has been suggested—and one or two places of the kind have been made up at the head of the waters of some streams—when the water is let off as compensation water it is found, in one case which I remember, that when it has run eight miles, after being started as a roaring torrent from the reservoir known as Lake Vyrnwy in Wales, the stream is not very perceptibly affected. I believe that it was raised about one inch; but there are other tributaries coming in, and if there were reservoirs on these other streams, and we had compensation water let off from them, we should get a rise of several inches instead of only one inch, and we should find that the result would be very beneficial.

I remember an attempt being made to bring up sea fish by an artificial spate at a place in Scotland, and it was eminently successful. The landed proprietor there blocked up the outlet from one of the lakes, and then when the salmon were waiting to come up the river he let off the water from this impounded lake, and the consequence was that he got a good run of fish. So successful was it, and so pleased was he, that he very soon tried it again, but the second time it was just as unsuccessful as the first time it had been successful. The consequence was that they came to the conclusion that the fish had found before that they had been deceived, that there had not been really a spate, that it had not been raining at all; and therefore the next time they fought shy of it and would not come up. When I came to make inquiry I could not find that there had been any fish waiting to come up; and when these artificial spates are made it is necessary to be exceedingly careful to make them not only in the right way but at the right time. In one instance water was let off from a reservoir very near the bottom, the bank being, I think, something like eighty or ninety feet high. The water was let off at a level very near the bottom of the reservoir. Now, if the water had been let off from a level near the surface it would have been very much more beneficial to the fish. The water low down in a reservoir contains very much more matter in suspension, and it is of a very different nature from the water on the surface; and so, for fish-cultural purposes we must take the water from the surface of the lake, or as near it as possible, and then we may expect the fish to appreciate it and follow the spate. Sometimes the fish do not want to go. Well, it is of no use to make a spate then. If the fish do not want to run you may let off water, and you may do what you like, but you cannot make them go. But in my experience, and I have

tried a good many experiments on trout, I have found that nothing is easier than to make trout run when you get an artificial spate at the proper time and made in the proper way.

In the case of sea fish there are some very important things to be considered. First of all we have the sea to contend with. The fish are coming up from the sea. Now we find that the anadromous or sea-going fish run on flood tides, and we know that they enter the river usually a little before high water, so that to let in the spate on an ebb tide would be absolutely useless. Then, again, we find that the wind has a great deal to do with the run of fish. On our west coast, or on some of our west coast rivers, when we get a wind from the westward we find, other things being equal, that the fish will run very much better than with an east wind. They will often hardly run at all with an east wind, even though other things may be favourable; so that the wind is an element which has to be considered. Barometrical conditions have also to be considered, and we find that they play a very important part indeed in influencing the movements of our fishes. Then we find, above all, that, although the fish run upon a flood tide, on spring tides they run very much better than they do on neap tides, when they often run very tardily; so that by taking advantage of a knowledge of these facts and others, we can let off impounded water at a time when it will be likely to bring them; and there is no doubt whatever that if the thing were properly carried out it would be eminently successful. As regards trout, a very moderate amount of water is sufficient to produce very great results. I have seen a stream utilised which ran almost dry in dry weather. The water of the stream has been made to produce a large quantity of fish, as I have just described.

Here we have such a stream (Fig. 8), but with a rocky bed almost dry. The water retires into the pools in which the fish live during the times of drought. On this stream we have a dam made to run across, and raising the water some three feet above its natural level. Here, where the man is sitting, is a sluice, and the water is allowed to escape through this sluice, which regulates the supply, and it flows away into the woods. It passes through a pine forest, and by means of this aqueduct goes on. Here is another view of the same aqueduct, and so it goes on flowing for a distance of about half a mile, the country through which it passes being from many circumstances unfavourable for the construction of ponds. That, however, is no great difficulty. It is simply a case of taking the water a little farther on until we get to a suitable place for the construction of the ponds in which the fish must live. The spout or bridge is to conduct the surface water or rain water over the aqueduct, and to prevent its getting in in excess. The surface water, if allowed to get in in excess, has a prejudicial effect, so we employ a large number of these little bridges for the purpose of keeping it out. Little canals are dug in various directions for conducting the water into these spouts. The water passes on and flows into this pond here, and in this pond a large number of fish have been produced.

Fig. 8.—Trout Stream

The pond has to be cultivated. The water is cultivated not only as regards the fish, but as regards the vegetation which is in it. A large number of plants are introduced both into the pond itself, and also into accessory ponds; and this is one of the most important branches, perhaps, of modern fish culture—the growing of the food upon which the fish live. Into the accessory ponds we can introduce creatures which multiply enormously under favourable circumstances; and we find that these creatures can be let off in large numbers by simply drawing the sluices and allowing a quantity of water to pass into the fish pond, and that the fish then take them. A sufficient quantity are left behind to keep up the supply, and the pond is refilled with water; and so, by having a few of these ponds constructed we can keep up a very fair supply of food for the fish. Where fish culture is carried on on a very extensive scale, it is necessary to supplement this supply, and in some cases to supplement it largely, by artificial food; but, as applicable to our rivers, it would not be necessary to do this; and I believe that on any river if the matter were taken up in earnest it would be possible to do it by impounding water so as to counteract the effects of drought in the summer, and also to partially counteract the effects of floods by impounding the water instead of letting it come pell-mell down the stream. By growing food to supply the fish, we can get a very much finer and better race of fish than we can if the matter be left entirely to nature. We find that there are certain streams which produce very much better fish than others. In these streams the fish are fed upon certain creatures, and by taking care of those creatures and multiplying them, we can produce a large amount of valuable fish food—a thing which was never thought of years ago, but which now is coming to the front, and probably before long the plan will be largely adopted.

This represents such an accessory pond as I have described. You see a number of water plants growing in the water. Here are the floating leaves in various directions, and there are others throwing up their stems and leaves with a mass of vegetation all round. This pond produces an enormous quantity of Limnea peregra and other creatures upon which the trout are fed. All these, it has been proved, are easily applicable to trout and to trout waters. The plan is also applicable on a very large scale to salmon rivers; and how much more important are salmon rivers than trout streams. How much more important are the salmon as articles of food for human consumption than the trout. And yet the salmon are being neglected, and the trout are being cared for. We want, not exactly the reverse, but we want to have the salmon cared for too; and that is one of the things that I have been trying to bring before the people of this country for years, and I think that I may say that already my efforts are being crowned with some kind of success.

We find that the practice of hill draining on the rivers produces a great effect; and what has been partly, I think, overlooked—for I have never heard it alluded to—is that the hill drainage, by lessening the

quantity of water in the rivers, largely lessens the quantity of fresh water which is poured into the estuaries into which the rivers flow during times of drought. Then, on the other hand, we get the contrary during floods, when an enormous bulk of fresh water is poured into the salt water in the sea, and in a shallow estuary, such as the one upon which I live, and which is represented roughly here, we find that, with these rivers flowing into it (the watershed of the firth is I think something like nearly ten times greater than the firth itself, and the firth is a very shallow one) that the specific gravity of the water, the temperature of the water, and other things, are tampered with to such an extent that some of the fish actually deserted it about forty years ago, which, I think, would be somewhere about the time that the hill drainage commenced. The herring is one fish that has deserted the firth, and since that time it has never to any extent come into it. Sometimes some herrings for a short time will come in, but they are very soon out of it again, which shows that when favourable conditions occasionally occur the fish will come into the water; whereas, owing to this drought altering the specific gravity, we find the fish keeping away.

All these matters are of the greatest importance with regard to the regulation of our fisheries, both marine and fresh water, and they want looking into. I think that, perhaps, one of the greatest delights, or the greatest delight, of fish culture is that there is so much to be learnt, and that we are always finding out something new, and that there is always a field to which we can turn for searching out the hidden mysteries of nature and increasing our knowledge, and learning more about the fishes that we have been talking about.

I would have liked to say a little about the diseases of fish, but I am afraid that there is no time. We have already over thirty of these diagnosed, and, what is more, we have found out the means of cure for a number of them, and we have been helping fish culture very much in this way. Many of the diseases are parasitic, and we find parasites which affect the fish which were not known to fish-culturists years ago. One is a curious protozoan which gets on the bodies of the fish, and has been known to kill them in large numbers. It can be destroyed in a rather peculiar way, by placing the fish in a tank with a current of water flowing through it, the bottom strongly impregnated with salt, a saturated solution of salt. The fish keep in the upper water, which is fresh. These curious little protozoans at certain times leave the fish and go down to the bottom. There they divide, and they are multiplied by division and produce enormous numbers. These free-swimming little creatures get into the water and swim about, and are taken up by the fish again. We find that by having a saturated solution of salt at the bottom of the water and a current of fresh over it, the fish live in the fresh water, and the parasites, when they leave the fish and go down to the bottom, are not able to reach the fish again, because they are killed at once by the salt.

[J. J. A.]

WEEKLY EVENING MEETING,

Friday, February 21, 1896.

Sir Frederick Abel, Bart. K.C.B. D.C.L. LL.D. F.R.S. Vice-President, in the Chair.

Edward Frankland, Esq. D.C.L. LL.D. For. Sec. R.S. *M.R.I.*

The Past, Present and Future Water Supply of London.

In a discourse to the Members of the Royal Institution on the subject of the metropolitan water supply nearly thirty years ago, I stated that out of every thousand people existing upon this planet at that moment three lived in London; and, as the population of London has in the meantime doubtless grown at a more rapid rate than that of the rest of the world, it will probably be no exaggeration to say that now, out of every thousand people alive on this earth, four live in London; and therefore any matter which immediately concerns the health and comfort of this vast mass of humanity may well merit our most earnest attention. Amongst such matters that of the supply, in sufficient quantity, of palatable and wholesome water is certainly not the least in importance.

It is not therefore surprising that this subject has received much attention from several Royal Commissions, notably from the Royal Commission on Water Supply of 1867, presided over by the Duke of Richmond; the Royal Commission on the Pollution of Rivers and Domestic Water Supply of Great Britain, presided over by the late Sir William Denison, of which I had the honour to be a member; and lastly the Royal Commission appointed in 1892 to inquire into the water supply of the metropolis, of which Lord Balfour of Burleigh was chairman, and of which Professor Dewar was a member.

The Royal Institution has also, for nearly three-quarters of a century, been prominently connected with the investigation and improvement of the metropolitan water supply, no less than four of our professors of chemistry having been successively engaged in this work, viz. Professors Brande, Odling, Dewar and myself, whilst three of them have been members of the Royal Commissions just mentioned. I may therefore perhaps be excused for accepting the invitation of our secretary to bring the subject under your notice for the third time.

On the present occasion, I propose to consider it from three points of view, viz. the past, the present and the future; and for reasons which will appear hereafter, I shall divide the past from the present at or about the year 1883, and will not go back further than the year 1828, when Dr. Brande, Professor of Chemistry in the Royal

Institution, Mr. Telford, the celebrated engineer, and Dr. Roget, Secretary of the Royal Society, were appointed a Royal Commission to inquire into the quality and salubrity of the water supplied to the metropolis.

The Commissioners made careful examinations and analyses, and reported as follows: "We are of opinion that the present state of the supply of water to the metropolis is susceptible of, and requires improvement; that many of the complaints respecting the quality of the water are well founded; and that it ought to be derived from other sources than those now resorted to, and guarded by such restrictions as shall at all times ensure its cleanliness and purity." (At this time the water was pumped from the Thames between London Bridge and Battersea). "To obtain an effective supply of clear water, free from insects and all suspended matter, we have taken into consideration various plans of filtering the river water through beds of sand and other materials; and considering this, on many accounts, as a very important object, we are glad to find that it is perfectly possible to filter the whole supply, and this within such limits, in point of expense, as that no serious objection can be urged against the plan on that score, and with such rapidity as not to interfere with the regularity of service."

Before the year 1829, therefore, the river water supplied to London was not filtered at all; but after the issue of this report, the companies set themselves earnestly to work to improve the quality of the water by filtration.

The first filter on a working scale was constructed and brought into use by the Chelsea Water Company in the year 1829. But even as late as 1850, only three out of the seven principal companies filtered the river water which they delivered in London; and it was not until 1856 that filtration was made compulsory by Act of Parliament; whilst it can scarcely be doubted that between this date and the year 1868, when my observations on turbidity were first commenced, the operation was very imperfectly performed.

In the year 1832, and again in 1849, London was severely visited by epidemic cholera, and the agency of drinking water in spreading the disease forced itself upon the attention of the observant portion of the medical profession. It was Dr. Snowe, however, who in August 1849 first formally enunciated the doctrine that drinking water, polluted by choleraic matters, is the chief agent by which cholera is propagated.

Received at first with incredulity, this doctrine was supported by numerous facts, and it soon caused renewed attention to be directed to the quality of the water then being supplied to the metropolis, with the result that the intakes of the various companies drawing from rivers were, one after another, removed to positions above the reach of tidal influence, the Thames water being withdrawn from the river above Teddington Lock, and the Lea water at Ponder's End, above the tidal reaches of that river.

In every visitation of Asiatic cholera to London, the water supply was either altogether unfiltered or imperfectly filtered, besides being derived from highly polluted parts of the Thames and Lea; and the enormous loss of life, amounting in the aggregate to nearly thirty-six thousand people, can only be attributed to this cause; for it has now been abundantly proved that cholera is, practically, propagated by drinking water *alone*, and that efficient filtration is a perfect safeguard against its propagation. Moreover, it is most satisfactory to know that, since the year 1854, no case of Asiatic cholera in London has been traced to the use of filtered river water. The following table clearly indicates the close connection between intensity of pollution and cholera mortality:—

CHOLERA EPIDEMICS IN LONDON.

—	Character of Water Supply as regards Excremental Pollution.	Total Mortality from Cholera.	Mortality from Cholera per 10,000 of Population.
Epidemic of 1832	Polluted	5,275	31·4
" " 1849	Very much polluted ..	14,137	61·8
" " 1854	Less polluted	10,738	42·9
" " 1866	Much less polluted ..	5,596	18·4

These are the results arrived at by the most general investigation of the subject. They show that in every epidemic, the mortality varies directly with the intensity of the drainage pollution of the water drunk by the people; but, if time permitted, a more detailed study of the statistics in these epidemics would demonstrate, much more conclusively, this connection between cholera mortality and the pollution of drinking water, a connection which has quite recently been terribly emphasised in the case of Hamburg.

Such is the verdict with regard to cholera, and the same is true of that other great water-borne disease, typhoid fever. But, unlike cholera, this disease is disseminated in several other ways, and its presence or absence in any locality may not, of necessity, have any connection with the drinking water, as is strikingly shown by the health statistics of Manchester.

There is no evidence whatever that, since the year 1869, when typhoid fever appeared for the first time as a separate disease in the Registrar-General's reports, it has been conveyed by the water supply of the metropolis. An inspection of the following diagram shows, it is true, a greater proportional mortality during the period of imperfect filtration than during the later period; that is to say, from 1883, when the process began to be performed with uniform efficiency; but the plotting of a similar curve for the deaths by typhoid in Manchester, shows that this disease arises from other causes than polluted water, since the water supply of Manchester, derived as it

is from mountain sources, is above all suspicion of this kind. These other causes have during the last ten years been much mitigated in London by various sanitary improvements; whilst, as shown in the diagram (Fig. 1), there has been no corresponding mitigation in Manchester. In this diagram the continuous dotted line shows the mortality per 100,000 of population from typhoid in Manchester, and the crossed broken line the death rate from the same disease in London; whilst the faint broken line represents the degree of turbidity of river water delivered in London.

Although, very soon after the year 1856, all the water supplied to the metropolis was obtained from sources much less exposed to drainage pollution, it was still very carelessly filtered. Previous to the year 1868, there are no records of the efficiency, or otherwise,

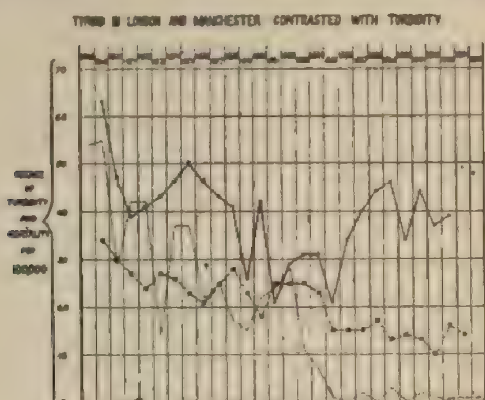

Fig. 1.

of the filtration of the metropolitan water supply derived from rivers as distinguished from deep wells, the water of which is perfectly clear without filtration.

It was in the year 1868 that I first began to examine the water supplied to the metropolis from rivers with reference to efficiency of filtration. In that year, out of eighty-four samples examined, seven were very turbid, eight turbid, and ten slightly turbid, so that altogether no less than nearly 30 per cent. of the samples were those of inefficiently filtered water. The metropolitan water supply, then, up to the year 1868, may be shortly described as derived, for many years, from very impure sources, with either no filtration at all, or with very inefficient filtration; and afterwards, when the very impure sources were abandoned, the supply was still often delivered in a very inefficiently filtered condition. But, after the establishment of monthly reports on the filtration of the river-derived supplies, the

quality of these waters gradually improved, in this most important respect, as is seen from the foregoing diagram.

These observations, graphically represented in the diagram, show that at the time they were commenced the filtering operations were carried on with great carelessness, and that this continued, though to a less extent, down to the year 1883, since which time, and especially since 1884, the efficiency of filtration of all the river waters supplied to the metropolis has left little to be desired.

What is it, then, that separates the past from the present water supply of London? In the first place there is the change of source; I mean the change in position of the intakes of the several companies drawing from the Thames and Lea, and the total abandonment of the much polluted Ravensbourne by the Kent Water Company. So long as the water supply was derived from the tidal reaches of the Thames and Lea, receiving, as these reaches did, the drainage of immense populations, the risk of infection from water-borne pathogenic organisms could scarcely be otherwise than imminent; for, although we now know efficient filtration to be a perfect safeguard, anything short of efficiency must be attended with risk in the presence of such extreme pollution.

Nevertheless, the line of demarcation between the past and the present water supply of the metropolis is, in my opinion, to be drawn not when the intakes of the river companies were removed to positions beyond the possibility of pollution by the drainage of London; but at the time when efficient filtration was finally secured and ever since maintained; that is to say, in the year 1884.

The removal of turbidity by sand filtration, however, refers only to suspended matter, but there are sometimes objectionable substances in solution, of which organic matter is the most important. River water and mountain water, even when efficiently filtered, contain more organic matter than spring or deep-well water; but this is reduced in quantity by storage and especially by filtration; although it can, perhaps, never be brought up to the standard of organic purity of spring and deep-well water.

The Present Water Supply.

At present, London is supplied with water from four sources, the Thames, the Lea, the New River and deep wells. Of these, the deep wells yield, as a rule, the purest water, requiring no filtration or treatment of any kind before delivery for domestic use. The river waters, on the other hand, require some kind of treatment before delivery;—storage and subsidence in reservoirs, and filtration. The water from the Thames is abstracted at and above Hampton, far above the reach of the tide and London drainage. The water from the Lea is taken out at two points, viz. at Angel Road, near Chingford, by the East London Water Company; and above Hertford by the New River

Company, who convey it to Green Lanes by an open conduit 25 miles long called the New River Cut, in which it is mixed with a considerable volume of spring and deep-well water.

All three river waters are affected by floods, and are, as raw materials, of considerably different quality as regards organic purity, as is seen in the diagram (Fig. 2). From these raw materials, by far the largest volume of the metropolitan water supply is derived; and the chemical, or organic, purity of the water sent out to consumers stands in direct relation to the organic purity of the raw material used, as is seen from the diagrams (Figs. 3, 4 and 5), which show the proportional amounts of organic elements in the raw and filtered waters, and also the advantage of storage in excluding flood water. Fig. 4 shows that floods in March and August were circumvented, but

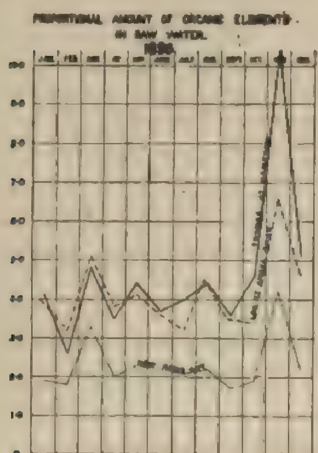

Fig. 2.

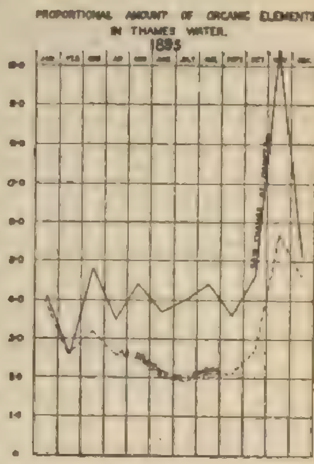

Fig. 3.

not in November. The numbers in the margins of the diagrams express the proportional amount of organic elements, that in the Kent company's water during the nine years ending December 1876, being taken as unity, as is depicted in the diagram (Fig. 5).

Hitherto I have spoken of chemical purity, or comparative freedom from organic matter only; but the spread of diseases, such as cholera and typhoid fever, through the agency of drinking water, has no connection whatever with the chemical or organic purity of the water. These diseases are propagated by living organisms of extreme minuteness, to which the names *bacilli*, *bacteria* and *microbes* have been given; and here comes the important question, how, if at all, does filtration secure immunity from these water-borne diseases?

To Dr. Koch, of Berlin, we are indebted for the answer to this

question. By his discovery of a means of isolating and counting the number of bacteria, or bacilli, or microbes, and their spores in a given volume of water, we were for the first time put into possession of a

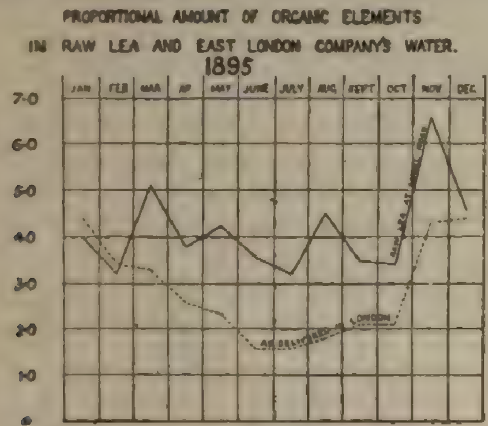

FIG. 4.

method by which the condition of water as regards these living organisms, before and after filtration, can be determined with quantitative exactness. The enormous importance of this invention, which was

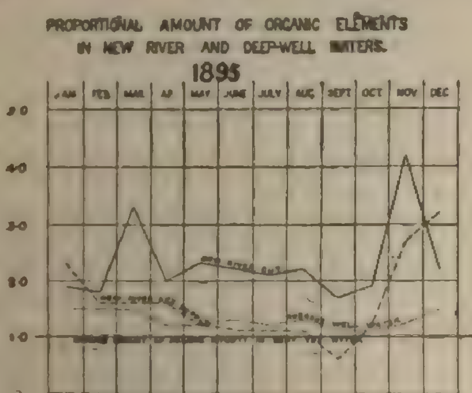

FIG. 5.

first made known and practised in England in 1882 by the late Dr. Angus Smith, is evident when it is borne in mind that the living organisms, harmful or harmless, contained in water are of such

extreme minuteness as, practically, to defy detection by ordinary microscopical examination. But, although the microscope cannot detect with certainty single bacteria or their spores, even the naked eye can easily discern towns or colonies consisting of thousands or even millions of such inhabitants.

Dr. Koch's method accomplishes at once two things: it isolates, in the first place, each individual microbe or germ; and secondly, places it in conditions favourable for its multiplication, which takes place with such amazing rapidity that, even in a few hours, or at most in two or three days, each organism will have created around itself a visible colony of innumerable members; a town, in fact, comparable to London itself for population.

By operating upon a known volume of the water under investigation, such as a cubic centimetre for instance, the number of separate organisms or their spores, in a given volume of the water, can thus be determined.

The following is the method now adopted in carrying out Koch's process for the investigation of drinking water:—

1. Preparation of the nutritive medium.
2. Sterilisation of the medium.
3. Collection of the sample of water in a vacuous tube to be hermetically sealed immediately afterwards.
4. Transport of the sample to the bacteriological laboratory.
5. Mixture of a known volume of the water sample with the nutrient medium.
6. Casting of the mixture into a solid plate.
7. Incubation of the solid plate.
8. Counting of the colonies (suitable time for the colonies to develop being given as shown in diagrams, Figs. 6, 7, 8 and 9).

Fig. 6 shows a gelatine culture of unfiltered Thames water placed on a ruled surface to assist counting; whilst Figs. 7, 8 and 9 illustrate the gradual development of the colonies in a gelatine culture of $\frac{3}{100}$ of a cubic centimetre of unfiltered Lea water collected at the East London Company's intake on January 13, 1896. Fig. 7 shows the condition of the colonies on the third day; Fig. 8 the further development on the fourth day; and Fig. 9 the condition of the colonies on the fifth day, when many colonies are mingled together and counting is no longer possible.

9. Examination of separate colonies, or rather of the individual members, under the microscope.

Sometimes the cultivations are made upon a plate of the substance called agar, which resembles isinglass, and bears a temperature of blood heat without melting (Fig. 10). There is a very remarkable colony on this plate, showing an apparently organised city, with suburbs stretching far into the country, and containing many millions of inhabitants.

In order to ascertain the effect of filtration upon the bacterial quality of the water, it is absolutely necessary that the sample should

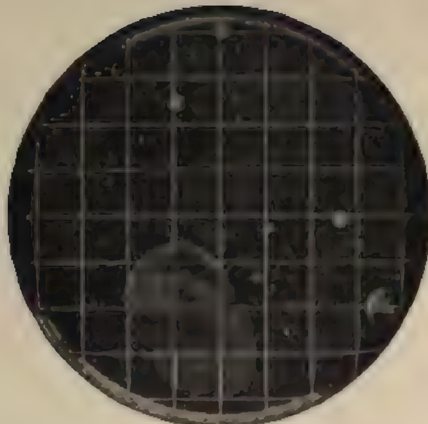

Fig. 6.

Fig. 7.

FIG. 8.

FIG. 9.

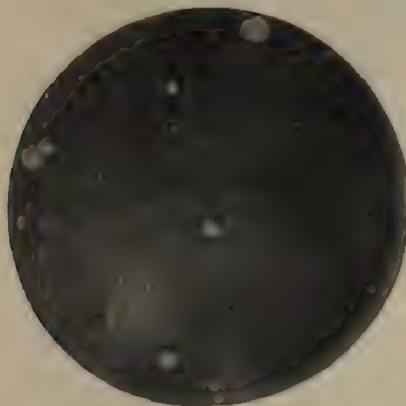

FIG. 10.

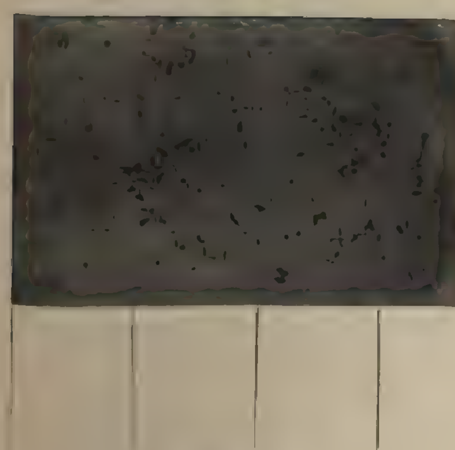

FIG. 11.

be taken immediately after it has passed through the filters; for if it be obtained from the delivery mains in town, that is to say, after the water has passed through many miles of pipes, the rapid multiplication of these organisms, except in very cold weather, is such that a water which contains only a single living organism per c.c., as it issues from the filter, may contain 100 or 1000 in the same volume when, after several hours, it arrives on the consumers' premises. Fig. 11 shows isolated bacteria, enormously magnified, taken from one of the towns or colonies. The scale at the foot of this figure represents thousandths of an inch.

Now what is the effect of sand filtration as carried out by the various water companies supplying London, upon the living matter contained in the raw river water? *It is simply astounding*—water which, when poured upon sand filters, contains thousands of bacteria per c.c.—for a single drop of Thames water sometimes contains nearly 3000 separate organisms—comes out from those filters with 50, 30, 10, or even less of these organisms per c.c.; or the number of microbes in a single drop is reduced to two or even to zero.

Rather less than one-tenth of the total volume of water supplied to London is derived by the Kent Water Company from deep wells in the chalk. As it issues from the porous rock into the fissures and headings of these wells, this water is, in all probability, absolutely sterile; but, by the time it has been pumped up to the surface, it usually contains a certain number, though small, of microbes. Thus, during the year 1892 it contained on the average 6 per c.c.; in 1893, 13; in 1894, 15; and in 1895, 8.

The diagram (Fig. 12) shows, graphically, the bacterial improvement of the river water by filtration during the year 1894. In this diagram, the black squares and white centres represent the relative numbers of microbes in the unfiltered and filtered waters respectively.

Thus, although the deep-well water has, from a bacterial point of view, a decided advantage, the filtered river waters are not very far behind; and there is every reason to believe that with the improvements which are now being carried out by the various river-water companies, the Kent company's water will before long be run very hard by the other supplies.

By the examination of the water as it issues from the filters, the utmost freedom from microbes, or maximum degree of sterility, of each sample is recorded. This utmost freedom from bacterial life, after all sources of contamination have been passed, is obviously the most important moment in the history of the water; for the smaller the number of microbes found in a given volume at that moment, the less is the probability of pathogenic or harmful organisms being present; and although the non-pathogenic may afterwards multiply indefinitely, this is of no consequence in the primary absence of the pathogenic; but it is only fair, in describing the character of the present water supply of London, to say that not a single pathogenic

organism has ever been discovered, even in the unfiltered water as it enters the intakes of the various companies, although these organisms have been carefully sought for. It is sometimes even said that the non-pathogenic organisms found in water may be beneficial to man, but this idea is not borne out by their entire absence from the food which nature provides for young animals. Milk is absolutely sterile in its normal condition.

As it is at present impracticable to obtain water, uniformly at least, free from microbes, it is desirable to adopt some standard of bacterial purity; and 100 microbes per c.c. has been fixed upon by

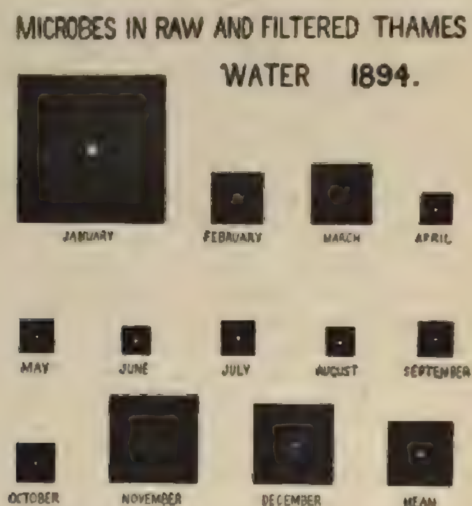

Fig. 12.

Dr. Koch and myself as the maximum number allowable in potable water. This standard is very rarely infringed by the London water companies; whilst I have every reason to hope that, in the near future, now that special attention is directed to bacterial filtration, it will not be approached within 50 per cent.

This hope is based, not only upon my own observations, but also upon the exhaustive and exceedingly important investigations carried out at the Lawrence Experiment Station by the State Board of Health of Massachusetts, under the direction of Mr. George W. Fuller, the official biologist to the Board. More than six years have already been spent in the prosecution of these American experiments, and many thousands of samples of water have been submitted to bacterial cultivation.

The Massachusetts experimental filters were worked at rates up to

three million gallons per acre daily, which renders the results available for application to public water supplies; indeed, none of the water delivered in London is filtered at so rapid a rate as this. It was found that, at these rates, all the disease-producing germs which were intentionally, and in large numbers, added to the unfiltered water, were substantially removed. The filters were so constructed and arranged as to allow direct comparison of the bacterial purification of water under different rates of filtration—with sand of different degrees of fineness, with different depths of the same sand, and with intermittent and continuous filtration.

The actual efficiency of these filters was also tested by the application of the bacillus of typhoid fever. During the earlier portions of the year 1893 very large numbers of these bacilli and other species were applied in single doses to the several filters at different times, and the effluent was examined four times daily for several days afterwards. The results so obtained give a thoroughly trustworthy test of the degree of bacterial purification effected by each of the experimental filters, and these are the data which have been largely used by the Massachusetts State Board of Health in deducing the rules which they consider ought to be observed in water filtration.

Among the subjects investigated by means of these experimental filters were:—

1. The effect upon bacterial purification of the rate of filtration.
2. The effect of size of sand grains upon bacterial purification.
3. The effect of depth of material upon bacterial purification.
4. The effect of scraping the filters upon bacterial purification.

These important experiments and my own bacterioscopic examinations of the London waters, continued for four years, lead to the following conclusions:—

1. The rate of filtration, between half a million and three million gallons per acre per day, exercises, practically, no effect on the bacterial purity of the filtered water. It is worthy of note that the rates of filtration practised by the several water companies drawing their supplies from the Thames and Lea, are as follows:—Chelsea Company, 1,830,000; West Middlesex, 1,359,072; Southwark Company, 1,568,160; Grand Junction Company, 1,986,336; Lambeth Company, 1,477,688; New River Company, 1,881,792; and East London Company, 1,393,920. Hence not one of the London companies filters at the rate of two million gallons per acre per day, at which rate in the Massachusetts filters, 99·9 per cent. of the microbes present in the raw water were removed.

2. The effect of the size of sand grains is very considerable. Thus, by the use of a finer sand than that employed by the Chelsea Company, the West Middlesex Company is able, with much less storage, to attain an equal degree of bacterial efficiency.

3. The depth of sand between the limits of one and five feet exercises no practical effect on bacterial purity, when the rate of filtration is kept within the limits just specified. Thus the New River Company,

with 1·8 foot of sand on the filters, compares favourably with the Chelsea Company, the sand on whose filters is more than twice that depth. Placed in the order of thickness of sand on their filters, the following table shows that the metropolitan companies range as follows:—Chelsea, Lambeth, West Middlesex, Southwark, East London, Grand Junction and New River. Placed in the order of efficient bacterial filtration, they range as follows:—Chelsea and West Middlesex equal, New River, Lambeth, East London, Southwark and Grand Junction.

THICKNESS OF SAND ON FILTERS.

Chelsea	4·0 feet.
Lambeth	2·8 "
West Middlesex	2·6 "
Southwark	2·5 "
East London	2·0 "
Grand Junction	1·9 "
New River	1·8 "

ORDER OF BACTERIAL EFFICIENCY.

{Chelsea.	East London.
{West Middlesex.	Southwark.
New River.	Grand Junction
Lambeth.	

4. When there is such an accumulation of deposit on the surface of the sand filter that, for practical purposes, sufficient water cannot be made to pass through it, the surface of the filter has to be scraped, that is to say, the mud and about half an inch of the sand are removed from the surface. After this operation, there is sometimes an increase in the number of bacteria in the filtered water, and it is noticed that the increase is greater in shallow than in deep filters, and with high than with low rates of filtration; and there is no doubt that the effect of scraping is considerably magnified when coarser descriptions of sand are employed, as is the case in the filters of the London water companies. I should like, therefore, to impress upon the engineers of these companies the desirability of using finer sands than are at present employed.

Influence of the Bacterial Condition of the Raw River Water upon that of the Filtered Effluent.

I have found that the number of bacteria in a given volume of filtered water is often, though not invariably, influenced by the number contained in the raw water supplying the filter; and from this point of view, therefore, the bacterial condition of the raw river water used in the metropolis is of no inconsiderable importance.

Since May 1892, I have been making monthly determinations of the number of microbes capable of developing on a gelatine plate in a given volume of Thames water collected at the intakes of the metropolitan water companies at Hampton; and the number has varied during this time between 631 and 56,630 per c.c., the highest numbers having, as a rule, been found in winter, or when the temperature of the water was low, and the lowest in summer, or when the temperature was high.

Now, besides temperature, there are two other conditions, to either of which this difference may be attributed, viz. sunshine and rainfall; and I have endeavoured, by a series of graphic representations, to disentangle these possible influences from each other, by placing the results of the microbe determinations in juxtaposition with (1) the temperature of the water at the time the samples were taken; (2) the number of hours of sunshine on the day and up to the hour when the sample was drawn, and on the two preceding days; and (3) the flow of the Thames over Teddington Weir on the same day, expressed in millions of gallons per twenty-four hours. And, although the graphic representations are confined to the Thames, the conditions affecting bacterial life in this river are doubtless equally potent in other rivers and streams.

The samples for microbe cultivation were collected at about nine inches below the surface of the water, in partially exhausted and sealed glass tubes, the ends of which, when the tubes were lowered to the required depths, were broken off by an ingenious contrivance devised by my assistant, Mr. Burgess. On being withdrawn from the river, the tubes were immediately hermetically sealed and packed in ice for conveyance to my laboratory, where the cultivation was always commenced within four hours of the time of collection.

For the records of sunshine I am indebted to the kindness of Mr. James S. Jordan, of Staines, and for gaugings of the Thames at Teddington Weir, to Mr. C. J. More, the Engineer to the Thames Conservancy Board.

The graphic representation of these collateral observations affords definite evidence as to which of the three conditions, temperature, sunshine and flow of the river, has the predominant influence upon bacterial life in the water. The first diagram (Fig. 13) compares the number of microbes per c.c. with the temperature of the water at the time the sample was taken. The horizontal lines express the numbers of microbes and the temperature, while the vertical lines denote the months when the samples were taken. For obvious reasons, the horizontal lines expressing the numbers of microbes and temperatures, are numbered in opposite directions.

The diagram shows that although coincidence between a high number of microbes and a low temperature are not wanting, some other condition entirely masks the effect, if any, of temperature.

The next diagram (Fig. 14) institutes the comparison between the number of microbes and the hours of sunshine to which the water

has been exposed. The diagram is constructed on the same lines as the first.

It is evident, therefore, from this comparison that, as in the case of temperature, there is some other condition which entirely overbears the influence of sunlight in the destruction of microbes in the river water. This condition is the amount of rainfall higher up the river, or, in other words, the volume of water flowing along the river bed, as is seen from the comparison represented in the next diagram (Fig. 15).

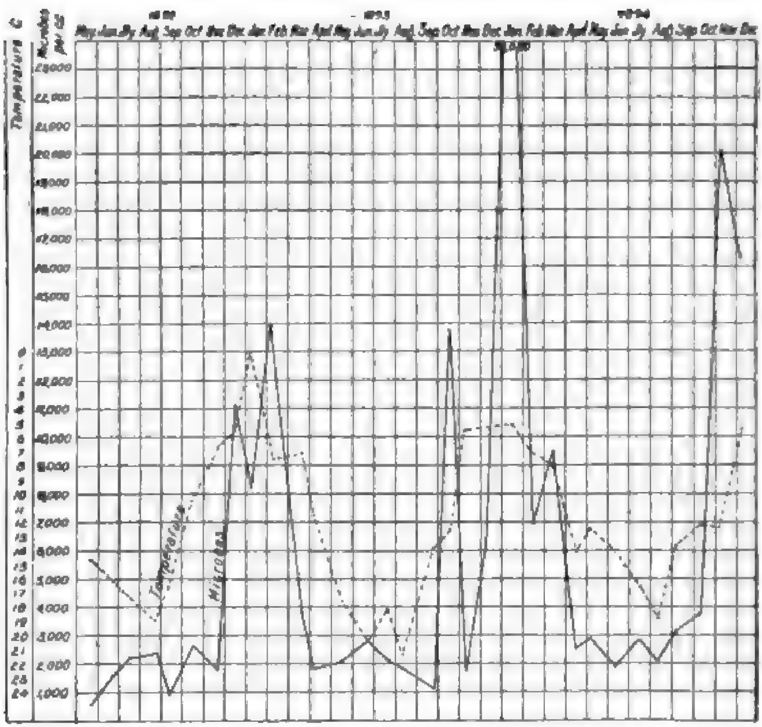

FIG. 13.

This diagram shows very conclusively that the volume of water flowing in the Thames is the paramount influence determining the number of microbes. It compares the volume of water in the river, gauged at Teddington Weir, with the microbes found in the raw Thames water at Hampton on the same day. In this diagram, the numbers representing the flow of the river in millions of gallons per day and the number of microbes per c.c. in the water, both run from the bottom of the diagram upwards.

Comparing the curves in the diagram, it is seen that, with a few exceptions, a remarkably close relation is maintained between them.

The only exception of any importance to the rule that the number of microbes varies directly with the flow of the river, occurring during the thirty-two months through which these observations were continued, happened in November 1892, when the flow increased from 501 millions of gallons in October to 1845 millions in

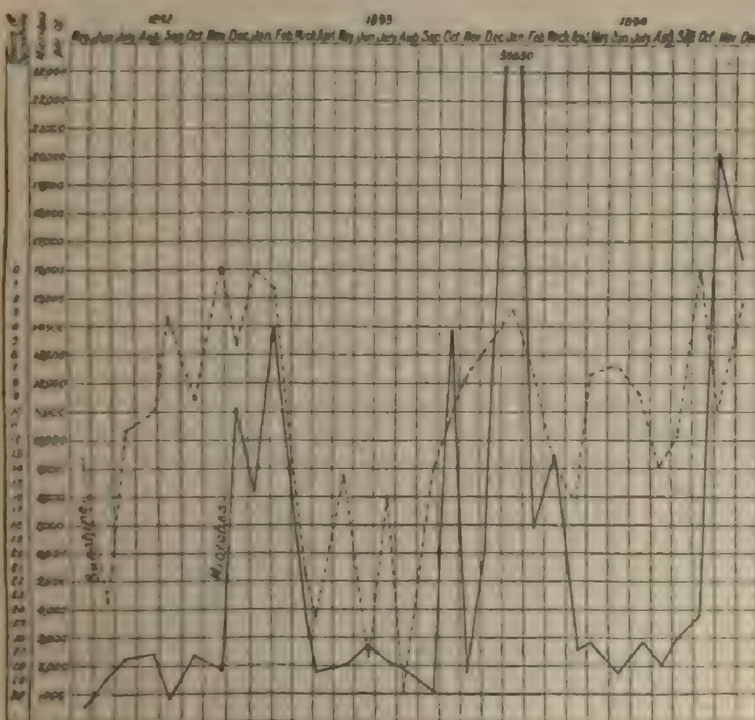

Fig. 14.

November, whilst the microbes actually diminished in number from 2216 to 1868 per c.c. Neither the sunshine nor the temperature records of these two months, however, afford any explanation of this anomalous result: for there was a good deal of sunshine in October before the collection of the sample, and the temperature was higher; whilst in November no ray of sunshine reached the Thames during the three days preceding the taking of the sample, and the temperature was nearly 4° C. lower than in the preceding month. I have

ascertained, however, that the Thames basin had been twice very thoroughly washed out by heavy floods shortly before the time when the November sample was taken, and this affords a satisfactory explanation of the anomalous result yielded by this sample.

These comparisons therefore demonstrate that the number of microbes in Thames water depends directly upon the rate of flow of the river, or, in other words, on the rainfall, and but slightly, if at all, upon either the presence or absence of sunshine

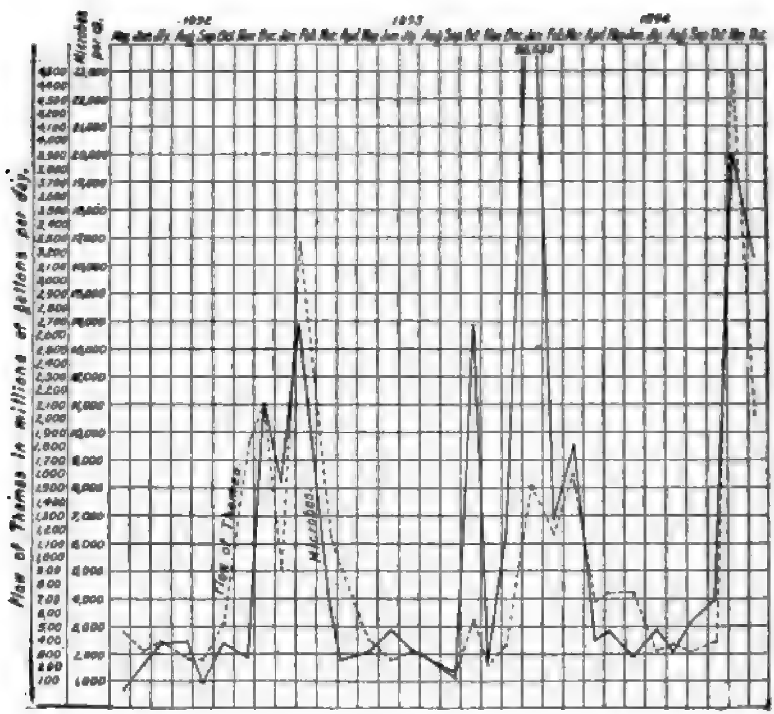

Fig. 15.

or a high or low temperature; and they are confirmed by the continuation of these observations during the year 1895. (See diagram, Fig. 16.)

With regard to the effect of sunshine upon bacterial life, the interesting observations of Dr. Marshall Ward leave no doubt that sunlight is a powerful germicide; still, it is obvious that its potency in this respect must be greatly diminished, if not entirely annulled, when the solar rays have passed through a stratum of water, of even

comparatively small thickness, before they reach the living organisms. By a series of ingeniously devised experiments, Mr. Burgess has demonstrated the correctness of this view.

A sterile bottle, about half filled with Thames water, was violently agitated for five minutes to insure equal distribution of the organisms. Immediately afterwards, a number of sterile glass tubes were partially filled with this water and sealed hermetically. Three of these tubes were immediately packed in ice, and the remainder were attached in duplicate at definite distances apart to a light wire frame which was then suspended vertically in the river. The experiments were made near the Grand Junction Company's intake, at a place favourable for the sun's rays to fall on the river without any obstruction.

The river was, at the time, in a very clear condition and contained but little suspended matter, whilst the day was fine, although clouds obscured the sun occasionally. The tubes were exposed to light in the river for 4½ hours, from 10.30 A.M. to 3 P.M. on May 15, 1895. At the end of this time they were packed in ice for transport to my laboratory, where the cultivation was started immediately. The colonies were counted on the fourth day, and yielded the results given in the following table:—

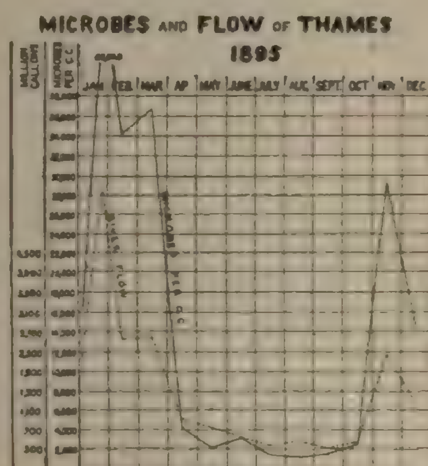

FIG. 16.

	No. of colonies per c.c.
Thames water packed in ice immediately after collection	2127
" " after exposure to sunlight for 4½ hours at surface of river	1140
" " after exposure to sunlight for 4½ hours at 6 in. below surface of river	1940
" " after exposure to sunlight for 4½ hours at 1 ft. below surface of river	2150
" " after exposure to sunlight for 4½ hours at 2 ft. below surface of river	2430
" " after exposure to sunlight for 4½ hours at 3 ft. below surface of river	2140

These experiments show that on May 15 the germicidal effect of sunlight on Thames microbes was *nil* at depths of 1 foot and

upwards from the surface of the comparatively clear water. It cannot, therefore, excite surprise that the effect of sunshine upon bacterial life in the great mass of Thames water should be nearly if not quite imperceptible. We have thus ascertained that sunlight can only kill the germs or microbes near the surface of the water, whilst those at any depth for the most part escape destruction.

On the other hand the enormous effect of floods in augmenting the number of microbes can hardly surprise us; for when a great body of water has flowed over the banks of the river, which are at other times dry and exposed, carrying along with it countless impurities—an effect common both to the main stream and its tributaries—the Thames basin is, as it were, on every such occasion, thoroughly washed out, and it is only to be expected that the number of microbes in the water should be enormously increased, as is found to be the case.

The Water Supply of the Future.

In view of the rapid increase of the population of London, fears have, from time to time, been entertained that the water supply from the Thames basin, that is to say, from the rivers Thames and Lea supplemented by water from springs and deep wells within the basin itself, would soon be insufficient in quantity; whilst the quality of the water taken from the rivers has, up to a comparatively recent date, been considered unsatisfactory. On these grounds various schemes have, from time to time, been brought forward for the supply of the metropolis from other river basins—from the Wye, the Severn, the river basins of North Wales, and of the lake districts of Cumberland and Westmoreland. It is worthy of note, however, that all the Royal Commissions have arrived unanimously at the conclusion, that the quantity of water obtainable from the Thames basin is so ample as to render the necessity of going elsewhere a very remote contingency.

I shall now endeavour to put very shortly before you the facts which, in my opinion, prove that, both as regards quantity and quality, the Thames basin will, for a very long time to come, afford an abundant supply for the metropolis. There is, indeed, no river basin in Great Britain which affords such an abundant supply of excellent water as that available in the Thames basin.

Besides that which flows directly into the river, this water is contained in the chalk, oolite and lower greensand, which are the best water-bearing strata in the kingdom. From these strata it issues in copious springs of unsurpassed organic purity. I have personally inspected every spring of importance in the Thames basin, and have analysed samples of the water. The results, in a very condensed form, are recorded in the following table:—

SPRING AND DEEP-WELL WATERS IN THE THAMES BASIN.

Results of Analysis, in Parts per 100,000.	Oolite. Average of 21 samples.	Lower Greensand. Average of 5 samples.	Chalk.	
			Springs. Average of 8 samples.	Wells. Average of 30 samples.
Total saline matters ..	27·34	18·25	30·14	37·45
Organic carbon	·035	·032	·041	·052
Organic nitrogen	·012	·006	·010	·019
Hardness before boiling	22·5	10·5	25·3	28·0
Hardness after boiling	5·5	3·6	4·9	6·5

Twenty-one samples of oolitic spring water were analysed, and every one of these was of even greater organic purity than the water delivered by the Kent company, which I have always regarded as the standard of organic purity to be aimed at in all other water-works.

Five springs issuing from the lower greensand were examined, and again every one of these was of even greater purity, organically, than the Kent company's water; whilst they were, on the average, only one-third as hard. Forty-six samples of water from the chalk were chemically examined, and these also contained but the merest traces of organic matter.

All these samples from the chalk were derived from sources where the water-bearing stratum is free from a covering of London clay; but as soon as the chalk dips beneath the London tertiary sands and clay, the quality of the water undergoes a remarkable alteration. The total solids in solution are greatly increased in amount, whilst the hardness is much mitigated, owing to the replacement of bicarbonate of lime by bicarbonate of soda. These waters are also of high organic purity; but, as the quantity is very limited, it is useless to dwell upon them. They supply the Trafalgar Square fountains and the London breweries, and we can well afford to leave them to be converted into beer. For dietetic purposes, there is no better water in the kingdom than the underground water of the Thames basin. For *sentimental* reasons, I should like to see it conveyed to the works of the various companies in special conduits; but we have seen that, on *hygienic* grounds, it may safely be allowed to flow down the bed of the Thames, if it be afterwards efficiently filtered.

So much for quality, now as to quantity. The basins of the Thames and Lea include an area of upwards of five thousand square miles. Of this, rather more than one-half, including the oolitic, cretaceous, and portions of the tertiary formations, is covered by a porous soil upon a permeable water-bearing stratum. The remainder is occupied by the Oxford, Kimmeridge, Gault and London clays, being thus covered by a clay soil upon a stiff and impervious subsoil.

The annual rainfall of the district is estimated on an average at 28 inches. The rivulets and streams of the Thames basin are formed and pursue their course on the clay land. There are no streams on the chalk. That which falls upon this porous stratum and does not evaporate, sinks, mostly where it alights, and heaps itself up in the water-bearing stratum below, until the latter can hold no more. The water then escapes as springs at the lowest available points. Innumerable examples of these springs occur all round the edge of the Thames basin, and at various points within it. Thus, from the chalk they are ejected at the lip of the gault, and in the oolitic area, by the fuller's earth below it, or by the Oxford clay geologically above it.

According to the gaugings of the engineer of the Thames Conservancy Board there passed over Teddington Weir in 1892, 387,000 millions of gallons, equal to an average flow of 1060 millions of gallons daily. In the following year, 1893, there passed over Teddington Weir an aggregate of 324,227 millions of gallons, or a daily average of 888 millions of gallons, the average for the two years being 974 millions of gallons; and this number does not include the 120, or 130, millions of gallons daily abstracted by the six London water companies who draw their supplies, wholly or partially, from the Thames.

Thus, in round numbers, we may say that after the present wants of London have been supplied from this river, there is a daily average of a thousand millions of gallons to spare. Surely it is not too violent an assumption to make, that the enterprising engineers of this country can find the means of abstracting and storing, for the necessary time, *one-fourth* of this volume.

As regards the quality of this stored water, all my examinations of the effect of storage upon the chemical, and especially upon the bacterial quality, point to the conclusion that it would be excellent. Indeed, the bacterial improvement of river water by storage, for even a few days, is beyond all expectation, as is proved by the accompanying photographic diagrams. Thus the storage of Thames water by the Chelsea company for only thirteen days, reduces the number of microbes to less than one-eighth of the original amount, as is proved by the photographic diagrams, Figs. 17 and 18. Fig. 17 shows the result of a gelatine plate culture of $\frac{1}{20}$ of a cubic centimetre of unfiltered Thames water collected on January 10, 1896. It gave 11,560 colonies per c.c.; whilst Fig. 18 shows the result of a similar cultivation of $\frac{1}{20}$ of a c.c. of Thames water collected on the same day, after storage for thirteen days. It gave only 1860 colonies per c.c. The storage of the River Lea water for fifteen days, by the East London company, reduces the number from 9240 to 1860 per c.c., or to one-fifth (see diagrams Figs. 19 and 20); and lastly, the water of the New River Cut, containing on the average 4270 microbes per c.c., contained after storage for less than five days only 1810 (see diagrams, Figs. 21 and 22, in which the results of the cultivation of $\frac{1}{20}$ of a c.c. of the water before and after storage are contrasted).

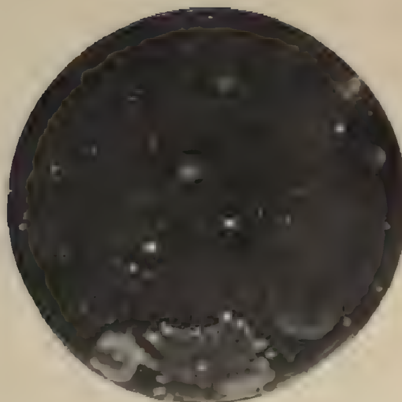

FIG. 17.

FIG. 18.

Fig. 19.

Fig. 20

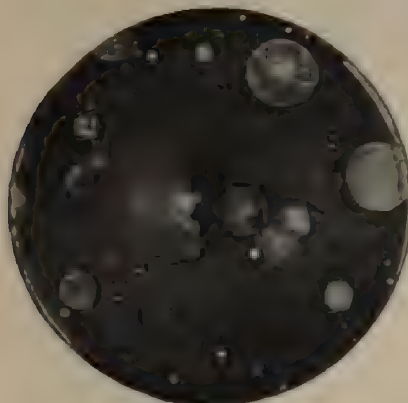

Fig. 21.

Fig. 22

These samples were collected on December 11, 1895.* The reduction here being not so great, partly on account of the shorter storage, but chiefly because the New River Cut, above the point at which the samples were taken, is itself a storage reservoir containing many days' supply. Indeed, quietness in a subsidence reservoir is, very curiously, far more fatal to bacterial life in river-water than the most violent agitation in contact with atmospheric air: for the microbes which are sent into the river above the falls of Niagara by the city of Buffalo seem to take little or no harm from that tremendous leap and turmoil of waters; whilst they very soon almost entirely disappear in Lake Ontario.

Thus it is not too much to expect that storage for, say a couple of months, would reduce the number of microbes in Thames flood water down to nearly the minimum ever found in that river in dry weather; whilst, by avoiding the first rush of each flood, a good chemical quality could also be secured. There is therefore, I think, a fair prospect that the quantity of water derivable from the Thames at Hampton could be increased from its present amount (120 millions of gallons per diem) to 370 millions.

Again, in the River Lea, although here the necessary data for exact calculation are wanting, it may be assumed that the present supply of 54 millions of gallons could be increased by the storage of flood water to 100 millions of gallons per day. To these volumes must be added the amount of deep-well water which is obtainable from those parts of the Thames basin which lie below Teddington Lock, and in the Lea basin below Lea Bridge, and which was estimated by the last Royal Commission appointed to inquire into the water supply of the metropolis, at rather more than 67½ millions of gallons.

Thus we get the grand total of 537½ millions of gallons per day of excellent water obtainable within the Thames basin, the quality of which can be gradually improved, if it be considered necessary, by pumping from the water-bearing strata above Teddington and Lea Bridge respectively, instead of taking the total supply from the open rivers above these points. Such a volume of water would scarcely be required for the supply of the whole water area of London at the end of fifty years from the present time, even supposing the population to go on increasing at the same rate as it did in the decade 1881–91, which is an assumption scarcely likely to be verified.

In conclusion, I have shown that the Thames basin can furnish an ample supply for fifty or more years to come, whilst the quality of the spring and deep-well waters and the filtered river water would

* All the bacteriological illustrations used in this discourse were photographs taken by Mr. Burgess from the actual growths on the gelatine plates; and my best thanks are due to him for the very skilful execution of this difficult and delicate work, involving, as it did in many cases, the watching of the cultivations from hour to hour.

be unimpeachable. To secure these benefits for the future, storage must be gradually provided for 11,500 millions of gallons of water, judiciously selected in the Thames valley, and a proportionate volume in the basin of the Lea; whilst filtration must be carried to its utmost perfection by the use of finer sand than is at present employed, and by the maintenance of a uniform rate during the twenty-four hours.

There is nothing heroic in laying pipes along the banks of the Thames, or even in making reservoirs in the Thames basin. They do not appeal to the imagination, like that colossal work, the bringing of water to Birmingham from the mountains of Wales; and there is little in such a scheme to recommend it to the mind of the ambitious engineers of to-day. Nevertheless, by means of storage, by utilising springs, by sinking deep wells, and by such comparatively simple means, there is, in my opinion, every reason to congratulate ourselves that, for half a century at least, we have at our doors, so to speak, an ample supply of water which, for palatability, wholesomeness, and general excellence will not be surpassed by any supply in the world.

[E. F.]

WEEKLY EVENING MEETING,

Friday, February 28, 1896.

Edward Frankland, Esq. D.C.L. LL.D. F.R.S. Vice-President, in the Chair.

John Murray, Esq. LL.D. Ph.D. D.Sc. F.R.S.

Marine Organisms and the Conditions of their Environment.

The ocean may be divided into two great biological regions, viz. the superficial region, including the waters between the surface and a depth of about 100 fathoms, and the deep-sea region extending from the 100 fathoms line down to the greatest depths. The superficial region may be subdivided into two provinces, viz. the shallow-water or neritic province around the land masses where the depth is less than 100 fathoms, and the pelagic province, embracing the superficial waters of the ocean basins outside the 100 fathoms line; these two provinces contrast sharply as regards physical conditions, which are of great variety in the neritic province, and very uniform over wide areas in the pelagic province.

Temperature is a more important factor in determining the distribution of marine organisms, mostly cold-blooded, than in the case of terrestrial species, mostly warm-blooded and air-breathing animals, the distribution of which depends rather upon topographical features than upon climatic conditions.

A map was exhibited showing the range of temperature in the surface waters of the ocean all over the world, and indicated northern and a southern circumpolar areas with a low temperature and small range (under 10° F.), and an almost circumtropical area with a similar small range but high temperature; in temperate regions the range is greater, the areas of greatest range (over 40° F.) being found off the eastern coasts of North America and of Asia and south of the Cape, due to the mixture of currents from different sources, which sometimes causes the destruction of enormous numbers of marine invertebrates and fishes.

The pelagic tropical waters of the ocean teem with various forms of life, of which probably 70 to 80 per cent. function as plants, converting, under the influence of sunlight, the inorganic constituents of sea-water into organic compounds, thus forming the original source of food of marine animals both at the surface and at the bottom of the sea.

The number of species living in the pelagic waters of the tropics

may greatly exceed the number in polar waters, where, on the other hand, there is often a great development of individuals, so that there is probably a greater bulk of organic matter in the cold polar waters than in the warm tropical waters. The rate of animal metabolism is slower at a low than at a high temperature, and organisms inhabiting tropical waters probably pass through their life-history much more rapidly than similar organisms living in polar regions. Carbonate-of-lime-secreting organisms are most abundant in the warm tropical waters, decreasing in numbers towards the polar regions, and it has been shown that the precipitation of carbonate of lime from solution in sea-water takes place much more rapidly at a high temperature. The pelagic larvæ of bottom-living species are always present in the warm surface waters of the tropics, sometimes growing to an enormous size; but they are absent from the cold polar waters and in the deep sea, where the majority of the bottom-living species have a direct development.

The Arctic fauna and flora, both at the surface and at the bottom, resemble the Antarctic fauna and flora, and a large number of identical and closely-related species are recorded from the two polar areas, though quite unknown in the intervening tropical zone.

The boundary line between the deep-sea region and the neritic province is marked out by what has been called the "mud-line," where the minute organic and inorganic particles derived from the land and surface waters find a resting place upon the bottom, or serve as food for enormous numbers of crustacea, which in their turn are the prey of fishes and the higher animals; this mud-line, in fact, appears to be the great feeding-ground in the ocean, and its average depth is about 100 fathoms along the borders of the great ocean basins.

The majority of deep-sea species are mud eaters; some are of gigantic size; some are armed with peculiar tactile, prehensile, and alluring organs; some are totally blind, whilst others have large eyes and are provided with a kind of dark lantern for the emission of phosphorescent light. The deep-sea fauna does not represent the remnants of very ancient faunas, but has rather been the result of migrations from the region of the mud-line in relatively recent geological times.

The Challenger investigations show that species are most abundant in the shallow waters near land, decreasing in numbers with increasing depth, and especially with increasing distance from continental land.* This is true as a general rule, especially of tropical waters, but in polar regions there are indications of a more abundant fauna in depths of 50 to 150 fathoms than in shallower water under 50 fathoms.†

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* See 'Challenger Reports,' "A Summary of the Scientific Results," by John Murray, pp. 1430–1436, 1895.

† See Murray, "On the Deep and Shallow-Water Marine Fauna of the Kerguelen Region of the Great Southern Ocean," Trans. Roy. Soc. Edin. vol. xxxviii. p. 313, 1896.

The various points touched upon regarding the distribution of marine organisms, might be explained on the hypothesis that in early geological times there was a nearly uniform high temperature over the whole surface of the globe, and a nearly uniformly distributed fauna and flora; and that with the gradual cooling at the poles, species with pelagic larvæ were killed out or forced to migrate towards the tropics, while the great majority of the species which were able to survive in the polar areas were those inhabiting the mud-line. The uniform physical conditions here referred to might be explained by adopting the views of Blandet * as to the greater size and nebulous character of the sun in the earlier ages of the earth's history.

[J. M.]

* Bull. Soc. géol. de France, sér. 2, t. xxv. p. 777, 1868.

GENERAL MONTHLY MEETING,

Monday, March 2, 1896.

Sir James Crichton-Browne, M.D. LL.D. F.R.S. Treasurer and Vice-President, in the Chair.

Herbert John Allcroft, Esq.
R. Lawrence Andrews, Esq.
Ernest Clarke, M.D. B.S. F.R.C.S.
Mayo Collier, Esq. M.B. F.R.C.S.
Henry Ernest Fry, Esq.
Mrs. Francis Gaskell,
Edward Gimingham, Esq.
Alexander Glegg, Esq.
Sir Cameron Gull, Bart. M.P.
Miss Catherine Imray,
Charles W. Keighley, Esq.
Edward Law, M.D. M.R.C.S.
Charles Letts, Esq.
Montefiore Micholls, Esq. M.A.
Reginald Empson Middleton, Esq. M.Inst.C.E.
Alexander Paine, M.D. B.S.
George H. Sykes, Esq. M.A. M.Inst.C.E.
William Lloyd Wise, Esq. J.P.

were elected Members of the Royal Institution.

The following Arrangements for the Lectures after Easter were announced:—

Professor James Sully, M.A. LL.D. of University College, London.—Three Lectures on Child-Study and Education; on Tuesdays, April 14, 21, 28.

C. Vernon Boys, Esq. F.R.S. A.R.S.M. *M.R.I.*—Three Lectures on Ripples in Air and on Water; on Tuesdays, May 5, 12, 19.

Professor T. G. Bonney, D.Sc. LL.D. F.R.S.—Two Lectures on The Building and Sculpture of Western Europe (The Tyndall Lectures); on Tuesdays, May 26, June 2.

Professor Dewar, M.A. LL.D. F.R.S. *M.R.I.*—Three Lectures on Recent Chemical Progress; on Thursdays, April 16, 23, 30.

W. Gowland, Esq. F.C.S. F.S.A. (late of the Imperial Japanese Mint).—Three Lectures on The Art of Working Metals in Japan; on Thursdays, May 7, 14, 21.

Robert Munro, M.D. M.A. (Secretary of the Society of Antiquaries of Scotland).—Two Lectures on Lake Dwellings; on Thursdays, May 28, June 4.

Professor W. B. Richmond, R.A.—Three Lectures on The Vault of the Sixtine Chapel; on Saturdays, April 18, 25, May 2.

F. Corder, Esq. (Curator, Royal Academy of Music).—Three Lectures on Three Emotional Composers—Berlioz, Wagner, Liszt: with Musical Illustrations; on Saturdays, May 9, 16, 23.

Dr. E. A. Wallis Budge, M.A. Litt.D. F.S.A. (Keeper of the Egyptian and Assyrian Antiquities, British Museum).—Two Lectures on The Moral and Religious Literature of Ancient Egypt; on Saturdays, May 30, June 6.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

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WEEKLY EVENING MEETING,

Friday March 6, 1896.

Sir Benjamin Baker, K.C.M.G. LL.D. F.R.S. M. Inst. C.E. Manager, in the Chair.

Alexander R. Binnie, Esq. M. Inst. C.E. F.G.S. *M.R.I.*
Chief Engineer L.C.C.

The Tunnel under the Thames at Blackwall.

The subject of this evening's discourse, the tunnel under the Thames at Blackwall, at once defines and narrows it to an account of the construction of a subaqueous tunnel; and although I shall describe the whole work, yet my remarks will be more particularly directed to that part of the tunnel which is situate under the Thames. A tunnel may be defined as a horizontal or inclined subterranean perforation or boring, generally constructed for the accommodation of a roadway, a railway, or a canal. It will be noticed that I use the word perforation or boring, by which I mean a subterranean excavation carried out in a horizontal or inclined direction underground, either from its two ends or from the bottoms of shafts sunk to the proper depth upon its centre line. I make this definition to prevent confusion with another very similar class of work, to which I shall have to allude, which is constructed by first sinking or digging a horizontal trench to the required depth, in which the roadway is formed and arched over, the excavation or trench afterwards being filled in above it. This mode of construction is termed cut and cover work, and is the way in which the sewers in our streets are generally built, and most of our underground railways were carried out as cut and cover work.

In tunnelling, therefore, at the outset of our description, I wish you to bear in mind that the work divides itself naturally into two main portions: (1) the excavation, digging or blasting of the material to be removed; and (2) into lining or arching in the excavation, so as to prevent the sides, top and bottom from falling in or being pressed upwards by the weight of the superincumbent earth or rock. It will at once be noticed, therefore, that the mode of constructing any particular tunnel will differ very much according to the nature of the material to be excavated, be it rock, clay, gravel, or quicksand, and that in construction the whole work will be rendered much more costly and difficult if it has to be carried through ground

highly charged with water; and when, as in the case of the Blackwall Tunnel, it has to be executed through gravel under a wide river like the Thames, the cost, difficulty and dangers of the work approach the limit of engineering skill to carry it successfully to completion.

It will be at once obvious that if the tunnel is of any considerable size, and the soil to be excavated is of a soft nature such as clay, sand, gravel, or the like, considerable difficulty will be experienced in supporting the face, sides and top of the excavation, before the lining is built into its place. If the work be of small dimensions it is often called a heading, and its top is supported on cross timbers resting on side props. Should it, however, be of larger size, the timbering becomes much more complicated and costly, and requires great skill in its design and management. When, between the years 1818 and 1825, Sir Mark Isambard Brunel was thinking out the mode of constructing the old Thames Tunnel between Rotherhithe and Wapping, he designed several pieces of apparatus, which he termed shields, to obviate the use of all the mass of timber usually required. Some contrivance of this description became necessary, for it was imperative that as far as possible, if settlements or subsidences in the bed of the river were to be avoided, no more material should be excavated than was just required to receive the brickwork of the tunnel. Besides which, it is very problematical if the mode of timbering usually adopted would withstand the varying strains to which it would be subjected under the varying pressures due to the different depths of water at high and low tide. The shield he ultimately adopted was a structure of iron, which could be pushed forward in front of the work as it progressed, a model of which stands on the table. It was so designed as to afford platforms on which the men could work at different levels; it afforded a means of supporting the face and roof during excavation, and a place of safety in the rear of the shield in which the brickwork of the tunnel could be built up, and it could be pushed forward gradually in sections by means of screw-jacks. I have now, I hope, made clear the general subject, and must proceed to the particular work before us to-night.

Position of the tunnel.—During the past ninety years many proposals have been made for crossing the Thames below London Bridge, where the port of London, with its river, ships and docks, forms a barrier to vehicular or pedestrian traffic between its opposite banks. The first work of the kind attempted, but not completed, was Vazes' tunnel at Limehouse, in 1805. We then have Brunel's tunnel, 1825 to 1841. Then the Tower Subway for foot passengers, 7 feet in diameter, carried through the London clay, in 1869–70, by Messrs. Barlow and Greathead. And finally, the late Metropolitan Board of Works obtained an Act in 1887 to construct a tunnel under the Thames at Blackwall, six miles from London Bridge. This tunnel crosses the river 1½ miles below Greenwich and 3 miles above Woolwich, and will bring these growing and populous places into direct communication with Poplar and the East and West India Docks on

the north side of the Thames. The section of the tunnel shows that at this point the river is 1200 feet in width and 46 feet in depth at high water; and borings revealed the fact that although the London clay was present on both banks of the river, yet that the tunnel must pass below it into the sands and clays of the Woolwich series, and for a considerable distance through a bed of gravel which apparently filled an older and deeper river bed. Not only had the river to be passed under, but it will be noticed that it is embanked, and that what were the old marshes on each side are below the level of high water in the river. Further, it will be observed from the section that the soil of these marshes, to a depth considerably below that of low water in the river, consists of vegetable soil, peat, sand and gravel, all of them highly charged with water. Under this is the London clay with its base beds of impure limestone full of fossils, and then the sands, clays, &c., of the Woolwich beds; all these beds were under the full pressure of water due to the varying tidal level, and in their natural state so saturated were some of the beds of sand as to convert them into quicksands.

But undoubtedly the most serious obstacle was the large deep bed of coarse gravel with but little sand. This gravel was open and fully saturated with the river water, and as the bottom of the tunnel was to be 80 feet below high water it was certain that a pressure of about 35 lbs. on the square inch would have to be encountered. This, however, was not the only difficulty, for it was clear that if the water could find an easy entrance to, and flow among the gravel, air would also as easily escape from it. It was obvious, therefore, from the outset that the tunnel would have to be constructed under difficulties never before contended with, either in the construction of Brunel's tunnel at Rotherhithe or elsewhere. Moreover, it was evident that no ordinary mode of tunnelling could be adopted, and that some description of shield would be required. Also, from the difficulties met with by Brunel at the much easier site at Rotherhithe, that some more than ordinary measures would have to be resorted to to keep out the inflow of water in passing under the river and through the gravel bed above referred to. It was consequently determined to use compressed air, as had been first suggested, in his patent of 1830, by Admiral Lord Cochrane (Earl Dundonald), and which had been successfully used under Lake Michigan and the Hudson River at New York, as well as at the tunnel under the Saint Clair River, and on a portion of the City and South London Railway at Stockwell. After consultation with Sir Benjamin Baker and Mr. Greathead, the final design was determined upon, and the contract was let by the London County Council to Messrs. S. Pearson and Son for 871,000*l.* early in 1891.

The whole work is 6200 feet in length; the incline on the south or Kent side of the river is on a gradient of 1 in 36 and has a run of 2168·6 feet; the portion under the river for a distance of 1212 feet is level, and the north or Middlesex incline has a gradient of 1 in 34 for

a length of 2579·6 feet. In other words, the inclined approaches will not be so steep as parts of St. James's Street and Regent Street, and very much less so than the east side of Trafalgar Square opposite Morley's Hotel; they will, however, be about equal to that of the Haymarket. The work may be divided into three portions: open approaches with side walls; cut and cover arched over with brickwork; and tunnel proper composed of cast-iron rings lined with concrete and faced with white glazed tiles, all the other parts of the work being faced with white glazed bricks.

The lengths of the various portions, including the shafts, are as follows:—

	Feet.
Open approach	1735
Cut and cover	1382
Cast-iron tunnel	3083
	6200

or a total of a little over 1 mile.

To facilitate the work, so as to permit of altering its direction, which it would be difficult to do by means of a long curve in a tunnel of this description lined with cast iron, and to secure ventilation, there are four shafts varying in depth from 75 to 98 feet, and having an internal diameter of 48 feet.

The tunnel proper is circular in cross section, 27 feet outside diameter, or 6 feet larger than that of St. Clair (the largest hitherto constructed), built up of fourteen cast-iron segments and a key-piece; each complete ring of segments is 2 feet 6 inches in width. The thickness of the cast iron is 2 inches, the flanges are 12 inches in depth, measured from the outside, and each segment weighs about one ton. The joints are brought to a true and even surface by machine planing, and all are bolted to each other and to the adjacent cast-iron rings by wrought-iron bolts and nuts. To ensure that the cast-iron plates have a firm abutment upon and against the surrounding earth, there is a hole near the centre of each fitted with a screw plug through which grout is forced as will be presently described. The internal edges of the flanges of the plates are recessed for a depth of 2 inches, and after they are fixed in position and bolted together this recess is filled and caulked with rust joint cement composed of iron borings and sal-ammoniac. The space between the flanges and for a distance of 4½ inches beyond in front is filled up solid with Portland cement concrete faced with white glazed tiles, so that the effective diameter of the tunnel is 24 feet 3 inches. Within this the road of 16 feet, with two foot-paths each 3 feet 1½ inches in width is formed, resting on an arched subway 12 feet in width and 5 feet 6 inches in height for the reception of water pipes. There are also proper drains for the road, and channels for smaller pipes for road cleansing, &c. This road of 16 feet will be of the same width as parts of Little Queen Street, Holborn, and King Street, Westminster, and of a greater width than parts of Drury Lane, Fetter lane, Upper and

Lower Thames Street, London Wall, Lombard Street and Threadneedle Street, and as there will be no occasion for stopping at shops, houses and street corners it should be ample for two lines of the largest vehicles. Should the traffic, however, increase beyond the capacity of the tunnel, land has been secured for the construction of another and parallel line of tunnel. The road will be paved with asphalt in the level portion under the river and with granite laid in tar and pitch on the inclined approaches.

The whole work underground will be lighted by three rows of incandescent 32 candle-power electric lamps placed alternately 10 feet apart on the common centre line, no gas being admitted to any portion of the tunnel. The cut and cover portions of the work are formed of brickwork varying in thickness from 18 inches to 2 feet, this is covered with $1\frac{1}{2}$ inches of asphalt and backed with 2 feet of Portland cement concrete, giving a thickness at the thinnest part of 3 feet 6 inches. Internally the cut and cover portions will in all respects resemble that of the tunnel proper formed in cast iron. The open approaches above referred to are flanked with inclined retaining walls, faced with white glazed bricks, carrying a high fence wall with stone coping. At each extremity the tunnel will be approached through an arched gateway supporting the lodge-keeper's house; there will also be stairway access at the junction of the open approach with the cut and cover, as well as stairways down one of the shafts on each side of the river. The shaft near the river on the south side being in private property is domed over and a ventilating chimney carried up from it; the similar shaft near the river on the north side is devoted to administrative and working purposes such as pumping, elevating, lighting, &c. Each shaft is 58 feet outside and 48 feet inside diameter and is formed as it were of two skins of riveted wrought ironwork; the two skins are braced and held together by wrought-iron struts and ties, the space between them being filled in solid with Portland cement concrete. Near the lower extremity of each shaft its walls are perforated by two openings 29 feet 4 inches in diameter. These openings are for the purpose of forming junctions with the tunnel, and were temporarily closed during the time the shafts were being sunk by means of large wrought-iron plugs. At a distance of 8 feet from the bottom the inner skin is bent outwards to join the outer skin and together form a comparatively sharp cutting edge. All the shafts will be lined, when finished, with white glazed brickwork. The shafts were sunk in the following manner. Having been built up to a considerable height above the surface of the ground in the positions they were to occupy, the earth, clay, sand, &c. were excavated within the circumference of the shafts and from below the cutting edge, and as this process of excavation proceeded, the shaft sank into the ground partly by its own weight and in some cases assisted by additional weight placed upon it. When the final level was reached the bottom for a depth of 13 feet was filled in with concrete, in which, and attached to

the walls of the shaft, was fixed a water-tight wrought-iron floor. As the junction between the tunnel and shafts nearest to the river had to be made under compressed air, provision was made for fixing temporary air-tight floors at a level of a few feet above the crown of the tunnel. These air-tight floors were held down by wrought-iron girders 12 feet and 4 feet in depth secured to the sides of the shafts so as to prevent the floors from being blown upwards under an air pressure of 4000 tons.

I think that I have now, in its main outlines, described the principal features of the work, and must proceed to give some account of the mode of its construction. In doing this, I shall have first to describe the shield and then the mode of working it under compressed air. This shield is a structure of steel, cylindrical in shape, 19 feet 6 inches in length and 27 feet 8 inches outside diameter. It is stiffened by two circular partitions 3 feet apart, and its forward or working face, which presses against the material to be excavated, is divided into twelve pockets or cells, by three horizontal and three vertical partitions. It is within these spaces, which are six feet in height, that the men work. Between the two circular stiffening partitions are formed air-locks and shoots for passing out the excavated material. Arranged round the inner circumference of the shield and attached to it and the circular partitions are disposed twenty-eight hydraulic rams 8 inches in diameter, for the purpose of pressing or pushing the shield forward. In the rear of the shield, or that portion of it which faces the completed tunnel, is a space which is merely enclosed within the outer skin of the shield; this space is called the tail of the shield: it always overlaps by 2 feet 6 inches, or one cast-iron ring, the last completed portion of the tunnel, and within it are built up the various rings of iron with which the tunnel is lined. Attached to the back or rearward part of the two circular stiffening partitions and projecting into the tail of the shield are two hydraulic erectors for placing the segments of the rings in position. There are two vertical rams which cause a rackwork to move up or down in a vertical direction. These racks gear into a pinion which carries an arm. Consequently, the vertical motion of the rack causes the arm to move through an arc of a little over 180°. This arm carries another ram by which the arm can be lengthened or shortened as desired. In working, the end of the arm can be attached to the lug or projection cast on the centre of the inner side of each segment, where, by the turning and lengthening motion of the arm, the segment can be placed in any desired part of the ring. The shield, weighing about 250 tons, was built in an excavation at the top of shaft No. 4, and when completed its ends were closed with timber to make it water-tight and it was floated into shaft No. 4, which had been filled with water. The water was then pumped out of the shaft, and as the water fell the shield floating on its surface gradually descended until it rested on the bottom.

As above described it will be noticed that the twelve working

cells or pockets are open in front, and the shield is so used in hard or stiff ground, but in the gravel beds the working face has, except when the excavation is in progress, to be very carefully closed with the wrought-iron shutters secured with screws as shown on the section, the mode of working which will be presently described.

Compressed Air.—We all know that air at the sea-level presses with a force of from 14½ to 15 lbs. per square inch, and can support a column of mercury of from 30 to 31 inches in height. We know that it has bulk, for if we invert a tumbler in a basin of water there will still be a space filled with air into which the water cannot enter. If we try the experiment we shall find that this air space will be larger or smaller depending on the depth to which we immerse the tumbler, consequently we see that air is an elastic body. By properly constructed air-compressing pumps, we can force air down into a diving bell until all the water is expelled from it and the surplus escapes through the open bottom of the bell. If we then measure the amount of compression of the confined air, we shall find it equivalent to the weight of a column of water equal to the area of the open bottom of the bell and as high as the depth of the water.

It having been decided to use compressed air to keep out the water from the tunnel during its construction, the question arose, what, having regard to the health of workers, was the highest pressure which could be adopted with safety, as on this clearly depended the greatest depth to which the bottom of the tunnel and shafts could be carried. In going into the matter, it was evident that it would not be a case of one or two men occasionally going down to perform some temporary work, but that gangs of from sixty to eighty men would have to be kept at work night and day, for many months, consequently a safe maximum had to be arrived at. In places in America, men had worked under a pressure of 48 lbs. per square inch above the atmospheric pressure, that is, 63 lbs. absolute; at Stockwell on the City and South London Railway it was about 15 lbs.; and after many inquiries 35 lbs. per square inch or 50 lbs. absolute was determined upon.

I have stated it in this way because in addition to whatever artificial pressure we may apply, it must be borne in mind that we always have the initial pressure of the atmosphere to work under, which is about 15 lbs. per square inch. In what follows, however, I shall speak only of the artificial pressure, leaving it to be understood that we always have the natural pressure in addition. If the extreme safe pressure be fixed at 35 lbs. per square inch, it follows that the bottom of the tunnel must not go lower than 80 feet below high water mark. This being settled, the next point to be decided was how large could we make a circular tunnel so that it did not project upwards through the gravel into the river. In other words, what was the safe minimum amount of cover that could be allowed over the top of the tunnel and between it and the river bed. This, after much consideration, was provisionally fixed at 6 feet, but in construction,

the least depth was somewhat less. It was due to these considerations coupled with the widths of the busy streets above spoken of and the size of the largest vehicles, such as furniture vans, &c., that the outside diameter was fixed at 27 feet.

Having now described the work and some of the main conditions under which it had to be constructed, you will have noticed that to keep out the water, compressed air is employed, and that to drive the shield forward, hydraulic pressure is used, the machinery for which requires a few words of description. For the purpose of air compression, six steam engines and air pumps are provided, and these are situate on the south bank of the river near shafts Nos. 3 and 4. They have a united capacity of 1500 horse-power, but only about 1000 to 1200 horse-power are used continuously as one engine has to be kept idle in case of accident or breakdown. When working at 1000 to 1200 horse-power, these engines and pumps force into the tunnel about 8000 cubic feet of air per minute, or 17 tons weight per hour. The air from these various engines is first conducted into a wrought-iron reservoir 28 feet in length and 7 feet in diameter, formed like a steam boiler. The first effect of compression, it is needless to say, is to raise the temperature of the air very much, in fact to about 90° or 100° F., consequently before it can be conducted into the tunnel it has to be cooled by passing it through a series of smaller tubes surrounded with cold water like the surface condenser of a steam engine. From the coolers it is led in pipes down shaft No. 4 and along the tunnel through the air-tight bulkhead, presently to be dealt with, to the working face.

In describing the shield, I mentioned that it weighs about 250 tons, and that it has to be thrust forward as the excavation is completed by the twenty-eight hydraulic rams which abut or press upon the last completed ring of the tunnel. To produce the necessary total pressure of about 2800 to 3000 tons an hydraulic pressure up to 2¾ tons per square inch has to be maintained. This is developed by two hydraulic engines of 70 horse-power, and transmitted in pipes down shaft No. 4, along the tunnel and through the air-tight bulkhead to the working face.

I have previously spoken of a certain structure which I have called the air-tight bulkhead. This I must now describe. It is clear that if we are to use the compressed air in the tunnel to press against the working face and keep out the water, it must in some way be confined, or it would rush out backwards and escape up the shaft. To confine the air in the tunnel, temporary air-tight walls or partitions called bulkheads are built across it. As these have to bear an outward thrust or bursting pressure of about 1000 tons, they are formed of massive walls 12 feet in thickness, built of brickwork in Portland cement. It is, however, obvious that they must not be solid but must have means of access formed to allow of entrance and exit both for men and materials. To permit of this access air-locks have to be formed through the bulkhead. These are for

a similar purpose, and act in a like manner, to the locks on a canal. In one case we have to overcome a difference of water-level, and in the other a difference of pressure between that of the ordinary atmosphere outside and the working pressure produced by the air-compressing engines inside the bulkhead, be it 20, 26, or 30 lbs. per square inch. The air-locks consist of wrought-iron cylinders, 15 feet in length and 6 feet in diameter, securely built into the brickwork of the bulkhead. There are two of these air-locks at the level of the road near the bottom, each provided with two doors 5 feet by 4 feet fixed at either end of the lock and opening inwards towards the pressure inside. There is another but smaller air-lock placed near the top of the tunnel to permit of escape in case of accident. Supposing you wish to enter, the outer door is open, but the inner one closed and pressed against by a force of say 30 tons. It is clear that you cannot open this door until you have equalised the pressure on both sides of it. To do this you enter the lock and close the outer door to prevent the escape of air, after which a tap or cock is opened which permits the compressed air from inside to rush into the lock until the pressure within it is equal to that on the inner side of the bulkhead. As soon as this equality is established the inner door can be opened and you step into the working pressure.

Visit to the Tunnel.—In attempting to describe the work of construction, I do not think I can do better than in imagination to conduct you over the work during a visit of inspection. It is first necessary for ladies and gentlemen alike to put on waterproof boots, woollen overalls and caps so as to keep dry and clean; these are in readiness for the purpose at the tunnel. Descending by the steps at the end of the open approach on the south side of the river, we pass for over 300 yards through the finished cut and cover portion of the work and have an opportunity of noticing what will be the general size and appearance of the interior of the tunnel, and that, although it is all below high-water mark and its lower end beneath the level of the bed of the river, yet it is quite dry and dusty under foot. On reaching the bottom of shaft No. 4 the large steam pumps for lifting out the water during construction will be noticed, for it must be remembered that although the work when finished will be quite dry and water-tight, yet during construction, even with the use of compressed air, a large volume of water enters the work and has to be got rid of. This mainly arises from the fact that the difference in hydrostatic external pressure due to the 27 feet in height of the shield amounts to about 12 lbs. per square inch. So that if the full air pressure due to the external hydrostatic pressure at the bottom of the shield and working face were always kept up, it would escape in too large volumes from the top of the excavation and through any porous soil. In fact, to prevent this too rapid escape of air through the gravel, as well as to weight the material over the shield, where the covering was least in thickness above it, clay was deposited in the bed of the river for a width of 150 feet,

and from 10 to 15 feet in depth immediately over the part of the tunnel under construction. While the tunnel was being formed beneath the river there was always a very large escape of air which boiled up through the water, and also came up in some places inland at a distance of 800 feet from the working face. Notwithstanding all the precautions taken the air pressure on two occasions blew up the bottom of the river, and once the surface water rose to a height of 25 feet over a diameter of 50 feet. Any water therefore which enters the tunnel between the working face and the air-tight bulkhead, is forced out through pipes which extend from the working face through the air-tight bulkhead to the bottom of shaft No. 4, the superior air pressure within the working part of the tunnel being used for the purpose; and from the bottom of shaft No. 4 it is raised by steam pumps to the surface. Passing from the bottom of shaft No. 4 down the incline to shaft No. 3, the visitor may observe the cast-iron rings of which the tunnel is built up quite uncovered as the inner lining of concrete has not yet been inserted. It will be noticed that the work is lighted by means of incandescent electric lamps which give sufficient light to see that, as fixed, the plates are quite water-tight and, but for appearance sake, require no internal lining. After passing the bottom of shaft No. 3, which is domed over, we enter on the portion of the tunnel below the river, and most probably soon after hear a loud rumbling roaring noise. This is caused by the escape of the compressed air from one of the air-locks as some men or materials are being locked out. Arriving at the air-tight bulkhead we enter the lock, close the outer door, and turn on the compressed air which enters from the working space beyond the bulkhead. The effect of so doing is at once apparent, for the noise of the inrushing air is as loud as that of the steam escaping from some large steam boiler, and quite drowns the voice and renders hearing impossible. At the same time every one feels a more or less acute pain in the ears caused by the increased pressure of the air on the outer surface of the drum of the ear; this can in most cases be removed by equalising the pressure through the Eustachian tubes which communicate with the middle ear; this is effected by swallowing, and blowing into the nose when it is pinched with the fingers, but if the pain becomes and continues very acute the person suffering should at once leave the air-lock.

As the air in the lock becomes more compressed the temperature rises rapidly; this is due to the compression and only lasts while in the lock, for as soon as equalisation is established and the inner door is opened and you step into the working space you find the temperature falls to about 60° to 65° F. I am often asked what it feels like in compressed air; this I think must in all cases be a personal matter. But summing up the result of my many weekly visits to the tunnel during the past two years, I should say that I feel no difference from that when under the ordinary atmospheric pressure. There is a very slight feeling of exhilaration if the pressure is over 20 lbs. per square

inch, probably caused by the larger amount of oxygen absorbed by the lungs; every one appears to speak with a nasal intonation, you cannot whistle, and the skin acts more freely than at the same temperature under normal conditions. I should here note that no one becomes ill from the effects of compressed air while under its pressure, the baneful effects, if experienced at all, usually show themselves on coming out of it. But I have arrived at the conclusion that among otherwise healthy persons some can and some cannot withstand air pressure, and I have had the pleasure of conducting many persons over the works, from little girls of thirteen up to gentlemen of over seventy years of age, who have not felt the least ill effects from compressed air.

Passing on to the shield and the working face we see the two main operations in progress: (1) excavating; and (2) erecting the cast-iron rings of the tunnel.

Excavation.—As to the excavation, the mode of conducting it depends on the kind of ground being pushed through. If it be hard or stiff enough to stand with a vertical face when pressed against by the various partitions of the shield, the men simply dig or pick it away in front for a few inches or a foot or two, passing the excavated material out to the stage behind the shield, from which it is tipped into wagons and removed. After a sufficient amount has been cleared and loosened in front of the shield, the latter is, by the hydraulic jacks, pressed forward, it may be a few inches or perhaps 2 feet 6 inches, the distance depending on the nature of the ground. Each ring displaces 54 cubic yards, and progress has varied from 1 foot up to 10 feet a day. If, however, the material be gravel the progress is very slow as this material will not remain vertical when dug into, but runs down as fast as it is excavated. Besides which, so rapid is the escape of air that if precautions were not taken it would pass out in dangerous quantities. To obviate this and to support the face, the front of each pocket or working face is closed with three wrought-iron shutters pressed forward by powerful screws, and all the joints luted with clay. In these circumstances the excavation is made either by raking out the gravel through holes in the shutters, or by drawing them back one at a time, digging out a small portion and then screwing forward the shutter again. When all the shutters have been screwed forward the shield is advanced, and as the screws are so arranged as to allow of their slipping through the nuts attached to the shield, the result is that it moves forward past the shutters which remain in the positions into which they have been screwed. It need not be said that this is slow and tedious work requiring great skill and patience.

Erecting the Rings.—After the shield has been pressed forward so as to leave a clear space in the tail of 2 feet 6 inches, the erectors are brought into work, and, as before described, the various segments and the key-piece erected. It will be noticed that as the tail of the shield overlaps the last finished ring of plates it leaves an annular

vacuity, 4 inches in width, between the back of the plates and the natural ground. This space is made solid by the injection of grout under pneumatic pressure by means of the contrivance patented by Mr. Greathead. This consists of a closed horizontal cylinder in which lime or cement can be mixed to the consistency of thick cream by a horizontal spindle with arms which pass through it. The upper side is furnished with a pipe through which air pressure can be applied to the surface of the grout, and from the lower side the grout is conveyed in another pipe to the holes in the plates through which it is forced by the air pressure.

To provide against accidents, two precautions are adopted in case of an inrush of water: one is an elevated temporary wooden gangway or path, extending from the shield to the upper escape air-lock in the air-tight bulkhead, the other is a fixed curtain of wrought iron which descends to the semi-diameter of the tunnel, so that in case of an irruption of water it would not fill the entire tunnel, but a certain portion of compressed air would be trapped between the curtain and the air-lock, and so form a kind of elongated diving bell.

Having now viewed all that is to be seen in compressed air, we return to the air-lock for the purpose of passing out. This is in some respects different from passing in, and is an operation requiring some little time and caution, as the removal of the artificial air pressure and the return to normal conditions is more than equivalent to an ascent beyond the tops of the highest mountains on the earth, as the artificial pressure may be 30 lbs. per square inch, all of which has to be removed before we return to the normal 15 lbs. As to sensation, no difficulties about the treatment of the ears is experienced as the compressed air in the middle ear gradually and naturally discharges itself with a not unpleasant crackling sound. Owing, however, to the expansion of the air in the lock, the temperature falls rapidly, so much so that the invisible aqueous vapour contained in the air is deposited as a thick damp fog, and a chill is experienced; beyond this there is nothing particular to notice. From the tunnel we ascend the shaft No. 4 and in the cabin at the top take a cup of hot coffee, which slight stimulant is sufficient to restore the system to its usual condition.

I am frequently asked if we have found any objects of interest or antiquity in our various excavations. But as most of our work has been through the tertiary beds of the London clay and Woolwich series, nothing but the fossils peculiar to these formations have been met with. On the table will be found specimens of the base bed of the London clay, and of the conglomerate bed which lies just below it. These two formations have also been met with on other works, as at Abbey Mills and the Beckton Gas-works. There is also a specimen of the shelly clay of the Woolwich series. In the superficial gravel, part of an elephant's tusk was found on the south shore of the river; a similar tusk was also found on another work in the gravel beds near Abbey Mills at Stratford. On the north side of the

river at Blackwall Cross, about 8 feet below the street level, a human skeleton was found, and as a stake was also found which appeared to have been driven through the body at the time of burial, in all probability the remains were those of some poor suicide who had been interred with all the superstitious rites of our ancestors. Beyond the above I do not think anything of interest has been discovered.

Experience of Compressed Air.—In some previous works carried out under compressed air, much illness and some deaths have occurred. The symptoms of the more frequent though not serious illnesses are violent and acute pains of a neuralgic kind, generally in the limbs, and which are experienced at the time of, or shortly after coming out of compressed air. The more serious, and in some instances fatal cases took the form of vertigo and paralysis, usually of the legs. Consequently at an early period the London County Council adopted every precaution; they obtained Parliamentary power to compensate persons permanently or temporarily injured, and they appointed a resident medical officer, Dr. Snell, whose duty it was not only to attend to cases of illness, but to see that none but healthy men were allowed on the work, and to keep a watch on all the men employed in compressed air, besides which he was instructed to note from a medical point of view, and make a study of, all the conditions of the problem. We have now been at work under compressed air for about two years, we have had no deaths and only one case of permanent injury (a case of Menier's disease, due to rupture of the semicircular canal of the inner ear). It had often been noticed, on previous works, that illness was most prevalent when the work progressed most slowly, and that it decreased as the progress became more rapid. We now believe, from our experience at Blackwall, that this was due to the larger amount of air pumped down during rapid work. Without for a moment wishing in any way to forestall Dr. Snell, who will no doubt make public the result of his observations at the proper time, we believe that up to a pressure of from 30 lbs. to 35 lbs. per square inch, healthy men can work with almost an entire absence of illness, if a sufficient amount of compressed air, say 8000 to 9000 cubic feet per hour, be supplied to each man.

Conclusion.— In drawing this discourse to a conclusion, I feel that I have but very imperfectly performed the duty which I have undertaken. We have now completed all the work on the south side, the river has been passed, and we are working up the incline near shaft No. 1, and if all goes well we hope to complete the whole by about March next year. In contemplating the work at Blackwall it is interesting to compare the progress in engineering work during the past fifty years. Brunel's tunnel was about the same length as the portion under the river at Blackwall, and it took about nine years, with many long pauses, to complete; the portion of the Blackwall tunnel under the river between shafts 2 and 3 was tunnelled in about thirteen months. The cost of Brunel's tunnel was at the rate

of about 1300*l.* per yard, while that at Blackwall averages 550*l.* per yard. This is most gratifying after the gloomy forebodings by which we were met before we commenced the work. It was at that time predicted, and I was personally warned by members of my own profession, that if we succeeded at all, it would only be by chance, and at the cost of much suffering and death. The success that has attended us is due to all who have been engaged upon the work, and particularly to the skill and untiring energy of three gentlemen, the two resident engineers, Messrs. Hay and Fitzmaurice, and to Mr. Moir, who acts as engineer for, and representative of the contractors, Messrs. Pearson and Son. But in claiming for ourselves at the present time credit for the success that has attended our efforts, we must not forget the honour due to those who have preceded us. No one can in a large and complicated modern work such as I have been describing, claim for himself the exclusive credit for the whole or any important part of it. We have been using a shield, under compressed air, pushed forward by hydraulic power, and at once the names of Bramah, Brunel and Dundonald remind us that we are largely indebted to them. Much has been said and written about the shield we have used, and some names have been associated with its design. I wish it clearly to be understood that no one has any right to do so, as it is a combination of all the good points in many previous efforts in the same direction. But if to any one is due more credit than to another it is to that remarkable genius the elder Brunel, who, although he was himself unable to use his own invention, saw clearly how the work could be best accomplished, and as far back as 1818 took out a patent for a shield and mode of constructing subaqueous tunnels. As described in and shown on the drawings attached to his specification of 1818, we find a cylindrical wrought-iron shield, divided into working cells or pockets, the tail of which overlapped a tunnel some 20 feet in diameter, which tunnel was formed of cast-iron rings, and the whole shield was to be pressed forward by hydraulic jacks. From Dundonald's specification of 1830 we get the mode of making a tunnel under compressed air; and to Bramah is due, in a great measure, the invention of the hydraulic press. Therefore in this as in so many other of our works, it is seen that we owe a deep debt of gratitude to our predecessors for the success we have attained.

[A. R. B.]

WEEKLY EVENING MEETING,

Friday, March 13, 1896.

George Matthey, Esq. F.R.S. Vice-President, in the Chair.

William Samuel Lilly, Esq. M.A. Hon. Fellow of Peterhouse, Cambridge.

The Theory of the Ludicrous.

The feelings aroused by the perception of the Beautiful, the Sublime and the Ludicrous, are referred by modern writers on psychology to the domain of what Kant has taught us to call the Æsthetic. It seems to be pretty generally allowed that the Beautiful attracts without repelling, and affects us with unmingled pleasure in the free exercise of our cognitive faculties; while the feeling of the Sublime is mixed of pleasure and pain, involving, as it does, fear and awe as well as admiration. Regarding the Ludicrous there is much less agreement, and few modern psychologists appear to have made it the subject of profound or far-reaching studies. That is one reason why I have chosen it as my topic to-night. Now in dealing with the Ludicrous, the first thing to be remembered is its vast extent.

Let us look a little at the varieties of it, as that will help us, perhaps, to the theory of which we are in quest. I have thought that it would be well to catalogue them—a thing, so far as I am aware, not previously attempted. My catalogue, which reduces them to twenty-one headings, is as follows:—

1. Humour.
2. Wit.
3. Irony.
4. Satire.
5. Sarcasm.
6. Parody.
7. Bathos.
8. Bulls.
9. Puns.
10. Banter.
11. Caricature.
12. Buffoonery.
13. Mimicry.
14. The Comical.
15. The Farcical.
16. The Burlesque.
17. The Grotesque.
18. Alliteration.
19. Conundrums.
20. Charades.
21. Practical Joking.

Now I am far from asserting that this catalogue is exhaustive, although I have taken a great deal of pains with it, and cannot call to mind any instance of the Ludicrous that may not be brought under one or another of its twenty-one headings, which, I may observe, are, so to speak, mere finger-posts for guidance in a vast and ill-explored

country. Most of them seem so plain and intelligible as to require no discussion. We all know, for instance, what Puns, Charades and Conundrums are. We all know, or may know with a little reflection, what is properly meant by Sarcasm, Banter, Caricature. But there are four varieties of the Ludicrous which seem to present special difficulties. And upon these I must offer a few remarks.

First then in this catalogue of mine stands Humour, which seems to me beyond question the highest manifestation of the Ludicrous. And I do not think we can have a better account of Humour than one given by an admirable writer to whom some of us had the pleasure of listening in this place yesterday afternoon: "That spirit of playing with the vain world and all that therein is, familiar to Socrates, which is always more or less discernible in the highest natures."* The question is often asked, What is the difference between Humour and Wit? A great many different answers have been given, one of the least satisfactory of them, as it seems to me, being Sidney Smith's in the 'Lectures on Moral Philosophy' which he delivered here ninety years ago. I shall return to that presently. For myself I would say, borrowing from the German a distinction now pretty familiar to cultivated people throughout the world, that Wit specially implies Understanding—*Verstand*—while Humour has most in common with Reason—*Vernunft*—in which there is always an element, latent it may be, of tragedy. The greatest humorist in Shakespeare is "the *melancholy* Jacques." And here I am reminded of some words of that most accomplished critic, the late Mr. Walter Pater. In his Essay on Charles Lamb he characterises Wit as "that unreal and transitory mirth which is as the crackling of thorns under a pot," and Humour as "the laughter which blends with tears, and even with the subtleties of the imagination, and which, in its most exquisite motives is one with pity—the laughter of the Comedies of Shakespeare, hardly less expressive than his moods of seriousness or solemnity of that deeply stirred soul of sympathy in him, as flowing from which both tears and laughter are alike genuine and contagious." This is, I think, true as regards Humour, although it hardly does justice to Wit. What Sidney Smith says in his 'Lectures' about Wit and Humour appears to me most unsatisfactory, which is the more surprising since he himself was doubtless one of the wittiest of his generation. Humour, he tells us, consists in "discovering incongruity between ideas which excite surprise, and surprise alone." It is a surprising proposition; but at all events it becomes intelligible when we see what it is that he means by Humour. He gives three instances: A young officer of eighteen years of age coming into company in full uniform, but with a wig on his head, such as was worn at the beginning of this century by grave and respectable clergymen advanced in years; a corpulent and respectable tradesman,

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* Dr. William Barry, the author of 'The New Antigone,' in an Essay on Carlyle.

with habiliments somewhat ostentatious, sliding down gently into the mud, and dedecorating a pea-green coat; and the overturning of a very large dinner table with all the dinner upon it. But these do not appear to me to be examples of Humour at all. My old friend Dr. Kennedy, for many years Regius Professor of Greek at Cambridge, a very dignified and correct person, was dining in the hall of one of the colleges of that University upon some festive occasion, and found himself next to a well-known joker, whose facetiousness, never very refined, grew coarser and coarser as the banquet proceeded, while the Doctor's face grew glummer and glummer. At last the funny man said, "You seem to have no taste for humour, Professor." "Sir," replied the Doctor, much in wrath, "I have a taste for humour, but I have no taste for low buffoonery." Well, what Sidney Smith gives as his first instance of Humour appears to me—to use Dr. Kennedy's expression—low buffoonery; his other two instances I should refer to the category of the Comical. As little can I accept Sidney Smith's account of Wit. "It discovers," he tells us, "real relations that are not apparent between ideas exciting surprise, and surprise only." Surely this will not stand. Consider, for example, the lines of Pope—Hazlitt judged them the finest piece of Wit he knew — on the Lord Mayor's Show, and the Lord Mayor's Poet Laureate.—

> "Now night descending the proud show is o'er,
> But lives in Settle's numbers one day more."

What discovery is there here of real but not apparent relations between ideas producing surprise, and surprise only? Or take the lines — far wittier I think than these—of Pope's Epistle to Dr. Arbuthnot. He is speaking of certain bad poets:—

> "He who still wanting, though he lives on theft,
> Steals much, spends little, yet has nothing left:
> And he who now to sense, now nonsense leaning,
> Means not, but blunders round about a meaning;
> And he whose fustian 's so sublimely bad,
> It is not poetry but prose run mad."

Surely the Wit here does not lend itself to Sidney Smith's explanation. But as I have ventured thus to criticise this gifted man's definition of Wit, perhaps I ought to offer for your criticism a definition of my own. I should say, then, that Wit consists in the discovery of incongruities in the province of the understanding (*Verstand*), the distinctive element which it leaves out being the element of reason (*Vernunft*).

I am equally dissatisfied with Sidney Smith's account of another variety of the Ludicrous, namely, the Bull:—"A Bull," he tells us, "is the exact counterpart of a Witticism, for as Wit discovers real relations that are not apparent, Bulls admit apparent relations that

are not real." I do not think Bulls necessarily do that. When Sir Boyle Roche told the Irish House of Commons that he wished a certain bill, then before that august assembly, at the bottom of the bottomless pit, he certainly produced a Bull, and a very fine one; but as certainly his aspiration does not admit apparent relations that are not real. It appears to me that a Bull may perhaps be defined—in so difficult and subtle a matter I don't like to dogmatise—as a contradiction in terms which conveys a real meaning. I observe in passing—and I hope I may not in so doing seem to be lacking in justice to Ireland—that the claim sometimes made on behalf of that country to a sort of monopoly of Bulls is untenable. Excellent Bulls are produced by people of other countries; as, for example, by the Austrian officer, mentioned by Schopenhauer, when he observed to a guest staying in the same country house, "Ah, you are fond of solitary walks, so am I; let us take a walk together:" or by the Scotchman who told a friend that a common acquaintance had declared him unworthy to black the boots of a certain person, and who in reply to his remark, "Well, I hope you took my part," said, "Of course I did, I said you were quite worthy to black them:" or again, by a well-known English judge, who when passing sentence on a prisoner convicted on all the counts of a long indictment, observed, "Do you know, sir, that it is in my power to sentence you for these many breaches of the laws of your country, to a term of penal servitude far exceeding your natural life."

There is yet another variety of the Ludicrous, upon which I should like to say a few words—Parody. A Parody is a composition which sportively imitates some other composition. I suppose that, in the majority of cases, the object, or at all events, the effect of the imitation is to cast a certain amount of ridicule upon the original. "What should be great you turn to farce" complains the honest farmer to his wife, in Prior's amusing poem, 'The Ladle.' Well, it must be confessed that this is what a Parody too often does. But this need not be so. A Parody must necessarily be sportive, or it would not belong to the great family of the Ludicrous; but the laughter, or the smile, which it excites need not be at the expense of the composition imitated. Pope speaks of his imitation of one of the 'Satires' of Horace as a Parody: but the laugh which he raises does not fall upon Horace. So, you will remember, in the 'Dunciad' he most effectively parodies certain noble lines of Denham's 'Cooper's Hill'—lines addressed by that poet to the river Thames:—

> "O could I flow like thee, and make thy stream
> My great example, as it is my theme!
> Though deep yet clear, though gentle, yet not dull,
> Strong without rage, without o'erflowing full."

Fine verses, indeed, are these: perhaps the finest example of that strength with which Pope, in a well-known line, rightly credits

Denham. And, assuredly, Pope by no means intended to ridicule them, when he addressed the unhappy Welsted:—

> "Flow, Welsted, flow, like thine inspirer Beer;
> Though stale, not ripe; though thin, yet never clear;
> So sweetly mawkish, and so smoothly dull;
> Heady, not strong; o'erflowing, though not full."

So much must suffice regarding the four varieties of the Ludicrous, which seem to me to present special difficulties. What I have said may serve to show how wide and varied its range is, and how many things have to be thought of and taken into account before we can even attempt to frame a theory of it. But, indeed, that is not all. The matter is further complicated by national differences. This is especially so in the case of Humour. Spanish Humour, for example—its chief monument is, of course, *Don Quixote*—differs very widely from all other. It is impossible to conceive of that marvellous book as being written out of Spain, not merely on account of its local colouring, but also, and far more, on account of its ethos, its *indoles*. Pope, in dedicating to Swift the 'Dunciad,' writes:—

> "Whether thou choose Cervantes' serious air,
> Or laugh and shake in Rabelais' easy chair."

The lines are singularly infelicitous. The Castilian gravity of Cervantes is one thing. The British gravity of Swift is quite another. Nor is there much in common between Rabelais and Swift. Rabelais is the supreme example of what Renan has called "the old Gallic gaiety"—it seems now well nigh extinct in France—in its moods of wildest and most unrestrained extravagance. Swift, "bitter and strange," is ever sober, ever holds himself in hand. Rabelais! Yes: we picture him to ourselves in his easy chair, laughing consumedly, quaffing his cup of good old wine to warm his good old nose, and ministered to, like Falstaff, "by a fair hot wench in a flame-coloured taffeta." Swift's most outrageous utterances are delivered with all the solemnity—I think this has been remarked by Taine—of a clergyman discoursing in his gown and bands. I can only glance at this subject of the difference in the Humour of different races. It is too large, and would want a lecture, or rather a book, to itself, for any adequate treatment. But, before I pass on, I should like to observe how distinctly a thing *sui generis* American Humour is. It is, I think, the only intellectual province in which the people of the United States have achieved originality. I cannot here enter upon an analytical and comparative examination of it. I suppose its peculiar charm lies in its homely and fresh grotesqueness. The dryness and crispness of the American climate seem to have passed into it. Lowell is unquestionably one of its chief masters.

"Parson Wilbur sez he never heerd in his life
That th' Apostles rigged out in their swaller-tail coats,
And marched round in front of a drum and a fife,
To git, some of 'em office and some of 'em votes;
But John P.
Robinson, he
Sez they didn't know everything down in Judee."

Artemus Ward, another great master of American humour, has not surpassed this. But I think he has equalled it: as, for example, in his account of his visit to Brigham Young:—

"You are a married man, Mr. Young, I bleeve," says I, preparing to write him some free parsis.

"I've 80 wives, Mr. Ward. I sertinly am married."

"How do you like it as far as you hev got?" said I.

He said, "Middlin."

But the American newspapers, even the humblest of them, constantly contain things just as good. A correspondent the other day sent me some obscure journal, published in the far West, I think, wherein I found a story which strikes me as so superlatively excellent a specimen of American humour that I shall venture to read it to you. It is called, "A Cool Burglar, Too."

"I think about the most curious man I ever met," said the retired burglar, "I met in a house in Eastern Connecticut, and I shouldn't know him either if I should meet him again, unless I should hear him speak; it was so dark where I met him that I never saw him at all. I had looked around the house downstairs, and actually hadn't seen a thing worth carrying off, and it wasn't a bad looking house on the outside, either. I got upstairs, and groped about a little, and finally turned into a room that was darker than Egypt. I hadn't gone more than three steps in this room when I heard a man say, 'Hello, there.'

"'Hello,' says I.

"'Who are you?' said the man, 'burglar?'

"And I said yes, I did do something in that line occasionally.

"'Miserable business to be in, ain't it?' said the man. His voice came from a bed over in the corner of the room, and I knew he hadn't even sat up.

"And I said, 'Well, I dunno; I've got to support my family someway.'

"'Well, you've just wasted a night here,' said the man. 'Didn't you see anything downstairs worth stealing?'

"And I said no, I hadn't.

"'Well, there's less upstairs,' says the man, and then I heard him turn over and settle down to go to sleep again. I'd like to have gone over there and kicked him. But I didn't. It was getting late, and I thought, all things considered, that I might just as well let him have his sleep out."

And now having thus taken, so to speak, a bird's-eye view of the vast domain of the Ludicrous, let us go on to inquire if we can arrive at any true theory about it. Can we define the Ludicrous? Is there a Ludicrous in the nature of things—an Objective Ludicrous, as well as a Subjective Ludicrous? In other words, what is the Ludicrous in itself, and what is it to us? And what is the faculty which comprehends and judges the Ludicrous? These are questions which confront us when we seek to deal with the matter philosophically. And

they are questions which it is far easier to ask than to answer. Plato, in the 'Philebus,' tells us "the pleasure of the Ludicrous springs from the sight of another's misfortune, the misfortune, however, being a kind of self-ignorance that is powerless to inflict hurt." A certain spice of malice, you see, he held to be of the essence of this emotion. Well, that may be so. It is always perilous to differ from Plato. But certainly his account is inadequate, as, indeed, is now pretty generally allowed. Far profounder is the view expounded by Aristotle, here, as in so many provinces, "the master of them that know." "The Ludicrous," he tells us in 'The Poetica,' "is a defect of some sort (ἁμάρτημά τι) and an ugliness (αἶσχος), which is not painful or destructive." These are words which, at first, may not seem very enlightening. But, as Professor Butcher admirably remarks, in his edition of 'The Poetica,' we cannot properly understand them without taking into account the elements which enter into Aristotle's idea of beauty. And when we have done that, we shall find that we may extend their meaning so as to embrace "the incongruities, absurdities, or cross purposes of life, its imperfect correspondences or adjustments, and that in matters intellectual as well as moral." Aristotle's view of the Ludicrous appears to be, in fact, something out of time and place without danger, some error in truth and propriety, which is neither painful nor pernicious. The treatment of the Ludicrous by the schoolmen is worth noting, as indeed is their treatment of every question to which they have applied their acute and subtle intellects. Their philosophy goes upon Plato's notion of ideals or patterns in the divine mind, compared with which individuals, both in themselves and in their relations with one another, fall short of perfection. This deficiency, they teach, when not grave enough to excite disgust or indignation, is the ground—the *fundamentum reale*—of our subjective perception of the Ludicrous. I believe I have looked into most of the modern philosophers who have dealt with this matter, and I do not think that, with one exception—to be presently dwelt upon—they take us much beyond the ancients and the schoolmen. Of course we have attained to a clearer perception of its physical side. And here we are indebted to Mr. Herbert Spencer for an explanation, which, so far as I can judge—and that is not very far—may very likely be true. This is the substance of it. "A large amount of nervous energy, instead of being allowed to expend itself in producing an equivalent amount of the new thoughts and emotions which were nascent, is suddenly checked in its flow." "The excess must discharge itself in some other direction, and there results an efflux through the motor nerves to various classes of the muscles, producing the half-convulsive actions we term laughter." I dare say Mr. Spencer may be right in the hypothesis he here presents. But I am sure he is wrong if he supposes that those "nervous discharges," of which he speaks, are the primary or the main element in the emotion of which laughter is an outward visible sign. That emotion begins with a mental act. A-

Lotze well puts it in his 'Microcosmos,' "The mechanism of our life has annexed the corporeal expression to a mood of mind produced by what we see being taken up into a world of thought, and estimated at the value belonging to it in the rational connection of things." Of course, the corporeal expression is not *necessarily* connected with the mood of mind. The physical phenomenon which we call laughter may be produced by purely physical means, for example, by titillation. The laugh of the soul and the laugh of the body are distinct. We may have each without the other. And only a gross and superficial analysis will confound them.

But, as I intimated just now, there is one modern philosopher who appears to me to have given us a satisfactory formula of the Ludicrous. That philosopher is Schopenhauer, unquestionably one of the most profound and penetrating intellects of this century, however we may account of his system as a whole. One of his cardinal doctrines is that all abstract knowledge springs from knowledge of perception, and obtains its whole value from its relation to perception. And upon this doctrine he hangs his theory of the Ludicrous. "The source of the Ludicrous," he teaches, "is always the paradoxical, and therefore unexpected, subsumption of an object under a conception which in other respects is different from it." Or, as he elsewhere in his great work, writes more at large:—

"The cause of laughter, in every case, is simply the sudden perception of the incongruity between a concept and the real objects which by means of it we have thought in a certain association, and laughter itself is the expression of this incongruity. Now incongruity occurs in this way: we have thought of two or more real objects by means of one concept, and have passed on the identity of the concept to the objects. It then becomes strikingly apparent, from the discrepancy of the objects, in other respects, that the concept applies to them only from one point of view. It occurs quite as often, however, that the incongruity between a single real object and the concept under which from one point of view, it has rightly been subsumed, is suddenly felt. Now the more correct the subsumption of such objects under a concept may be from one point of view, and the greater and more glaring their incongruity from another point of view, the stronger is the ludicrous effect which is produced by this contrast. All laughter, therefore, springs up on occasion of a paradoxical and unexpected subsumption, whether this is expressed in words or actions."

Now, I believe this account to be, in the main, correct. It is, in substance, the thought of Aristotle, but it brings in the element of paradox, unexpectedness, suddenness, which is lacking in that philosopher's definition. And it is cast into an accurate and scientific form. "The source of the Ludicrous is always the paradoxical, and therefore unexpected, subsumption of an object under a conception which, in other respects, is different from it." Yes; I think that this is true. Every instance of the Ludicrous, in its twenty-one varieties, which I have been able to call to mind, fits in with this formula. But there are two points in Schopenhauer's exposition to which I must demur. In the first place, I do not think him well warranted in affirming—as he does—that his theory of the

Ludicrous is inseparable from his particular doctrine of perceptible and abstract ideas. And therefore it is not necessary for me, on the present occasion, to enter upon an examination of that doctrine; of which I am heartily glad, for to do so, even in briefest outline, would take up far more time than is left of my hour. Besides, I hate talking metaphysics after dinner, and I fancy very few people really like hearing metaphysics talked at that period of the day. Again, Schopenhauer certainly uses unguarded and too general language when he tells us that *all* laughter is occasioned by the paradoxical, and therefore unexpected, subsumption of an object under a conception which in other respects is different from it. The phenomenon of laughter may be due to a variety of causes. It may be due to merely physical causes, as I pointed out just now. It may be due to quite other mental causes than paradoxical and unexpected subsumption. Paradoxical and unexpected subsumption is not the explanation of the heavenly laughter of which Dante speaks in the twenty-seventh canto of the 'Paradiso'—the laughter of Beatrice, "so gladsome that in her countenance God himself appeared to rejoice."

> "Ma ella che vedeva il mio disire
> Incominciò, ridendo, tanto lieta
> Che Dio parea nel suo volto gioire."

It is not the explanation of what is called fiendish laughter, laughter *propter malitiam*, the outcome of mere malice—the sort of laughter which, by the way, one of his critics has attributed to Schopenhauer himself; the laugh of a demon over the fiasco of the universe. It is not the explanation of that ringing laugh of pure human happiness which one sometimes hears from the lips of young girls; is there any music like it? They laugh as the birds sing. Nor is the laughter of women at their lovers—a common phenomenon enough—always to be referred to the paradoxical and therefore unexpected subsumption of an object under a conception which in other respects is different from it. It is far oftener the expression of mere triumph. "The outburst of laughter," Dr. Bain truly tells us in his 'Mental and Moral Science,' "is a frequent accompaniment of the emotion of power." But it is sometimes a manifestation of pain too deep for tears. This is the laughter of which Antigone speaks: Ἀλγοῦντα μὲν δῆτ' εἰ γέλωτ' ἐν σοὶ γελῶ—"I laugh in sorrow if I laugh at thee." That laugh of sorrow—so piercing and pathetic!—who does not know it? Surely it is the saddest thing in the world. Lastly, not to continue unduly the enumeration, laughter is very often the expression of mere mental vacuity. I remember a gentleman who was fond of relating utterly imbecile stories concerning himself, the invariable ending of them being, "And then I roared." We gave him the name of the Roarer, and fled at his approach as we would have done from a ramping and roaring lion. But I am quite sure his laughter was not due to the paradoxical, and therefore unexpected, subsumption of an object under a conception which in other respects was different from it. No; his

was the inane laughter which Cicero justly calls the most inane thing in the world—*nam risu inani nihil est inanius*.

With these reservations, then, I think we must admit Schopenhauer's theory of the Ludicrous. It is true as far as it goes. I use these words of limitation, because it does not attempt to answer the deeper questions connected with the subject which I mentioned just now. Perhaps they are unanswerable. Certainly the few minutes left to me will not suffice even for the most superficial examination of them. I would rather employ those minutes for another and more practical purpose—an Englishman is nothing if not practical. We have seen that the Ludicrous is the paralogical, and therefore unexpected, subsumption of an object under a conception which, in other respects is different from it. Well, but what is the function of the Ludicrous in human life? What end does it serve? Please note that this question is quite congruous with the title of my lecture: for in order really to know anything, we must know its end: according to that profound saying of Aristotle, ἡ δὲ φύσις τέλος ἐστί.

I observe, then, that a sense of the Ludicrous is the most sane thing we have. Incorrectness and abnormality are the notes of the Ludicrous. And they provoke me to affirm—*ridentem dicere verum*—what is correct and normal. We may say then, that the Ludicrous is an irrational negation which arouses in the mind a rational affirmation. And so, in strictness, a sense of the Ludicrous cannot be attributed to animals less highly evolved than man in the scale of being, because, though they have understanding, they have not, properly speaking, reason: they have knowledge of perception: they have not abstract knowledge. Still, in this province, as elsewhere, we may observe among them what Aristotle calls μιμήματα τῆς ἀνθρωπίνης ζωῆς: mimicries of the life of man. As in the most favoured individuals of the higher species of them there appear analoga of the operations of reason, so do we find also indications of the lower kinds of the Ludicrous: farce, buffoonery, practical joking. But, indeed, there appear to be whole races of men—the North American Indians and the Cingalese Veddas, for example—that are destitute of the sense of the Ludicrous. And, in the higher races this sense is by no means universally found. The richest intellects possess it in amplest measure. The absence of it is a sure indication of mental poverty. "Here comes a fool, let's be grave," said Charles Lamb on one occasion. And, I remember a friend of my own observing of a somewhat taciturn person whom we had met, "He must be a man of sense, for, although he said little, he laughed in the right place." That laugh is a manifestation of intellectual abundance or exuberance: it is something over and above the actual work of life. And so we may adapt to our present purpose certain words of Schiller's in his "Letters on Æsthetic Education": "Man sports (*spielt*) only when he is man in the full signification of the word, and then only is he complete man (*ganz Mensch*) when he sports."

I need hardly observe how impressive this faculty of the Ludicrous

may be abused. There is nothing more diabolical—in the strictest sense of the word—than to turn into ridicule "whatsoever things are true, whatsoever things are honest, whatsoever things are just, whatsoever things are pure, whatsoever things are lovely, whatsoever things are of good report." There is no more detestable occupation than that of "sapping a solemn creed with solemn sneer." But it is a maxim of jurisprudence, *Abusus non tollit usum*. And this holds universally. No; the abuse of the Ludicrous does not take away its uses. Those proper, healthy and legitimate uses are obvious. And very few words will suffice for such of them as I can here touch on. Now one office of the Ludicrous is to lighten "the burden and the mystery of all this unintelligible world." Beaumarchais has indicated it in his well-known saying: "I make haste to laugh at everything for fear of being obliged to weep." I remember a story of the late Lord Houghton meeting some obscure author who had given to the world a play, and exclaiming, with his usual bonhomie, "Ah! Mr. So-and-So, I am so glad to make your acquaintance: I remember reading your tragedy with great interest." "Tragedy!" the other explained in dismay: "no, no; it was a comedy." "God bless my soul," Houghton replied, "I thought it was a tragedy; please forgive me." Well, "life's poor play" is tragedy or comedy, as you take it. It is best not to take it as tragedy, at all events too habitually. A certain novelist, I forget who, says of a certain lady who adorns his pages, I forget her name, that on a certain occasion, I forget what, "not knowing whether to laugh or cry, she chose the better part, and laughed." It *is* the better part. And one office of Humour—to speak only of that variety of the Ludicrous—is to show us the folly of quarrelling with such life as we have here. Ah, it is so easy to strip off the illusions of human existence! And so foolish! Yes; and may we not add, so ungrateful? For, assuredly, the Almighty Hand which has hung the veil of Māya over the darker realities of life, was impelled by pity for the "purblind race of miserable men." Illusions! what would the world be without them? And it is the function of the humourist to teach us to enjoy them wisely; to lead us to make the most of life's poor play, while it lasts; which assuredly we shall not do if we are for ever examining too curiously the tinsel and tawdry which deck it out, if we are for ever thinking of the final drop of the curtain upon "the painted simulation of the scene," and the extinguishment of the lights for ever. *Memento mori* is undoubtedly a most wholesome maxim. So is *Disce vivere*. "Ah, mon enfant," said the old priest, touching lightly with his withered hand the blooming cheek of the young girl, too vain of her pretty face, "Ah, mon enfant, tout cela pourrira." "Oui, mon père," she replied, naively, "mais ce n'est pas encore pourri." Well, they were both right, the sage confessor and the silly coquette. And we may learn a lesson from them both. There is an admirable saying of Joubert, "L'illusion et la sagesse réunies sont le charme de la vie et de l'art."

But again, the Ludicrous has a distinct ethical value. Aristotle places *εὐτραπελία* among the virtues, and by *εὐτραπελία* he means decorous wit and humour, as distinguished from the low buffoonery to which Dr. Kennedy so strongly objected. It is said that ridicule is the test of truth. And there is a true sense in the saying. The Platonic irony—which is really the feigning of ignorance in order to get a man to make a fool of himself—may illustrate this. And, to look at the matter from another point of view, it may be seriously maintained that we never really believe a thing until we are able to treat it sportively. The more profound our wisdom, the more lightly we shall wear it. It is a tradition of the Catholic Church, in her colleges and seminaries, that all ethical questions should be dealt with humorously. The Professor of Moral Philosophy in those institutions is "der Lustige," as the Germans would say: the man who does the comic business. Carlyle, in one of his early Letters, speaks of a sense of the ridiculous as "brotherly sympathy with the downward side." It is a most pregnant saying. "Twenty-seven millions, mostly fools." Well, better to view them as fools than as knaves. For the emotion raised by folly is rather pity and ruth than anger. Then again, the Ludicrous, and especially the variety of it which we call Satire, is an inestimable instrument of moral police. I do not say of moral reformation. What moral reformation really means is the conversion of the will from bad to good. And I do not think Satire, as a rule, likely to effect that. But it is certainly a most effective deterrent. Goethe makes Werther, as the supposed author of the 'Letters from Switzerland,' say, "One would always rather appear vicious than ridiculous to any one else." And I suppose this is true of the vast majority of people. Hence it was that Pope was led to magnify his office:—

> "Yes, I am proud, I must be proud, to see
> Men not afraid of God, afraid of me:
> Safe from the Bar, the Pulpit and the Throne,
> But touched and scared by ridicule alone."

But the clock, which beats out the little lives of men, has beaten out the brief hour of the lecturer. And so with these noble lines of the great ethical poet of the last century, I take my leave of my subject and my audience.

[W. S. L.]

WEEKLY EVENING MEETING,

Friday, March 20, 1896.

Sir James Crichton-Browne, M.D. LL.D. F.R.S. Treasurer and Vice-President, in the Chair.

Professor Thomas R. Fraser, M.D. LL.D. F.R.S.

Immunisation against Serpents' Venom, and the Treatment of Snake-bite with Antivenene.

From a remote period of antiquity, there has been enmity between the human race and serpents, and, in a literal sense, man has bruised the head of the serpent, and the serpent has bruised the heel of man. This long-continued feud has not resulted in victory for either side. Venomous serpents still annually destroy the lives of tens of thousands of human beings, and, in self-defence, tens of thousands of serpents are annually slain by man.

The progress of knowledge has greatly increased the means for protecting mankind against the death-producing effects of many diseases; yet, although these means have been liberally employed in the contest against venomous serpents, none of them have hitherto been found sufficient.

The reality of the contest is appreciated when we find pervading medical literature from its earliest beginnings—from the time of Pliny and Celsus—to the present time, disquisitions on the treatment of the bites of venomous serpents, and lengthy descriptions of the numerous remedies, organic and inorganic, that have been used for this purpose. Although extended experience and the application of the scientific methods of the present day, have resulted in showing that each of these remedies had been recommended on insufficient grounds, we may hesitate in pronouncing their recommendation to have been premature, in view of the impossibility of waiting, in the presence of imminent dangers, until accurate demonstration has been obtained by the usually tardy and laborious processes of science.

Let me pause here for a few minutes to indicate the practical importance of a scientific demonstration of the value of any remedy that is used in the treatment of snake-poisoning.

When a serpent inflicts a wound, I need scarcely say that it is not the wound, but the venom introduced into it which causes the symptoms of poisoning, and the death that may result. This venom is now known to be a complex mixture, containing several non-poisonous as well as poisonous substances. The latter are not

ferments, and have no power of reproducing themselves in the body, but they are substances that produce effects having a direct relationship to the quantity introduced into the body. This quantity in the case of each serpent varies with its size and bodily and mental condition; with the nature of the bite—whether both fangs or only one has been introduced, whether they have penetrated deeply or only scratched the surface; and with other circumstances related to the serpent, such as whether it had recently bitten an animal or not, and thus parted with a portion or retained the whole of the venom stored in the poison glands.

A bite may, therefore, result in very little danger, or it may be rapidly fatal; but, in order to produce death, there must have been introduced into the tissues at least a certain quantity of venom, which is spoken of as the minimum-lethal quantity or dose. The minimum-lethal quantity for the animal bitten, again, is different for different species of animals, and different also for different individuals of the same species, the chief cause of difference between adult animals of the same species being the body weight of the individual, the quantity required to produce death being very exactly related to each pound or kilogramme of weight.

If even a minute fraction below the minimum-lethal has been introduced into the tissues by an effective bite, death will not follow, although serious and alarming symptoms will be produced of exactly the same kind as those which follow a bite which terminates fatally.

How then can we be assured, in any case of snake-bite in man, that a quantity of venom sufficient to produce death has been introduced? It is impossible to answer this question except by the result. If a quantity less than the minimum-lethal has been introduced, although the gravest symptoms may be produced, the patient will recover, whatever remedies are administered, provided, obviously, that the remedies have not been so injudiciously selected or used that they themselves, and not the insufficient quantity of venom, produce a fatal termination. The recovery of a patient after the introduction of less than the smallest quantity of venom capable of producing death, has thus too often been attributed to the remedies that have been administered; and, consequently, as, indeed, is exemplified in the treatment of many diseases, a large number of substances have acquired an unjust reputation as antidotes. The list of antidotes has, accordingly, become a very large one; but when their pretensions have been subjected to sufficient tests, the verdict is that all of them are valueless to prevent death when even the smallest quantity of venom required to produce death has been received by an animal.

Without entering into details, I will content myself with reproducing the opinion of Sir Joseph Fayrer, that, "after long and repeated observations in India, and subsequently in England, I am forced to the conclusion that all the remedies hitherto regarded as antidotes are absolutely without any specific effect on the condition produced by the poison."

But while medical practice and science, in each period of its

development, has thus failed to protect man against this ancient enemy, legendary traditions, the tales of travellers and of residents among nations and tribes existing outside of the civilisation of the time, at least suggest that, by means apart from the use of remedies, some measure of success may actually have been obtained.

Many of these legends and statements are probably of great significance, and, in connection with facts derived from experiment, which to-night I have to describe, they possess a deep interest.

We learn from these legends that from a remote period of time the belief has existed that a power may be acquired by man of freely handling venomous serpents, and even of successfully resisting the poisonous effects of their bites.

The Psylli of Africa, the Marsi of Italy, the Gouni of India, and other ancient tribes and sects, were stated to have been immune against serpents' bites, and this immunity has been explained on the supposition that serpents' blood was present in the veins of the members of these tribes and sects.

In more modern times and, indeed, at the present day, the same belief is expressed in the writings of many travellers. In 'A New and Accurate Description of the Coast of Guinea,' by William Bosman, published in 1705, an account is given of the great "reverence and respect" of the negroes for snakes, worshipped by them as gods; in connection with which the following statements are made. "But what is best of all is that these idolatrous snakes don't do the least mischief in the world to mankind; for if by chance in the dark one treads upon them, and they bite or sting him, it is not more prejudicial than the sting of millipedes. Wherefore the natives would fain persuade us that it is good to be bitten or stung by these snakes, upon the plea that one is thereby secured and protected from the sting of any poisonous snake" (p. 379).

At Southern Africa, the Rev. John Campbell, in 1813, observed that it was "very common among the Hottentots to catch a serpent, squeeze out the poison from under his teeth, and drink it. They say it only makes them a little giddy, and imagine that it preserves them afterwards from receiving any injury from the sting of that reptile" (p. 401).

Drummond Hay, in his work on Western Barbary, published in 1844, gives a description of the performances by members of a sect of snake-charmers, called the Eisowy, who freely handled, and allowed themselves to be bitten by serpents proved to be venomous by a rapidly fatal experiment performed on a fowl. At the termination of the exhibition, the Eisowy, apparently as a usual part of the performance, "commenced eating or rather chewing" a poisonous snake, "which, writhing with pain (to quote Mr. Hay's words), bit him in the neck and hands until it was actually destroyed by the Eisowy's teeth." He states that, on another occasion, at Tangier, a young Moor, who was witnessing the performance of a snake-charmer, ridiculed his exhibition as an imposture, and having been dared by the Eisowy to touch one of the serpents, the lad did so

was bitten by one of them, and shortly afterwards expired. In connection with my subject, a special interest is attached to the account given by Mr. Drummond Hay, and repeated in its main features by Quedenfeldt in the 'Zeitschrift für Ethnologie' of 1886, of the origin of this Eisowy sect, and of the immunity which they claim. The founder, Seedna Eiser, was being followed through the desert of Soos by a great multitude, who, becoming hungry, clamoured for bread. On this, Seedna Eiser became enraged, and turning upon them he uttered a common Arabic curse, "Kool sim," which means "eat poison." So great was their faith in the teaching of the saint, that they acted upon the literal interpretation of his words, and thereafter ate venomous snakes and reptiles; and from that time they themselves and their descendants have been immune against serpents' bites (p. 65).

Dr. Honigberger, in his 'Thirty-five Years in the East,' published in 1852, relates the incident of a faqueer who was bitten by a serpent, and to whom he at once sent medicines which he judged likely to prevent the ill-effects of the venom. "On the same afternoon," he writes, "I visited him and found him in good spirits. I at first attributed the circumstance to the effect produced by the remedies I had sent him, but was surprised on hearing that he had not taken them, he being of opinion that the venom of the serpent was incapable of affecting him, inasmuch as he had often been bitten by serpents without having sustained any injury." On the suggestion of the faqueer, the same serpent, which had been caught and retained, was allowed to bite him again, and afterwards to bite a fowl. This fowl was taken home by Dr. Honigberger, and he found it dead on the following morning, "although the faqueer, who was bitten first, was quite well" (p. 135).

Nicholson, in his work on 'Indian Snakes' (1875), and Richards, in his 'Landmarks of Snake-poison Literature' (1885), also narrate instances, the latter with obvious disbelief in their reality, suggesting that snake-charmers may possess some means for protecting themselves against the bites of venomous serpents.

Many other examples might be quoted in which this suggestion is made. The attention which has been drawn to the subject during the last twelve months has prompted the publication of other instances, such as that related by Dr. Bawa, of a Tamil snake-charmer who, in the course of his performances, was bitten by a cobra without any effect, while an onlooker, foolishly repeating the performance, was bitten by the same cobra, and died in three hours; and the description given by M. D'Abbadie, in a recent issue of the Comptes rendus, of the custom, recently prevailing at Mozambique, of inoculating with serpents' venom, under the firm conviction that protection is thereby produced against the effects of serpents' bites.

It may be instructive to associate with these statements the belief that venomous serpents are themselves protected against the effects of bites inflicted upon them by individuals both of their own and of other species. On mere anatomical grounds, it is difficult to

understand how serpents could escape the absorption of their own venom through mucous surfaces, even admitting that absorption of venom does not occur in normal conditions of these surfaces. Venom must, however, be so frequently introduced into their bodies, in situations where absorption could not fail to occur, by the bites inflicted upon them by other serpents, that the conclusion seems inevitable that they possess some protective quality, without which, probably, no venomous serpents would now be in existence. Not only have many general observations been made in favour of this belief, but it has been supported by direct experiments, such as those made by Fontana of Tuscany more than a century ago, and by Guyon, Lacerda, Waddell, Kaufmann, and Sir Joseph Fayrer.

This, and other evidence, pointing to the existence of protection against venom, not only in serpents themselves, but also, in certain exceptional circumstances, in human beings, several years ago originated a wish to investigate the matter. It was obviously suggested that if protection occurs, it must be caused by some direct result of the absorption of venom; and, therefore, that its existence could be proved or disproved by experiment. In the former event, the first steps would already have been taken to obtain, by further experiments, results likely to be of value in the treatment of poisoning by serpents' venom, and, indeed, likely to be of suggestive importance in even the wider field of general therapeutics.

The general plan to be followed in the first stages of the investigation was obviously suggested by some of the statements I have reproduced; for they indicate that individuals might become accustomed to, or protected against the effects of serpents' bites, by the introduction into their bodies of a succession of doses of venom, no one of which, necessarily, at the beginning of the process, was so large as the minimum-lethal. A consideration also of the facts, proving the possession of protection on the part of venomous serpents themselves, indicated the same plan of procedure; for, equally obviously, these serpents, from an early period of their existence, must absorb venom from their own gradually-developing poison-glands, until, in the course of time, they had acquired sufficient protection to remain unaffected by the larger quantities which the now fully-developed glands would introduce into their bodies.

My first supplies of cobra venom were obtained in 1869, from the late Dr. Shortt, of Madras, and in 1879 from Surgeon-Colonel Moir, of Meerut. They were in very small quantity, but with them I was able to satisfy myself that, by a succession of minute doses, animals became able to receive the minimum-lethal dose without any distinct injury. At this point, however, the supply of venom failed, and the observations could not then be carried further. It became evident that until large quantities of venom had been obtained, definite results could not be hoped for.

It was not until several years afterwards that a sufficient supply had been gradually accumulated, by further small quantities received from Sir Joseph Fayrer, the Thakore of Gondal, and Dr. Phillips;

and by larger quantities from Sir William Mackinnon, Director-General of the Army Medical Department, and especially from Surgeon-Colonel Cunningham, of Calcutta, who for many years has been engaged with much success in the study of venoms and their antidotes. Within the last few months, and subsequently to the publication of some of the experimental results which had by this time been obtained, the India Office has also placed at my disposal a considerable quantity of venom, which had been collected by Dr. Hankin, of Agra, at the request of Dr. Cleghoorn, Surgeon-General with the Government of India.

But, besides these specimens of the venom of the cobra of India, I have also been fortunate in obtaining specimens of venoms from other parts of the world.

From America, Dr. Weir Mitchell, of Philadelphia—whose work on the chemistry and physiology of serpents' venom constitutes the great advance of the century on the venom of viperine serpents—has supplied me with the venom of three species of rattlesnakes, viz. *Crotalus horridus*, *C. adamanteus*, and *C. durrisus*, and also with a specimen of the venom of the Copper Head (*Trigonocephalus contortrix*).

From Australia, Dr. Thomas Bancroft, of Brisbane, has at various times sent specimens of the venoms of the black snake (*Pseudechis porphyriacus*), the brown snake (*Diemenia superciliosa*), and of a large unidentified snake of the Diamantina district of Queensland (probably a new species of Diemenia).

From Africa, the kindness of Mr. Andrew Smith, a distinguished naturalist of Cape Town, of Dr. Brook, of the Orange Free State, and of Dr. John Murray and Mr. Van Putten, of Cape Colony, has placed at my disposal small quantities of the venom of the puff adder (*Vipera arietans*), the night adder (*Aspidelaps lubricus*), the yellow cobra (*Naja haie*), and the "Ring Hals Slang" or "Rinkas" (*Sepedon hæmachates*).

In the meantime, however, the results of experiments on the inoculation of the toxines of diseases, as well as of proteid toxines of vegetable origin, had suggested to several observers that serpents' venom, because of its chemical analogies with several of these substances, might possibly be found capable, like them, of producing immunity against the effects of poisonous doses; and further important evidence has thus been obtained in favour of the reality of the protection to which I have referred.

Sewall, in 1886, undertook an investigation with the object of determining if immunity against the fatal effects of rattlesnake venom could be produced by the inoculation of repeated doses, each too small to produce ill-effects. The experiments were made on pigeons, and he succeeded in proving that immunity could be secured to the extent, at least, of protection against seven times the minimum-lethal dose. Kanthack made a similar series of experiments in 1891, which allowed him to conclude that rabbits may be accustomed to resist lethal doses

of cobra venom. Working with the venom of vipers, Kaufmann in 1891, and Phisalix and Bertrand in 1893, obtained experimental evidence of the possibility of producing a definite, though not high degree of resistance against the toxic effects of this venom. In the following year, Calmette, continuing some earlier observations which had led him to express the opinion that protection against snake venom could not be produced, published evidence confirming the results of previous investigators, but also showing that a higher degree of protection could be secured than they had obtained, for he succeeded in administering to each of several rabbits, within a period of eight months, a total quantity of from 30 to 35 milligrammes of venom.

In 1894, also, both Phisalix and Bertrand and Calmette obtained evidence of the power of the blood-serum of protected animals to counteract the effects of venom. Calmette at the same time claimed that hypochlorite and chloride of calcium were antidotes of considerable value; and in a later publication, he showed that the blood-serum of animals immunised by the administration of venom possesses a certain degree of antidotal efficacy against the toxines of several diseases.

In the case of many of the venoms which I have had the good fortune to obtain, the quantity at my disposal was not sufficient for experimental examination on the plan that seemed desirable, and, besides, the examination of each of them would require several months of work. The venoms that have as yet been used are four in number, those namely of the cobra of India (*Naja tripudians*), of the *Crotalus horridus* of America, of a large colubrine snake, probably a species of *Diemenia* from Queensland, Australia, and of the *Sepedon haemachates* of Africa. They are, therefore, those of the most deadly of the poisonous serpents of Asia, America, Australia and Africa respectively; and, further, they are representative of the chief differences that occur in the composition and action of venoms, for they are derived from members of the two great groups of the colubrine and viperine serpents. My supply of cobra venom, however, being much larger than that of any of the others, this venom was chiefly used in the experiments.

An essential preliminary to exact investigations with active substances must always be the determination of the activity of the substances. The only convenient method for doing this is to define the smallest dose capable of producing death for any given weight of animal—that is, the minimum-lethal dose. The venoms in their natural liquid state are unstable, and they are also inconstant in activity, mainly because of variations in the quantity of the water which they contain. Dried venoms have therefore been used in all the experiments. The cobra venom has, however, nearly always been received in the form of a dry solid; but when this was not so, it has been dried *in vacuo* over sulphuric acid.

Experiments were made with it on several animals—as the frog,

guinea-pig, rabbit, white rat, cat, and the innocuous grass snake of Italy (*Tropedonotus natrix*). Very considerable differences were found to occur in the minimum-lethal dose for each of these animals. For the guinea-pig, the minimum-lethal dose per kilogramme was ·00018 grm.; for the frog, ·0002 grm.; for the rabbit, ·000245 grm.; for the white rat, ·00025 grm.; for the cat, somewhat less than ·005 grm.; and for the grass snake, the relatively large dose of ·03 grm.* Cobra venom thus takes a position among the most active of known substances, rivalling in its lethal power the most potent of the vegetable active principles, such as aconitine, strophanthin or acokantherin.

These facts having been ascertained, attempts were next made to render animals proof against lethal doses, by administering to them a succession of gradually increasing non-lethal doses. These were, for the first few doses, in some of the experiments, one-tenth of the minimum-lethal, in others one-fifth, in others one-half of the minimum-lethal, and in others almost as great as the minimum-lethal. At varying intervals the doses were repeated, and by-and-by gradually increased, until the actual minimum-lethal had been attained. The subsequent doses by gradual increments exceeded the minimum-lethal, and after five or six times the minimum-lethal had been reached, it was found that the increments could be increased so that each became twice, four times, and latterly even five times the minimum-lethal, and still the animal suffered little, and, in many cases, no appreciable injury.

This brief statement, however, does not represent the experimental difficulties that were encountered. It describes the course of events in the altogether successful experiments. Non-success, however, was frequent, and many failures occurred before experience indicated the precautions and conditions that are necessary for success.

Serpents' venom exerts what may broadly be described as a duplex action. It produces functional disturbances unassociated with visible structural changes, and it also produces obvious structural changes. The latter are of a highly irritative character, causing intense visceral congestions in the lungs, kidneys, and other organs, and when the venom is given by subcutaneous injection, on all the structures of the skin and subjacent parts. There are apparently also some definite changes produced in the blood, with regard to which several important facts have been discovered by Dr. Martin, of the University of Sydney, and by Surgeon-Colonel Cunningham, of Calcutta. Irritative effects are obviously produced by cobra venom, even in non-lethal doses, and with greatly increased virulence by doses that exceed the

* Guinea-pig, nearly	1/5 millig.	Kitten (6 weeks),	2 millig.
Frog,	1/5 "	Cat,	5 "
Rabbit, nearly	1/4 "	Grass snake,	3 centig.
White rat,	1/4 "		

minimum-lethal; but, in respect to this action, the other three venoms used are greatly more active than the venom of the cobra. Evidence was obtained to indicate that in the process of immunisation a diminution occurs in the intensity of these local actions; but this diminution does not proceed so rapidly as that in the unseen functional or other changes which are the more direct causes of death; and, further, the local irritative changes, after having been produced, are slower to disappear than the unseen functional disturbances. Until these facts had been appreciated, and, indeed, even with the adoption of precautions suggested by them, frequent failures occurred. The apparently contradictory results, accordingly, were obtained of the production, by gradually increasing doses, on the one hand, of a protection against quantities much above the minimum-lethal, so perfect that no apparent injury was caused; and, on the other hand, when the intervals of time separating successive doses had been too brief, of an intolerance so decided that death was produced by the last of a succession of gradually increasing doses, no one of which was so great as the minimum-lethal. The latter unfortunate event was frequently displayed in frogs and guinea-pigs, and attempts to carry immunisation in them to a high point usually resulted in failure.

Notwithstanding these difficulties, however, such gratifying results have been obtained as that rabbits could at last receive, by subcutaneous injection, so much as ten, twenty, thirty, and even the remarkable quantity of fifty times the minimum-lethal dose, without manifesting any obvious symptoms of poisoning.

Almost the only observable phenomena were a rise in the body temperature, which continued for a few hours after the injection, and which contrasts with the fall that occurs after the administration of even non-lethal doses, in non-protected animals; and a loss of appetite, which usually, though not invariably, occurred, and was probably the cause of a temporary fall in weight during the day or two days succeeding each injection. On the other hand, during the process of successful immunisation, the animals increased in weight, fed well, and appeared to acquire increased vigour and liveliness.

It is marvellous to observe these evidences of the absence of injurious effects, and even of the production of benefit in an animal which, for instance, has received in one single dose a quantity of venom sufficient to kill, in less than six hours, fifty animals of the same weight, and in the course of five or six months a total quantity of venom sufficient to destroy the lives of 370 animals of the same species and weight (Fig. 1, overleaf).

With the cobra venom I have also immunised cats and white rats, both by subcutaneous and by stomach administration; but the significance of the latter method of administration will be afterwards considered. A horse has also been immunised; and I have to express my obligations to Principal Williams and Prof. W. Owen Williams for granting me the accommodation of their establishment, and to

Mr. Davis, also of the New Veterinary College, for much valuable assistance.

Following the same plan of research with the three other venoms, it was found that for rabbits the minimum-lethal dose per kilogramme of the Diamantina venom is 0·015 grm.; of the venom of *Sepedon hæmachates*, ·0025 grm.; and of the venom of Crotalus, ·004 grm.* The Crotalus venom was, in its purity, altogether comparable with the cobra venom; and the determinations, therefore, show that cobra venom is sixteen times more powerful than Crotalus or rattlesnake venom. This venom, as well as the two others, however, much exceed cobra venom in the intensity of their local action. When death is produced by Crotalus venom, the subcutaneous tissues become

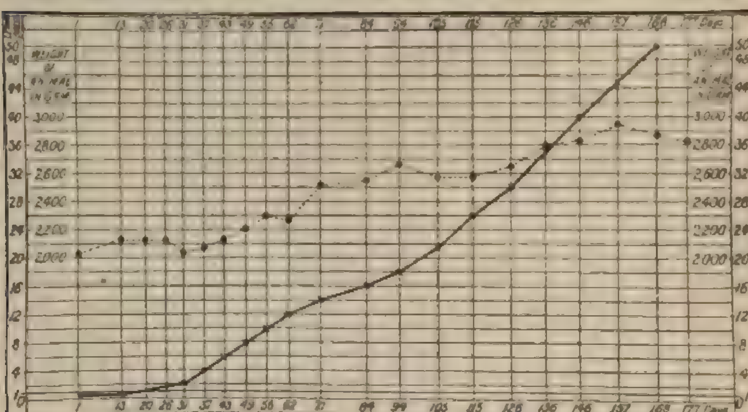

FIG. 1.—Immunisation of a rabbit against 50 times the minimum-lethal dose of cobra venom. The crosses connected by the continuous line represent administrations of venom. The dots connected by the interrupted line represent the weights of the animal.

extensively infiltrated with a large quantity of blood and of blood-stained serum, the underlying muscles are reduced to an almost pulpy blood-stained substance, and decomposition occurs very soon after death. Similar changes in the subcutaneous tissues, but to a rather less degree, are caused by the Diamantina venom, and in addition, hæmaturia, or more probably hæmoglobinuria, was invariably produced by lethal and by large non-lethal doses. I mention these circumstances to indicate the perfection of the protection which is produced by the administration of successive gradually increasing doses; for they can be so adjusted that a dose of the Diamantina

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* Diamantina venom,	1½ milligramme.	
Sepedon hæmachates,	2½	„
Crotalus horridus,	4	„

venom, even fifteen times larger than the minimum-lethal, may be administered without producing more than an inconsiderable degree of local destructive effect.

Experiments have also been made by which it has been demonstrated that when an animal has acquired a resistant power over the minimum-lethal dose of one venom, that animal is also able successfully to resist the lethal action of a dose above the minimum-lethal of other venoms. To a rabbit protected against cobra venom, a dose above the minimum-lethal of Sepedon venom has been administered; to rabbits protected against Crotalus venom, doses above the minimum-lethal of Diamantina and of cobra venoms have been given; to rabbits protected against the Diamantina venom, doses above the minimum-lethal of Crotalus and Sepedon venoms have been given; and in each case the animal has recovered, and but few symptoms of injury were produced. At the same time, in other experiments, indications were obtained that animals protected against a given venom are capable of resisting the toxic effect of that venom more effectually than the toxic effect of other venoms.

The experiments have not yet proceeded sufficiently far to show for what length of time the protection conferred by any final lethal dose may last. It has been discovered, however, that protection lasts for at least a considerable period of time, even when the last protective dose has not been a large one. For example, to a rabbit which had last received four times the minimum-lethal dose of cobra venom, twice the minimum-lethal dose was administered thirty-four days subsequently; while to another rabbit, which had last received twice the minimum-lethal dose of Crotalus venom, the same dose of this venom was administered twenty days subsequently, and in each case the second dose failed to produce any toxic symptom.

Having thus succeeded in producing a high degree of protection in animals against the toxic effects of serpents' venom, the blood-serum of these animals was, in the next place, collected for the purpose of testing its antidotal properties. In this portion of the investigation, the method followed was essentially the same as that described in a communication made by me to the Royal Society of Edinburgh in 1871, on "The Antagonism between the Actions of Physostigma and Atropia," as it appeared to be the most direct method for obtaining accurate knowledge of the value of an antidote.

A few preliminary experiments were, however, early made with the serum of animals in whom the protection had not been carried to a high degree, and they were sufficient to show that antidotal properties are possessed even by this serum. It soon became apparent that in order to obtain some reasonable approximation to constancy in the conditions of the experiments, it was necessary that the serum should be in such a state that it would remain unchanged during at least several weeks. It was found that this could be insured, without any appreciable loss of antidotal power, by drying the freshly-separated serum in the receiver of an air-pump over sulphuric acid.

A perfectly dry and easily pulverisable solid is thus obtained from which a normal serum can readily be prepared as required, by dissolving a definite quantity of the dry serum in a definite quantity of water. The dry substance is on the average equivalent to about one-tenth of the weight of the liquid serum. I have found that, without any special precautions, it retains its antidotal power unimpaired for at least a year, and it is probable that it may be kept unchanged for an unlimited period of time.

To this antidotal serum, whether in the dry form or in solution, I have given the name "Antivenene," a name which, notwithstanding etymological objections, has the advantages of brevity and freedom from ambiguity.

The experiments now to be described were made with antivenene derived from a horse which had last received a dose of cobra venom estimated to be twenty times the minimum-lethal. On some previous occasions I have stated the results of observations on the antidotal value of the blood-serum of rabbits which had last received thirty and fifty times the minimum-lethal, respectively. The antivenene obtained from cats and white rats has also been examined. The special interest, however, is attached to antivenene derived from the horse, that it is more likely than any others to be used in the treatment of snake-bite in man.

The experiments were so planned as to obtain in different conditions of administration as exact a definition as possible of the antidotal power of the antivenene. In the meantime, four series of experiments have been undertaken on rabbits. In one series the venom was mixed outside of the body with the antivenene, and immediately thereafter the mixture was injected under the skin of the animal; in the second series the venom and antivenene were almost simultaneously injected into opposite sides of the body; in the third series the antivenene was injected some considerable time before the venom; and in the fourth series the venom was first injected, and thirty minutes afterwards the antivenene.

In the experiments of the *first series*, the doses of cobra venom administered were the minimum-lethal, one-and-a-half the minimum lethal, twice, thrice, four times, five times, eight times and ten times the minimum-lethal. In the case of each dose of venom, experiments were made with different quantities of antivenene, until the smallest quantity required to prevent death was discovered. In order to render it certain, in this and in the other series, that a lethal dose had been administered in the experiments with the so-called minimum-lethal, the minimum-lethal indicated by previous experiments was not used, but instead of it a slightly larger dose (·00025 instead of ·00024 gramme per kilogramme).

When this certainly lethal dose, capable of producing death in three or four hours, was mixed with the antivenene, and the mixture injected two minutes afterwards, under the skin, it was found that so small quantities were sufficient to prevent death as ·001 c.c.,

·0008 c.c., ·0005 c.c., and ·0004 c.c. (1/1000, 1/1500, 1/2000, and 1/2500 of a c.c., for each kilogramme of the weight of animal; with ·0003 c.c. (1/333) per kilogramme, however, the animal died. The antivenene was therefore found to be so powerful as an antidote, in the conditions of these experiments, that even the 1/2500 part of a cubic centimetre, equivalent to about the one-hundred-and-fiftieth part of a minim, acted as an efficient antidote, while even with the one-two-thousandth part of a cubic centimetre not only was death prevented, but there was almost no symptom of poisoning produced. In the experiments of this series with one-and-a-half the minimum-lethal dose, recovery occurred when the doses of antivenene were ·32 c.c., ·3 c.c., ·28 c.c., ·25 c.c., and ·24 c.c. per kilogramme; but ·23 c.c. and ·2 c.c. failed to prevent death. In the experiments with twice the minimum-lethal dose, recovery occurred when the doses of antivenene were ·5 c.c., ·4 c.c., and ·35 c.c.; but ·3 c.c. and ·2 c.c. failed to prevent death. In the experiments with thrice the minimum-lethal dose, a dose capable of producing death in less than two hours, recovery occurred when the doses of antivenene were ·7 c.c. and ·65 c.c.; but death occurred with ·6 c.c., ·55 c.c., and 5 c.c. With four times the minimum-lethal dose, recovery occurred with 1·5 c.c., 1·3 c.c., and 1·2 c.c., and death with 1 c.c. With five times the minimum-lethal dose, recovery occurred with 2·5 c.c., 2·2 c.c., 2 c.c., 1·8 c.c., and 1·5 c.c.; but death with 1·3 c.c. With eight times the minimum-lethal dose, recovery occurred with 2·6 c.c. and 2·5 c.c.; but death with 2·4 c.c., 2·3 c.c., and 2 c.c. And even the enormous dose of ten times the minimum-lethal failed to produce death, or any important symptoms, when it had previously been mixed with 3·5 c.c. and 3·4 c.c. of antivenene for each kilogramme of animal; and it only succeeded in producing death, although not until the lapse of several hours, when the doses of antivenene were 3·3 c.c., 3·2 c.c., ·3 c.c., and 2·5 c.c. per kilogramme.

These results show a remarkable, an almost directly proportional accordance in the increment required in the dose of antivenene for each increment in the dose of venom. In the diagram, the comparatively straight direction of oblique line separating the fatal from the non-fatal experiments is noteworthy, considering that the conditions of the experiments, in regard both to the animals and the substances used, could never be absolutely the same. Indeed, from twice the minimum-lethal dose of venom upwards, the addition of little more than ·3 c.c. per kilogramme represents the addition in the quantity of antivenene required for each addition of a minimum-lethal dose of venom. Apparently the antivenene is able in this proportion to prevent death from almost any lethal dose of venom, however large it may be (Fig. 2, overleaf).

These results are in marked contrast with those that occur when an antidote acts because of its physiological properties, and they alone suggest that the antidotism is rather the effect of a chemical than of a physiological reaction. The indications obtained with

doses of twice the minimum-lethal and upwards cannot, however, be carried down to the minimum-lethal dose. The quantity of antivenene required to prevent death from this dose is much less than might have been anticipated when the results of experiments with larger doses are considered. Thus, it appears that while ·35 c.c. of antivenene per kilogramme is required to prevent death from twice the minimum-lethal of venom, the minute quantity of the 1/2500th of a c.c., or nearly 1000 times less (·0004 as compared with ·35 c.c.), is sufficient to prevent death from a little more than the minimum-lethal dose of venom. It is apparent that this minute quantity of antivenene does not render inert the whole of the minimum-lethal dose. All that is required, in order that the minimum-lethal dose

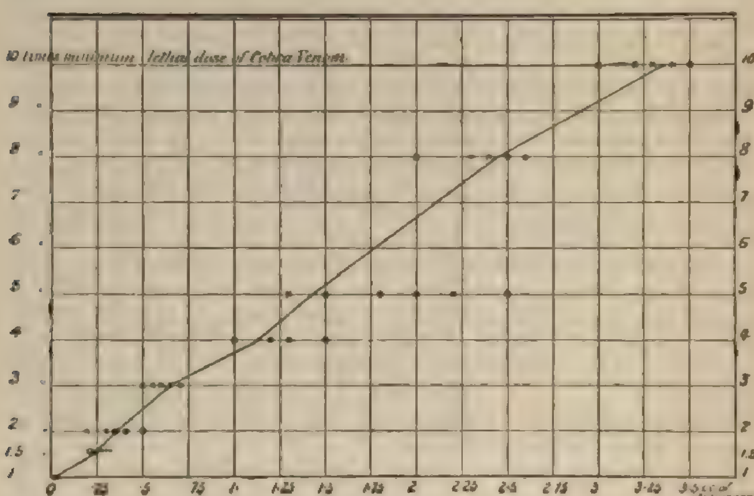

Fig. 2.

should not produce death, being that only a minute portion of it should be rendered inert; for, if this dose be the actual minimum-lethal, the rendering inert of any portion of it, however minute, will prevent the remainder from causing death.

In the *second series*, experiments with the antivenene of the horse have been completed only with one-and-a-half the minimum-lethal dose of venom. When this dose was injected into the subcutaneous tissues of one side of the body, and, immediately thereafter, a dose of antivenene into the subcutaneous tissues of the opposite side, it was found that antivenene in doses of 3 c.c. and 3·3 c.c. per kilogramme failed to prevent death, but that 3·5 c.c. and 3·6 c.c. per kilogramme were able to do so.

In the *third series*, experiments have been made with the minimum-

lethal, one-and-a-half the minimum lethal and twice the minimum-lethal dose of cobra venom. With the first of these doses, recovery occurred with ·5 c.c., ·45 c.c., and ·42 c.c.; but death with ·4 c.c., ·3 c.c., and ·25 c.c. of antivenene, administered thirty minutes before the venom. With one-and-a-half the minimum-lethal of venom, 2·9 c.c. and 2·7 c.c. of antivenene were able to prevent death; while 2·6 c.c., 2·5 c.c., 2·3 c.c., and 2 c.c. each failed in doing so. With twice the minimum-lethal dose of venom, recovery occurred when the doses of antivenene were 5 c.c., 4·5 c.c., and 4 c.c.; but 3·9 c.c., 3·8 c.c., 3·5 c.c., 2·5 c.c., and 2 c.c. were insufficient to prevent death.

In the *fourth series*, where the results give the truest indications of the antidotal value of antivenene in the actual treatment of snake-poisoning, it was found that recovery occurred in the experiments in which ·8 c.c., ·7 c.c., and ·65 c.c. per kilogramme of antivenene was injected thirty minutes after an assuredly minimum-lethal dose (·00025 per kilo.) of venom; but that the antivenene was insufficient in quantity to prevent death when ·6 c.c. or any smaller quantity was administered. In this series, further, it was found that 3·4 c.c. and 3·2 c.c. per kilogramme of antivenene were sufficient doses to prevent death after one-and-a-half the minimum-lethal dose of venom, but that 3 c.c., 2·8 c.c., and 2·5 c.c. per kilogramme were insufficient. In a corresponding series of experiments made with the antivenene derived from rabbits which had last received thirty and fifty times the minimum-lethal dose of cobra venom, it was found that 5 c.c. per kilogramme of this antivenene was the smallest dose by which death could be prevented in an animal which had received twice the minimum-lethal dose of venom thirty minutes previously.

Attention is conspicuously drawn by these facts to the remarkable difference in the dose of antivenene which is required to prevent death when it is mixed with the venom before administration, as contrasted with the doses required when the two substances have not previously been mixed together. Restricting attention to the experiments in each series in which the dose of venom was the same—to the experiments with one-and-a-half the minimum-lethal dose, for instance—it appears that in order to prevent death, when this dose was mixed with antivenene before administration, only ·24 c.c. of antivenene is required; whereas, when both substances were injected simultaneously, but under the skin at different parts of the body, the required dose of antivenene is 3·5 c.c.; when the antivenene was injected thirty minutes before the venom, it was 2·7 c.c.; and when the venom was injected thirty minutes before the antivenene, it was 3·2 c.c. per kilogramme.

It is impossible to consider the great difference between the dose of antivenene required when the two substances, though in each case simultaneously administered, are, in the one case, mixed together before injection, and in the other not so mixed, without again having the suggestion originated that the antidotism is the result of chemical, and not of physiological reactions.

This suggestion receives a further support from the fact, observed in several experiments, that the longer before their administration the two substances were allowed to remain together after they had been mixed, the greater is the antidotal efficiency of the antivenene. Thus, while 1·3 c.c. per kilogramme of antivenene, mixed with five times the minimum-lethal dose of venom, was followed by death when the two had been mixed together five and also ten minutes before administration, this mixture was, on the other hand, followed by recovery when the interval before the administration was extended to twenty minutes. In order to obtain uniform and comparable results in the first series of experiments, it was therefore found necessary to adhere, in all the experiments made with the larger doses of venom, to a time limitation of not more than ten minutes before the mixed substances were injected.

I have also administered cobra-antivenene thirty minutes after a dose one-twelfth larger than the minimum-lethal of the venoms, respectively, of the *Sepedon hæmachates*, the *Crotalus horridus*, and the Diamantina serpent; and the animals experimented on have recovered when the dose of cobra-antivenene was not smaller than 1·5 c.c. per kilogramme. This successful result is all the more remarkable when the intensely destructive effects produced by even smaller doses of each, but especially of two, of these venoms is recollected.

The antivenene derived from rabbits which had been protected to the extent that they had last received fifteen times the minimum-lethal dose of the Diamantina venom has also been tested against the Diamantina venom itself. When the two were administered together, after having been mixed *in vitro*, this antivenene in a dose of 0·5 (1/20) c.c. per kilogramme was able successfully to antagonise slightly less than one-and-a-half the minimum-lethal dose of the venom; but ·025 (1/40) c.c. per kilogramme failed to do so.

In the experiments which I have hitherto described, and, indeed, apparently in all others made in this new subject of serum therapeutics, protection has been produced, and the antidotal properties of the antitoxic blood-serum have been tested, by the subcutaneous, or, less frequently, by the intravenous injection of the venom or other toxic substance. No endeavour seems to have been made to discover how far the same effects, or what effects, may be produced by stomach administration.

Anticipating that results of an interesting nature might be obtained by this method of administration, I have adopted it for the introduction of both antivenene and venom into the body, and the results have even exceeded my anticipations.

The plan followed was the simple one of mixing the substances, previously dissolved in water, with a small quantity of milk, and allowing white rats, which had not received any food for several hours previously, to drink this milk. In the meantime, I will briefly describe only those experiments in which antivenene was thus

administered, reserving, for a few minutes, a description of the results that were obtained when the venom itself was used.

The first experiments were made with the object of determining if, by repeating the process followed in the production of immunity, with the exceptions that the administrations were by the stomach, and that antivenene was substituted for venom, an animal could be protected against the poisonous effects of venom. With this object, a white rat received on alternate days during several weeks, doses of antivenene, which were gradually increased from 1 to 10 c.c. per kilogramme, and then, by subcutaneous injection, one-and-a-half the minimum-lethal doses of cobra venom; with the result that death was not produced. Other white rats received 10 c.c. per kilogramme on each of four days, and on the fifth day 15 c.c. per kilogramme of antivenene, and still recovery took place when one-and-a-half and one-and-three-quarters the minimum-lethal dose of venom was injected under the skin. To other white rats, 10 c.c. and 15 c.c. of antivenene were given by the stomach, on two successive days, and on the second day, one-and-a-half the minimum-lethal dose of venom, and the result also was that death was prevented. It was thus suggested that a single administration of antivenene might be as efficacious as a succession of administrations; and accordingly, the antidotal efficiency of single doses of 7 and of 10 c.c. per kilogramme was tested, in some instances three hours, in others two days, and of 15 c.c. three days before one-and-a-half the minimum-lethal dose of venom was subcutaneously injected; and in all cases the animals recovered. When, however, 5 c.c. per kilogramme of antivenene was thus administered three hours before, and 10 c.c. per kilogramme three days before, one-and-a-half the minimum-lethal dose of venom, the animals died.

The experiments have not as yet been carried further, but I hope to continue them so that the limits of the antidotal power of the antivenene, and the duration of the protection after single doses of antivenene, may be defined. Enough has, however, been done to prove that the stomach administration of antivenene, equally with its subcutaneous administration, confers protection against lethal doses of serpents' venom, and to justify the use of antivenene by the former and more convenient method for the purpose of securing protection for, at least, a period of several days after a single administration of the protecting antidote.

The facts hitherto narrated are sufficient to establish that the protection acquired by animals as a result of the administration of venom is not chiefly, or even to any important degree, caused by the venom having produced a tolerance by accustoming the body, as it has been expressed, to the presence of the venom—although a certain degree of this protection may possibly be due to such accustoming—but rather to the presence in the body, as a result of the introduction into it of venom, of a definite substance having antivenomous qualities. Notwithstanding the powerful protective and antidotal action of this

substance (antivenene) against serpents' venom, it is instructive to find that it is itself almost devoid of any physiological action, for even very large quantities may be injected under the skin without producing any other physiological reaction than a moderate degree of irritation in the neighbourhood of the injection. How then are we to explain the operation of this physiologically inert substance in protecting an animal against even fifty times the minimum-lethal dose of venom, or by a single administration of it, in saving an animal from death after there has been introduced into its body more than twice the quantity of venom that is required to kill it? When an answer has been attempted to be given to this question in discussions in the wider field of the serum therapeutics which deals with the toxines of diseases, the answer has been found either in the destructive power of phagocytes upon microbes and their toxines, or in the theory that the toxine elaborates from the blood the antidotal antitoxine, which, whether thus originated or separately introduced into the body, confers upon the body a resisting power which enables it to oppose successfully the injurious action of the toxines.

These answers cannot solve the problem in so far as snake venom is concerned. Phagocytosis cannot, of course, operate *in vitro* in solutions which are free from organised structures. Even when solutions of venom and antivenene, mixed together *in vitro*, have been inserted into the body, it is incredible that the increase in the quantity of antivenene by the 1/500th part of a cubic centimetre could cause such an increased proliferation of leucocytes as to prevent a lethal dose of venom from producing death, whereas a dose only the 1/500th part of a cubic centimetre smaller would be unable to do so. Further, there is no observable increase of leucocytes when much more than these infinitesimal quantities of antivenene have been administered to an animal.

In view of many of the facts that have to-night been stated, the "resistance of tissues" theory is also untenable. It is opposed, for instance, by the fact that so great a quantity of antivenene as ·42 c.c., or nearly ½ of a cubic centimetre, per kilogramme is required to prevent death when given thirty minutes before a lethal dose of venom, whereas, for the same dose of venom, only ·0004 c.c., or the 1/2500th part of a cubic centimetre, or nearly the 1/1000th part of the former dose, is sufficient, when it is mixed with the venom before administration, and in circumstances, therefore, which are much less favourable for the production by the antivenene of this supposed increase in the resistance of the tissues.

As I have already pointed out, however, a chemical theory, implying a reaction between antivenene and venom, which results in a neutralisation of the toxic activities of the venom, is entirely compatible with the observed facts.

The experiments which I have described to-night indicate that, with some limitations in the largest quantities, the greater the quantity of venom that has been introduced into the body in the process of

producing protection, the greater is the anti-venomous power of the blood-serum, and therefore the larger is the production of the antivenene. While not an actual proof, this circumstance is at the same time in harmony with the supposition that the antivenene may actually be a constituent of the venom itself. The difficulties encountered in the separation by chemical methods of the several constituents of venom are so great, that it is not probable that the only proof or disproof of this supposition will soon be obtained by chemical analysis. Some physiological experiments which I have made seem, however, to go a long way in supplying the demonstration, which in the meantime has not been obtained from chemistry.

With the object of determining, in the first place, if the still disputed statement is correct, that serpents' venom is inert, or nearly so, when introduced into the stomach of an animal, cobra venom was administered, in a series of gradually increasing doses, to a cat, until finally it had received a single dose eighty times larger than the minimum-lethal; and to each of six white rats, single doses corresponding to 10, 20, 40, 300, 600, and 1000 times the minimum-lethal, if given by subcutaneous injection. Although no poisonous symptoms were produced in the animals by even the largest of these enormous quantities, it was found that the cat had so far been protected, that it could afterwards receive, by subcutaneous injection, one-and-a-half the minimum-lethal dose of cobra venom, without any other injury than some localised irritation at the seat of injection; and that the white rat, into whose stomach 1000 times the minimum-lethal dose had been introduced by one administration, survived perfectly, when seven days afterwards slightly more than the minimum-lethal dose of venom was injected under the skin.

It was also found that the blood-serum of the cat was definitely antivenomous, and the curious further fact was ascertained that her progeny had acquired protection through the milk supplied by the protected mother, thus supplying a scientific foundation for a half-admitted conviction, expressed by Wendell Holmes throughout his 'Romance of Destiny,' in regard to the heroine Elsie Venner.

These significant facts have been extended in a number of other experiments on white rats. In one group of experiments, each animal received, by stomach administration, 500 times the minimum-lethal, if given subcutaneously; and, as before, no toxic symptoms were observed. On the day following this administration, three of the animals received subcutaneously one-and-a-half the minimum-lethal dose of the same cobra venom, and they all recovered. In one of the other three animals, however, death was caused by this dose, when it was injected only three hours after the stomach administration; in a second, when this dose was injected two days after the stomach administration; and in the third, when nearly twice the minimum-lethal was injected twenty-four hours after the stomach administration.

In a second group of experiments, a dose of cobra venom equivalent to 1000 times the minimum-lethal by subcutaneous injection was

introduced into the stomach. On several occasions in which this had been done, an injection under the skin of one-and-a-half the minimum-lethal dose of venom made, in some experiments, two days, and in others three days afterwards, resulted in the recovery of the animals. As was anticipated, this large quantity introduced into the stomach, conferred immunity against only certain lethal doses of venom, and, for each lethal dose capable of being rendered innocuous, only within certain definable intervals of time.

The extraordinary result was thus obtained that serpents' venom introduced into the stomach in large quantity—in a quantity, which, if injected under the skin, would be sufficient to kill 1000 animals of the same species and weight—while it failed to produce any definite symptoms of poisoning, nevertheless produced complete protection against the lethal effect of doses of venom more than sufficient to kill the animals. There is a probable significance, further, in the general resemblance between the results of these experiments and those already described in which antivenene, and not venom, was introduced into the stomach. The bearing of these facts is obvious upon discussions relating to the production of immunisation against the toxines of diseases and to the origin of the antidotal qualities of the blood-serum used in their treatment. It is difficult to account for them otherwise than by supposing that the venom while in the stomach had been subjected to a process of analysis, by which the constituents which are poisonous had failed to be absorbed into the blood, or had been destroyed in the stomach or upper part of the alimentary canal, while the constituent or constituents which are antivenomous, or rather antidotal, had passed into the blood, in sufficient quantity to protect the animals against otherwise lethal administrations of venom. I confidently anticipate that this natural process of analysis will, by-and-by, be successfully repeated outside of the body by chemical methods.

It is further to be observed that by stomach administration a degree of protection was acquired in a few hours against lethal doses, such as cannot be attained until after the lapse of several weeks by the method of injecting under the skin a succession of gradually increasing doses of venom. In circumstances, which are no doubt exceptional, the application of this method may therefore acquire some practical value.

Early this evening, I had occasion to point out that the leading facts connected with immunisation or protection, now being advanced as scientific novelties, had apparently been ascertained and practically applied for centuries by savage and uncultured tribes and sects in various parts of the world. In regard to the results I have last described, also, I discover that I have been anticipated by a long-existing and even now prevailing practice of unlearned savages. I have found in the *Lancet* of 1886, an interesting note by Mr. Alford Bolton, containing the following: "The most deadly snakes here are the puff-adders, the yellow cobra capellas, the horn-snakes, and the

night adders. Whilst frequently hearing of horses and cattle rapidly succumbing to the bites of these snakes, it appeared strange that the natives themselves, who mostly ramble about the Veldt almost naked, seldom or never appeared to suffer any further inconvenience from the bites of poisonous snakes than would be usual from any accident which would cause a local inflammation; and, on close inquiry, I found that the natives in Bushmanland, Namaqualand, Damaraland, and the Kalahari, are in the habit of extracting the poison-gland from the snake immediately it is killed, squeezing it into their mouths, and drinking the secretion, and that they thereby appear to acquire absolute immunity from the effects of snake-bites." He proceeds to describe the native treatment of snake-bite, and then adds: "Having a month ago seen a native named Snellstove, who is a snake-poison drinker and collector, put his hand into a box containing two yellow cobras, and several horn- and night-adders, in doing which he was severely bitten, and has never since suffered anything more than a little pain, such as might be caused by any trivial mishap, I feel I can no longer refuse to believe in the efficacy of the snake virus itself as a remedy against snake-poison." Among several communications which I have recently received on the subject, is one from Dr. Knobel, of Pretoria, who writes that when a boy he came into frequent association with a Bushman shepherd, who informed him that he had for years been in the habit of swallowing small quantities of the dried venom-glands of serpents, and he averred that by doing so he obtained protection against serpents' bites, for he had often been bitten without any other ill effect than that an irritable wound was produced. He stated that the swallowed venom of the cobra produced greater protection than the venoms of less poisonous serpents; and that not only was this benefit produced by the swallowing of venom, but that there was also produced an exciting intoxication, differing from that of Indian hemp in so far that the venom always produced the same degree of intoxication with a definite quantity, however frequently it was taken, while the effects of the Indian hemp were gradually lessened by repetition. Another correspondent, Dr. Laurence, of Cape Colony, writes that a Kaffir boy, "aged about twenty-five years, frequently brings me for sale snakes of all kinds. . . . I have frequently seen this boy take hold of some most deadly snakes, especially the well-known puff-adder, which he will allow to bite him with impunity. Yesterday, I obtained from him what he states as the reason why the poison did not harm him. When a little boy, while walking in the Veldt, a puff-adder fastened on his leg. He shook it off, calling to his father, who a few minutes after killed the puff adder and removed the poison glands. He then made small paper pellets and dipped them in the poison, and administered one occasionally to the boy, who stated that that cured him. He expressed his willingness to let any snake bite him."

Several other letters I have received describe similar events, and also confirm the statement of Dr. Knobel, that serpents' venom

produces intoxicating effects in man, evidences of which have been observed in many of the experiments made by me on the lower animals.

The results of the experiments in which the venom was introduced into the stomach, probably also afford an explanation of the protection enjoyed by certain snake-charmers, as well as by other individuals who claim to be protected, whether members of special sects or not; for although inoculation of the venom is apparently sometimes practised by them, and protection is no doubt assisted and maintained by the bites, which with impunity they frequently receive, they are known also to swallow the venom or the dried poison-glands containing it.

These experiments also seem to throw a new light upon the clearly established protection possessed by venomous serpents against their own venom. They suggested the importance of determining if the blood-serum of venomous serpents contains, as does that of artificially protected animals, an actual substance possessing antivenomous properties.

In order to arrive at some definite conclusions on this subject, I last year obtained from India several living specimens of the Hamadryad (*Ophiophagus elaps*), a serpent of greater size and more aggressive disposition than the cobra, and reputed to be as deadly as it. From the blood of several of these serpents a serum was separated, which when dried gave a product having the same physical characters as the antivenene from artificially protected animals. It was tested against cobra venom, both when mixed with rather more than a minimum-lethal dose, and also when injected thirty minutes after this lethal dose of cobra venom. In the former case, ·25 c.c. per kilogramme of this natural antivenene prevented death; and, indeed, so perfectly antagonised this certainly lethal dose that no decided symptoms of poisoning were manifested. In the latter case, 5 c.c. per kilogramme was found to be a sufficient quantity to prevent death. I hope by-and-by to extend these observations by testing the antidotal power of this serum against the venom of the actual Hamadryads from whose blood it had been separated.

A determination of this kind has, however, been made with the blood-serum and venom of the Australian black snake (*Pseudechis porphyriacus*), a deadly serpent whose bite produces intense destructive changes, not only at the place where it has been inflicted, but also in the blood and in many of the organs of the body. When the blood-serum and the venom of this serpent were mixed together outside of the body, and then injected under the skin of a rabbit, it was found that half a cubic centimetre per kilogramme of the blood-serum was sufficient to prevent death from rather more than the minimum-lethal dose of venom.

Notwithstanding the obliging co-operation of the India Office, I have not yet succeeded in obtaining the blood-serum of the cobra, but it may safely be anticipated that it also will be found to possess antivenomous properties.

It has thus been shown that venomous serpents themselves possess a definite substance in the blood-serum which is capable of protecting them against their own venom, and the venom of other serpents. The results of the experiments made by stomach administration of venom, supply at the same time an explanation of one, at least, of the methods by which this substance is introduced into the blood. This natural antivenene, however, is apparently not so powerfully antidotal as the antivenene obtained by the process of artificial protection.

The foregoing statements, although referring mainly to observations on the lower animals, have, probably in every particular, a very direct bearing upon both the prophylaxis and treatment of snake-poisoning in man.

Some little consideration of the details of the application of the antivenene and the employment of auxiliary measures may, however, be serviceable; and, equally of practical service, some consideration of the probable limitations to the capacity of antivenene as an antidote.

In the meantime, I cannot adduce any actual experience of its use in human beings, as although a considerable quantity, both in the liquid and dry state, was last summer sent to India, and a smaller quantity to Africa, no opportunity for using it as an antidote has as yet occurred in the districts to which it had been sent.

But, first, let me say in regard to the altogether unsatisfactory experience of the use of medicines, ordinarily so-called, that I am not prepared to take the extreme position that no good can be done by their employment. While the evidence shows that no one of the very large number of those that have been recommended as antidotes is able, in any conditions of administration, to prevent death after the reception of even the smallest lethal dose of venom, it still may be that, by the physiological effects which they produce, they may assist any efficient antidote, such as antivenene, in preventing death; and also, by prolonging life, increase the opportunity for a more thorough use of this antidote. In this category I would especially place medicines which increase excretion, such as diaphoretics and diuretics; many of the rapidly acting stimulants of the circulation, such as alcohol and the old snake-remedy, ammonia; and stimulants of respiration, such as atropine and strychnine, the latter of which is enthusiastically championed by Dr. A. Mueller, of Sydney. And not only medicines, but also any measures that are available for these purposes, including artificial respiration, so distinctly indicated as a probably valuable therapeutical application in snake-bite by Fayrer and Brunton, which, though shown by the Indian Snake Commission to be incapable of preventing death when alone trusted to, was also shown to possess the valuable auxiliary power of prolonging life.

The first measure, however, that is usually and properly taken in the treatment of snake-bite, is to restrict, as far as is possible, the

absorption of the venom into the blood-vessels, from the place into which it has been injected by the poison-fangs, by separating this place from the more central parts of the body by a tight ligature. The efficiency of this measure, preventive rather than curative, is fortunately aided by the circumstance that snake-bites are most usually inflicted at parts to which a ligature can conveniently be applied; for in fifty-four cases collected by Wall, the part in nearly 89 per cent. of the cases was on the arms or legs. The ligature having been applied, whenever it is possible to do so, the next measure to adopt is to open up with a knife, to a considerable depth, the minute though deep punctures made by the fangs, and then to apply suction to the wound. Justification is found for this procedure in the fact, demonstrated by experiment, that notwithstanding the rapidity with which venom may be absorbed, a portion of it still remains for a considerable time in the tissues immediately surrounding the wound. This has been clearly demonstrated by both Kaufmann and Wall. The suction may be produced by the mouth, and in the absence of more effective apparatus this ready method would be serviceable, while it is attended with danger to the operator only in the infrequent occurrence of fissures or abrasions of the mouth. It is, however, more effectively and without any risk accomplished by a suction pump, such as the most useful pump invented by Mr. Andrew Smith, of Cape Colony, which I now show.

These steps having been taken, antivenene should be injected into the tissues at and near the wound and, also, under the skin above the ligature; and the ligature should not be removed until at least half an hour after a sufficient quantity of antivenene has been injected under the skin above it.

But the important question has yet to be answered, What is a sufficient quantity? The whole tenor of my remarks to-night has been to show how necessary it is to bear in mind that there is a definite relationship between the dose of venom received and the dose of antivenene required to antagonise it, and that this relationship also varies with the conditions of the administration of the antivenene, and, especially, with the interval of time that elapses between the reception of the venom and the administration of the antivenene.

In snake-bite in man it is impossible to estimate the dose of venom which has been injected, for the nature of the symptoms in the patient cannot give the information even approximately. In searching for a solution of this problem, several facts may be taken into consideration from which assistance may be obtained. And, firstly, what is the probable quantity of venom that a serpent injects into a wound? Some data for answering this question have, very kindly, been obtained for me by Brigade-Surgeon Lieut.-Colonel Cunningham, of Calcutta. Taking nine adult cobras, healthy and vigorous, he collected from each the venom ejected at a single bite,

dried and weighed each collection separately, and sent me the weights. They are as follows:—

(1) 0·726 gramme.	(4) 0·114 gramme.	(7) 0·239 gramme.
(2) 0·262 „	(5) 0·132 „	(8) 0·306 „
(3) 0·115 „	(6) 0·113 „	(9) 0·253 „

The total venoms yield an average of 0·255 gramme for each bite; but, if the exceptionally large quantity stated in the first figure be excluded, the average for the remaining eight becomes ·195 gramme. It must also be considered that these quantities were obtained in the most favourable conditions for securing the total quantity ejected at a single bite, whereas in actual practice the conditions are less favourable for the insertion of the total available venom into the tissues of the victim.

Reverting now to determinations of the minimum-lethal dose for the lower animals, we find that if the minimum-lethal dose for the cat be adopted as being the same as that for man, the total quantity of dry cobra-venom required to kill a man of ten stones weight would be ·317 gramme, which is considerably more than the quantity, judging from the above averages, that a cobra is usually able to eject during a single bite. It would therefore appear necessary to assume that the minimum-lethal dose per kilogramme for man is smaller than for a cat; but, as it is probably greater than for a rabbit, we may for convenience assume that it is twice that dose. In this case, the smallest quantity required to produce death in a man of ten stones would be about ·0317 gramme, which, however, seems to be considerably less than the quantity which a fresh cobra has at its disposal. Applying now the facts that have been stated in the series of experiments where the smallest quantity of antivenene required to prevent death when injected thirty minutes after twice the minimum-lethal dose was determined, it will be recollected that that quantity is 5 c.c. per kilogramme of animal. Taking this as a basis for the dose of antivenene, in order to prevent death in man from the estimated minimum-lethal dose of cobra-venom, so considerable a quantity as 330 c.c., or about 11½ ounces, of antivenene would be required, if the antivenene be injected not much longer than thirty minutes after the bite had been inflicted. This, though a large, is by no means an impossible dose, and it could, without much inconvenience, be introduced under the skin at several parts of the body.

On the other hand, the estimate which I have adopted of the minimum-lethal dose for man may be too high a one, and if it should prove to be nearer that for the rabbit, then the quantity of antivenene required to prevent death, if administered half an hour after the snake-bite, would be reduced to about four ounces. It is also to be recollected that if dry antivenene be used, it may be dissolved in a much smaller quantity of liquid than is required to restore it to its original bulk.

As to the probability, in a fatal snake-bite, of the quantity of venom received by the victim being only about, and not much in excess of, the minimum-lethal dose, it would appear that, in many cases, even so large a dose is not introduced; for general experience indicates that the majority of persons who are bitten actually recover, whatever treatment is adopted. Sir Joseph Fayrer also shows, in his classical 'Thanatophidia,' that in 64 per cent. of fatal cases of snake-bite in India, the victims survived the infliction of the bite for periods of from three to twenty-four hours; and this duration of life implies that the dose of venom received could not have been much greater than the minimum-lethal.

It must be admitted, however, that even for the minimum-lethal dose of venom, the quantity of antivenene required to prevent death in man is probably inconveniently large, especially if, in the treatment, reliance is placed solely upon the administration of antivenene, to the exclusion of all or several of the auxiliary measures to which I have referred. It is desirable, also, that the antivenene treatment should be a practical one, not only for doses of venom which do not much exceed the minimum-lethal, but also for the considerably larger doses that are occasionally introduced in snake-bite.

To attain this object, further work is required in order that there may be obtained an antivenene even more powerful than that whose antidotal capabilities I have described.

I am not sanguine that this will be accomplished by carrying to a higher degree the process of artificial protection in animals. A comparison of the antivenene of rabbits which had last received thirty times the minimum-lethal dose of cobra venom with that of other rabbits which had last received fifty times that dose, has shown that the latter has but little antidotal advantage over the former, and has suggested that, in the process of artificial protection, the saturation point of the blood for antivenene is reached before the possible maximum non-fatal dose of venom has been administered.

I would anticipate with more hope the results of endeavours to separate the true antivenomous principles from the inert constituents of the blood-serum with which they are mixed; and although the required chemical manipulations are attended with many difficulties, some success has already been obtained in effecting this separation.

In the foregoing remarks, it has, however, been shown that even with the antivenene whose properties have been described, human life may be saved in a considerable, if not in a large, proportion of the cases of snake-bite which would otherwise terminate in death. The attainment of this result is a satisfactory one; for the mortality from snake-bite is large, and is not restricted to the 20,000 deaths which annually occur in India, but includes additional thousands in all the tropical and sub-tropical regions of the world.

[T. R. F.]

WEEKLY EVENING MEETING,

Friday, March 27, 1896.

Edward Frankland, Esq. D.C.L. LL.D. F.R.S. Vice-President, in the Chair.

Professor Dewar, M.A. LL.D. F.R.S. *M.R.I.*

New Researches on Liquid Air.

Of all the forms of engineering plant used in low temperature research, the best and most economical for the production of liquid air or oxygen is one based on the general plan of the apparatus used by Pictet in his celebrated experiments on the liquefaction of oxygen in the year 1878. Instead of using Pictet's combined circuits of liquid sulphur dioxide and carbon dioxide, maintained in continuous circulation by means of compression, liquefaction and subsequent evaporation, it is preferable to select ethylene (after Cailletet and Wroblewski) for one circuit, and for the other either nitrous oxide or, better, carbon dioxide. Further, instead of making highly compressed oxygen to be liquefied by heating potassium chlorate in an iron bomb directly connected with the refrigerator, it is safer and more convenient to use gas previously compressed in steel cylinders. The stopcock that Pictet employed to draw off liquid and produce sudden expansion, was in his apparatus placed outside the refrigerator proper, but it is now placed inside, so as to be kept cool by the gases undergoing expansion. This improvement was introduced along with that of isolating the liquid gases by surrounding them with their own cooled vapour in the apparatus made wholly of copper, described and figured in the Proc. Roy. Inst. for 1886. In all continuously working circuits of liquid gases used in refrigerating apparatus, the regenerative principle applied to cold, first introduced by Siemens in 1857, and subsequently employed in the freezing machines of Kirk, Coleman, Solvay, Linde and others, has been adopted. Quite independently, Professor Kamerlingh Onnes, of Leiden, has used the regenerative principle in the construction of the cooling circuits in his cryogenic laboratory.* Apart, therefore, from important mechanical details, and the conduct of the general working, nothing new has been added by any investigator to the principles involved in the construction and use of low temperature apparatus since the year 1878.

* See paper by Dr. H. Kamerlingh Onnes, on the "Cryogenic Laboratory at Leiden, and on the Production of very low Temperatures," Amsterdam Akademie, 1894.

Detailed drawings of the Royal Institution refrigerating plant now in use have not been published, simply because changes are constantly being made in the apparatus. Science derives no benefit from the description of transitional apparatus when there is no secret about the working process and how to carry it into effect. The Phil. Mag. of February, 1895, contains a fantastic claim put forward by Professor Olszewski, of Cracow, that because he used in 1890 a steel tube combined with a stopcock to draw off liquid oxygen, he had taught the world, to use his own language, "the method of getting large quantities of liquid gases." In addition the Professor alleges, four years after the event, that the experiments made at the Royal Institution are chiefly borrowed from Cracow, and that he is entitled to the credit of all low temperature research. As to such claims, one can only wonder at the meagre additions to knowledge that in our time are unhesitatingly brought forward as original, and more especially that scientific men could be got to give them any currency in this country. Such persons should read the late Professor Wroblewski's pamphlet, entitled 'Comment l'air a été liquefié,'* and make themselves generally acquainted with the work of this most remarkable man before coming to hasty conclusions on claims of priority brought forward by his some time colleague.

Liquefying Apparatus.—A laboratory apparatus for the production of liquid oxygen and other gases is represented in section (Fig. 1). With this simple machine, 100 c.c. of liquid oxygen can readily be obtained, the cooling agent being carbon dioxide, at the temperature of $-79°$. If liquid air has to be made by this apparatus, then the carbonic acid must be kept under exhaustion of about 1 inch of mercury pressure, so as to begin with a temperature of $-115°$. Under such conditions the yield of the liquid gases is much greater. The gaseous oxygen, cooled before expansion by passing through a spiral of copper tube immersed in solid carbon dioxide, passes through a fine screw stopcock under a pressure of 100 atmos., and thence backwards over the coils of pipe. The liquid oxygen begins to drop in about a quarter of an hour from starting. The general arrangement of the circuits will be easily understood from the sectional drawing. The pressure in the oxygen cylinders at starting is generally about 150 atmos., and the best results are got by working down to about 100. If a small compressor is combined with the apparatus the liquefaction can go on continuously. This little apparatus will enable liquid oxygen or air to be used for demonstration and research in all laboratories.

Vacuum Vessels.—It has been shown in previous papers † that a good exhaustion reduces the influx of heat to one-fifth part of what is conveyed when the annular space in such double-walled vacuum vessels is filled with air. If the interior walls are silvered, or excess

* Paris, Libraire du Luxembourg, 1885.

† "On Liquid Atmospheric Air," Proc. Roy. Inst. 1893; "Scientific Uses of Liquid Air," ibid. 1894.

of mercury is left in the vessel, the influx of heat is diminished to one-sixth part of the amount entering without the metallic coating. The total effect of the high vacuum and silvering is to reduce the ingoing heat to one-thirtieth part, or, roughly, 3½ per cent.. Vessels constructed with three dry air spaces only reduced the influx of heat to 35 per cent. An ordinary mercury vacuum vessel is therefore ten times more economical for storing liquid air, apart from considerations of manipulation, than a triple annular spaced air vessel. It has been suggested that the metallic coating of mercury does no good, because Pictet has found that all kinds of matter become transparent to heat at low temperatures. The results above mentioned dispose of this assumption, and direct experiment proves that no increase in the transparency of glass to thermal radiation is effected by cooling it to the boiling point of air.*

An ocular demonstration of the correctness of the above statements can easily be shown by mounting on the same stem three similar double-walled test tubes, two of which have been simultaneously exhausted and sealed off from the air pump together, while the third is left full of air. One of the vacuum test tubes is coated with silver in the interior. The apparatus is shown in Fig. 2. A has the annular space filled with air; B and C are exhausted, C being coated with silver. On filling liquid ethylene to the same height into each vessel, and inserting corks with similar gas jets and igniting the escaping gas, the relative volumes of the flames is roughly proportional to the influx of heat, and resembles what is shown in the drawing. It is satisfactory to have independent corroboration of the advantages of the use of vacuum vessels, and this may be found in a paper by Professor Kamerlingh Onnes, of Leiden, communicated to the Amsterdam Academy of Sciences, 1896, entitled 'Remarks on the Liquefaction of Hydrogen, on Thermodynamical Similarity, and in the Use of Vacuum Vessels,' in which he says:—"In the same degree as it becomes of more importance to effectuate adiabatic processes at very low temperatures, the importance of the vacuum vessels of Dewar will increase. It seems to me that they are the most important addition since 1883 to the appliances for low temperature research." . . . "It is a rejoicing prospect that practical engineers will doubtless feel the want of such non-conducting mantles. For as soon as this stage is

* At a meeting of the French Academy in 1895 a paper by M. Solvay of Brussels was read, in which my 1892 device of vacuum vessels was attributed to M. Cailletet, and tacitly accepted by him! In 1875 I had already used a highly exhaustive vessel, of similar shape to the vacuous test tube, in calorimetric experiments. See paper on "The Physical Constants of Hydrogenium," Trans. Roy. Soc. Ed. vol. xxvii. Even as late as April 1896, Professor Tilden, D.Sc. F.R.S. of the Royal College of Science, in a paper entitled "L'Appareil du Dr. Hampson pour la Liquefaction de l'air et des gas," communicated to the 'Revue Générale des Sciences,' thought proper to write as follows: "Un manchon de verre, dans lequel on a fait le vide (manchon semblable à ceux décrits par Cailletet ou Dewar)." Where did Professor Tilden find Cailletet's description of a vacuum vessel? This is not the only statement in the paper requiring correction.

reached, numbers of heads and hands are disposed to take over the problem from the scientific researcher."

Solid Air.—As Professor Olszewski has recently alleged that air does not solidify at the lowest pressures,* the author's former experiments were repeated on a larger scale. If a litre of liquid air is placed in a globular silvered vacuum vessel and subjected to exhaustion, as much as half a litre of solid air can be obtained and maintained in this condition for half an hour. At first the solid is a stiff, transparent jelly, which, when examined in the magnetic field, has the liquid oxygen drawn out of it to the poles. This proves that solid air is a nitrogen-jelly containing liquid oxygen. This statement was made in a paper "On the Refraction and Dispersion of Liquid Oxygen, and the Absorption Spectrum of Liquid Air" (Professors Liveing and Dewar), published in the Phil. Mag. for September 1895, yet Professor Olszewski, in 1896† is declaring "that Professor Dewar has stated that liquid air solidifies as such, the solid product containing a slightly smaller percentage of nitrogen than is present in the atmosphere. My experiments have proved this statement to be incorrect." The Cracow professor may well have the satisfaction of correcting a statement which was never made by me. He seems also to forget that in 1893, Proc. Roy. Inst. Lecture on Liquid Air, it is distinctly stated that "all attempts to solidify oxygen by its own evaporation have failed." Solid air can only be examined in a vacuum or in an atmosphere of hydrogen, because it instantly melts on exposure to air cooled to the temperature of its boiling point, giving rise to the liquefaction of an additional quantity of air. It is strange to see a mass of solid air melting in contact with the atmosphere, and all the time welling up like a kind of fountain. The apparatus shown in Fig. 3 is well adapted for showing the direct liquefaction of the air of a room and its solidification. A large vacuum vessel G, is mounted on a brass stand containing another smaller vessel B of the same kind. By means of the two cocks C and D, either the large vessel G or the bulb B can be connected to the air pump circuit. Liquid oxygen is placed in A, which can, by opening the stopcock D, be cooled to -210° by exhaustion. If the stopcock C is shut and a barometric gage is joined on at F, the dropping of the liquid air from the outside of A will go on even at as low a pressure as 4 in. of mercury; which is equivalent to saying that this apparatus would liquefy air if taken by a balloon ten miles high. If F is now opened, giving a supply of air at atmospheric pressure, the cup B soon fills with liquid air. Unless the air supply is passed over soda lime and strong sulphuric, the liquid is always turbid from the presence of ice crystals and solid carbonic acid. Now on shutting F and opening C, the air in B is placed under exhaustion and soon solidifies to a jelly-like mass. When the vacuum is about 14 mm. then the temperature of the solid air is -232° by the platinum resistance

* Phil. Mag. February 1895.

† See 'Nature,' Aug. 20, p. 378.

thermometer, or $-216°$ C. On allowing the air to enter, the solid instantly melts and more liquid air is formed. The same experiment may be repeated many times by simply opening and shutting the stopcocks. When the liquid air loses too much nitrogen, then it no longer solidifies. This apparatus may be used to show that when liquid air is running freely into B, liquefaction is instantly arrested by allowing hydrogen to enter instead of air.

Samples of Air Liquefied in Sealed Flasks.—In a paper "On the relative behaviour of chemically prepared and of atmospheric nitrogen," communicated to the Chemical Society in December 1894, the plan of manipulating such samples was described. The arrangement shown in Fig. 4 illustrates how oxygen in A under 0·21 of an atmos. pressure, and nitrogen in B under 0·79 of an atmos., can be compared as to the first appearance of liquefaction in each, and finally as to their respective tensions when the temperature is as low as that of solid nitrogen. The flasks A and B have a capacity of more than a litre. Each has a manometer sealed on, and in each phosphoric anhydride is inserted to secure dryness. A large vacuum vessel C holds the liquid air, which is gradually lowered in temperature by boiling under exhaustion. The moment liquefaction takes place, the tubes D′, D″ begin to show liquid. These tubes must be drawn fine at the end when accurate observations are being made. In the same manner two oxygen flasks were compared. One filled with gas made from fused chlorate of potash, contained in a side tube sealed on to the flask. The other was treated in the same way, only the chlorate had a little peroxide of manganese added. The former gave perfectly clear blue liquid oxygen, the latter was turbid from solid chlorine. Two flasks of dry air that had stood over phosphoric anhydride were liquefied side by side, the only difference between the samples being that one was free from carbonic acid. The one gave a liquid that was perfectly clear, the other was turbid from the 0·04 per cent. of carbon dioxide.

The temperature was lowered by exhaustion until samples of liquid air from two flasks placed side by side as in Fig. 4 became solid. The flasks were then sealed off for the purpose of examining the composition of the air that had not been condensed. The one sample contained oxygen, 21·19 per cent., and the other 20·7 per cent. This is an additional proof to the one previously given that, substantially, the oxygen and nitrogen in air liquefy simultaneously, even under gradually diminishing pressure, and that in these experiments all the known constituents of air are condensed together. These results finally disprove the view expressed in 'A System of Inorganic Chemistry,'* by Professor Ramsay, where he says: "Air has been liquefied by cooling to $-192°$, but as oxygen and nitrogen have not the same boiling points, the less volatile oxygen doubtless liquefies first." My old experiments† showed that the substance now known as argon became solid before nitrogen, but chemical

* 1891, p. 70.

† See Proc. Chem. Soc. Dec. 1894.

nitrogen and air nitrogen, with its 0·1 per cent. of argon, behaved in substantially the same way on liquefaction.

Liquid Nitric Oxide.—Great interest attaches to the behaviour of nitric oxide at low temperatures. Professor Olszewski has examined the liquid and describes it as colourless. Samples of nitric oxide have been prepared in different ways. These have been transferred to liquefaction flasks, where they were left in contact with anhydrous potash, sulphuric acid alone, a mixture of sulphate of aniline and sulphuric acid, or phosphoric acid, for many days before use. Each of the samples, when cooled, gave a nearly white solid, melting into a *blue liquid.* The colour is more marked at the melting point than at the boiling point. Liquid nitric oxide is not magnetic; neither is the solid phosphorescent. Colour in the oxides of nitrogen evidently begins with the second oxide. Solid nitric oxide does not show any chemical action when placed in contact with liquid oxygen, provided the tube containing it is completely immersed; but if the tube full of liquid oxygen is lifted into the air, almost instantly a violent explosion takes place.

Specific Gravities taken in Liquid Oxygen.—In a good vacuum vessel specific gravities may be taken in liquid oxygen with as great ease as in water. The shape of the vacuum vessel which works best is shown in Fig. 4. It must contain excess of mercury and be thoroughly boiled out, so that the inner vessel becomes completely coated with a mercury mirror as soon as the liquid oxygen is filled in. Instead of a mercury vacuum, the interior may be silvered and highly exhausted by a Sprengel pump. The flasks must also be thoroughly clean and free from dust, otherwise the liquid oxygen will not remain tranquil. Any superheating is prevented by inserting a long narrow piece of wood for a moment before the final weighing.

Some twenty substances were weighed in liquid oxygen,* and the apparent relative density of the oxygen determined. The results were then corrected, using Fizeau's values for the variation of the coefficient of expansion of the solids employed, and thereby the real density of liquid oxygen calculated. The resulting value was 1·1375, bar. 766·5, in the case of such different substances as cadmium, silver, lead, copper, silver iodide, calc-spar, rock crystal. The following table gives some of the observations:—

Mean Cubical Coefficient of Expansion between 15° C.–183° C.		Apparent Density of Liquid Oxygen.	Real Density of Liquid Oxygen.
Cadmium,	7986×10^{-5}	1·1188	1·1359
Lead,	7892 „	1·1197	1·1367
Copper,	4266 „	1·1278	1·1370
Silver,	5185 „	1·1278	1·1385
Calc-spar,	1123 „	1·1352	1·1376
Rock crystal,	2769 „	1·1316	1·1376
Silver Iodide,	0189 „	1·1372	1·1376

* The liquid oxygen might possibly contain a small proportion of nitrogen.

Direct determinations with an exhausted glass cylindrical vessel displacing about 22 c.c. gave 1·1378. Fizeau's parabolic law for the variation of the coefficient of expansion holds down to $-183°$. The solid which showed the greatest contraction was a block of compressed iodine; the one that contracted least being a compressed cylinder of silver iodide. Wroblewski gave the density of liquid oxygen at the boiling point as 1·168, whereas Olszewski found 1·124. The variation of density is about $\pm 0·0012$, for 20 mm. barometric pressure. Much work requires to be done in the accurate determination of the physical constants of liquid gases.

Liquid Air.—A large silver ball weighed in liquid air gave the density of the latter as 0·910, and the corresponding density of nitrogen at its boiling point 0·850. It is difficult to be quite certain that the constituents of liquid air are in the same proportion as the gaseous ones, so that further experiments must be made. Liquid air kept in a silvered vacuum vessel gradually rises in boiling point from the instant of its collection, the rate of increase during the first hour being nearly directly proportional to the time. As the increase amounted to 1° in ten minutes, the boiling point of oxygen ought to have been reached within two hours. The density of liquid air, however, does not reach that of pure oxygen even after thirty hours' storage. The large apparatus of the Royal Institution for air liquefaction can be arranged to deliver liquid air containing 49 per cent. of oxygen, which gives off gas containing 20 per cent. of oxygen, rising after six hours to 72·6 per cent.

Combustion in Liquid Oxygen.—A small ignited jet of hydrogen burns continuously below the surface of liquid oxygen, all the water produced being carried away as snow. There is a considerable amount of ozone formed, which concentrates as the liquid oxygen evaporates. In the same way graphite or diamond, when properly ignited, burns continuously on the surface of liquid oxygen, producing solid carbonic acid and generating ozone. If liquid oxygen is absorbed in wood charcoal, or cotton-wool, and a part of the body heated to redness, combustion can start with explosive violence.

Gas Jets containing Liquid.—The experiments of Joule and Thomson and Regnault on the temperature of gas jets issuing under low pressures are well known. The following observations refer to the pressure required to produce a lowering of temperature sufficient to yield liquid in the gas jet.

The apparatus used in the study of highly compressed gas jets is represented in Fig. 2; where C is a vacuum tube which holds a coil of pipe about 5 mm. in diameter surrounded with carbon dioxide or liquid air for cooling the gas before expansion, and A is a small hole in the silver or copper tube about ½ mm. in diameter, which takes the place of a stopcock. When carbon dioxide gas at a pressure of 30 or 40 atmos. is expanded through such an aperture, liquid can be seen where the jet impinges on the wall of the vacuum tube, along with a considerable amount of solid. If oxygen gas escapes from the small hole at the pressure of 100 atmos. having been cooled previously

to $-79°$ in the vessel C, a liquid jet is just visible. It is interesting to note, in passing, that Pictet could get no liquid oxygen jet below 270 atmos. This was due to his stopcock being massive and outside the refrigerator. If the oxygen is replaced by air, no liquid jet can be seen until the pressure is 180 atmos., but on raising the pressure to 300 atmos. the liquid air collected well from the simple nozzle. If the carbon dioxide is cooled by exhaustion (to about 1 inch pressure) or $-115°$, then liquid air can easily be collected in the small vacuum vessel D, or if the air pressure is raised above 200 atmos., keeping the cooling at $-79°$ as before.* The chief difficulty is in collecting the liquid, owing to the rapid current of gas. The amount of liquid in the gas jet is small, and its collection is greatly facilitated by directing the spray on a part of the metallic tube above the little hole, or by increasing the resistance to the escaping gas by placing some few turns of the tube, like B in the figure, in the upper portion of the vacuum tube, or generally by pushing in more tube in any form. A vacuum vessel shaped like an egg-glass also works well. This practically economises the cool gas which is escaping to reduce the temperature of the gas before expansion, or, in other words, it is the cold regenerative principle. Coleman pointed out long ago that his air machine could be adapted to deliver air at as low a temperature as has yet been produced in physical research. Both Solvay and Linde have taken patents for the production of liquid air by the application of cold regeneration, but the latter has the credit of having succeeded in constructing an industrial apparatus that is lowered in temperature to $-140°$, or to the critical point of air, in about 15 hours, and from which liquid air containing 70 per cent. oxygen is collected after that time.

For better isolation, the pipe can be rolled between two vacuum tubes, the outer one being about 9 inches long and $1\frac{1}{2}$ inch diameter, as shown in Fig. 3. The aperture in the metal pipe has a little piece of glass tube over it, which helps the collection of the liquid. With such a simple apparatus, and an air supply at 200 atmos. with no previous cooling, liquid air begins to collect *in about five minutes*, but the liquid jet can be seen in between two and three minutes. It is not advisable to work below 100 atmos.

In Fig. 4 the metallic tube in the vacuum vessel is placed in horizontal rings, leaving a central tube to allow the glass tube C to pass, which is used to cool bodies or examine gases under compression. The inner tube can be filled for an inch with liquid air under a pressure of 60 atmos. in about three minutes. Generally, in the experiments, about $\frac{1}{2}$ to 4 cubic feet of air passes through the different sized needle holes per minute when the pressure is about 200 atmos. As the small hole is apt to get stopped, for general

* The liquefaction is taking place in this condition at $1\frac{1}{2}$ times the critical temperature. Hydrogen similarly expanded at the melting point of air ($-214°$ C.) behaves exactly in the same way.

working it is better to use a needle stopcock, worked from the outside by a screw passing through the middle of the coil of pipe.

In testing the individual coils as to the amount of air passed per minute under different pressures, the arrangement of apparatus shown in the Plate 7 was used.

A is a bottle of compressed air, to which the copper pipe B is attached. This coiled pipe first passes through the vessel C containing water, in order to equalise the temperature, and then through the cork D into the glass vacuum vessel E, when it is led by a large number of convolutions to the bottom, terminating in a minute pinhole valve F. The released air passes from F right up through the coils and out of the vent by the copper tube G, which in its turn passes through a vessel H similar in its object to C, and is then conducted to a measuring meter Z J.

The following table gives the results of a series of experiments made on one coil as to the rate of discharge of air at different pressures:—

Pressure in Atmospheres.	Cubic Feet per Minute Measured under Atmosphere at 15°.
55	0·22
105	0·42
155	0·63
198	0·79
210	0·84
250	1·00
287	1·15
290	1·18

The results show that the rate of air discharge through a fine aperture is directly proportionate to the pressure, or the velocity with which the gas on the high-pressure side enters the orifice, is independent of the density. Actual measurements of the size of the needle-hole resulted in proving that the real velocity of the air entering the aperture on the high-pressure side was about 500 feet per second. In all these experiments the temperature of the coil was not allowed to get so low as to produce any visible trace of condensation in the air jet. Just before liquefaction the rate of discharge of air through the same aperture may be doubled, the pressure remaining steady, owing to change in the viscosity of the gas and other actions taking place at low temperatures. The above measurements can only be regarded as representing the general working of such regenerating coils.

A double coil of pipe has advantages in the conduct of some experiments. The efficiency is small, not exceeding the liquefaction of 2 to 5 per cent. of the air passing, but it is a quick method of

reaching low temperatures, and easy to use for cooling tubes and collecting a few hundred c.c. of liquid air, especially if the compressed air is delivered at the temperature of $-79°$ before expansion. With larger vacuum vessels and larger regenerating coils no doubt the yield of liquid could be increased. The liquid air resulting from the use of this form of apparatus contains about 50 per cent. of oxygen. If the air is cooled with solid carbonic acid previous to its reaching the vacuum tube coil of pipe, the only change is to reduce the percentage of oxygen to 40. Successive samples of liquid taken during the working had nearly the same composition. If the arrangement shown in Fig. 2 is used, with silver tube, about $\frac{1}{16}$ inch bore, and a foot or two coiled in upper part of the vacuum vessel, liquid air containing 25 per cent. of oxygen is obtained. On the other hand, the percentage of oxygen can be increased by a slight change in the mode of working.

In the above experiments air is taken at the ordinary temperature, which is a little above twice its critical temperature, and is partially transformed in a period of time which, in my experiments, has never exceeded ten minutes, simply and expeditiously into the liquid state at its boiling point, $-194°$, or a fall of more than 200° has been effected in this short period of time.

Experiments on Hydrogen.—Wroblewski made the first conclusive experiments on the liquefaction of hydrogen in January 1884. He found that the gas cooled in a tube to the boiling point of oxygen, and expanded quickly from 100 to 1 atmos., showed the same appearance of sudden ebullition as Cailletet had seen in his early oxygen experiments. No sooner had the announcement been made than Olszewski confirmed the result by expanding hydrogen from 190 atmos. previously cooled with oxygen and nitrogen boiling in vacuo. Olszewski declared in 1884 that he saw colourless drops, and by partial expansion to 40 atmos. the liquid hydrogen was seen by him running down the tube. Wroblewski could not confirm these results, his hydrogen being always what he called a "liquide dynamique." He proposed to get "static" liquid hydrogen by the use of hydrogen gas as a cooling agent. Professor Ramsay, in his 'System of Inorganic Chemistry,' published long after the early experiments of Pictet, Cailletet, Wroblewski and Olszewski on the liquefaction of hydrogen had been made, sums up the position of the hydrogen question in 1891 as follows (p. 28):—"It has never been condensed to the solid or liquid states. Cailletet, and also Pictet, who claim to have condensed it by cooling it to a very low temperature, and at the same time strongly compressing it, had in their hands impure gas. Its critical temperature, above which it cannot appear as liquid, is probably not above $-230°$." It has to be remembered that 7 per cent. of air by volume in hydrogen means about 50 per cent. by weight of the mixed gases. Even 1 per cent. by volume in hydrogen is equivalent to some 13 per cent. by weight.

The following table gives the theoretical temperatures reached for

an instant during the adiabatic expansion of hydrogen under different conditions:—

Initial Pressure Atmospheres.	Initial Temperature.	Theoretical Final Temperature (Absolute).
	°	°
500 (Pictet)	−130	25
300 (Cailletet)	0	52
100 (Wroblewski)	−184	24
180 (Olszewski)	−210	14
100	−200	19·5
200	−200	15·7
500	−200	12·7

The calculations show that little is gained by the use of high pressures. The important inference to be drawn from the figures is to start with as low a temperature as possible.

From 1884 until his death, in the year 1888, Wroblewski devoted his time to a laborious research on the isothermals of hydrogen at low temperatures. The data thus arrived at enabled him, by the use of Van der Waal's formulæ, to define the critical constants of hydrogen, its boiling point, density, &c., and the subsequent experiments of Olszewski have simply confirmed the general accuracy of Wroblewski's results. Wroblewski's critical constants of hydrogen are given in the following table:—

Critical temperature	− 240°
" pressure	13·3 atmos.
" density	0·027
Boiling point	− 250°
Density at boiling point*	0·063

In a paper published in the Phil. Mag. September 1884, "On the Liquefaction of Oxygen and the Critical Volumes of Fluids," the suggestion was made that the critical pressure of hydrogen was wrong, and that instead of being 99 atmos. (as deduced by Sarrau from Amagat's isothermals) the gas had probably an abnormally low value for this constant. This view was substantially confirmed by Wroblewski finding a critical pressure of 13·3 atmos., or about one-fourth that of oxygen. The 'Chemical News' (September 7, 1894) contains an account of the stage the author's hydrogen experiments had reached at that date. The object was to collect liquid hydrogen at its boiling point in an open vacuum vessel, which is a much more difficult problem than seeing the liquid in a glass tube under pressure and at a higher temperature. In order to raise the critical point of hydrogen to about − 200°, from 2 to 5 per cent. of nitrogen or air was mixed with it. This is simply making an artificial gas containing a large

* It is probable that the real density of boiling liquid hydrogen may lie between 0·12 and 0·18.

proportion of hydrogen, which is capable of liquefaction by the use of liquid air. The results are summed up in the following extract from the paper :—"One thing can, however, be proved by the use of the gaseous mixture of hydrogen and nitrogen, viz. that by subjecting it to a high compression at a temperature of $-200°$, and expanding the resulting liquid into air, a much lower temperature than anything that has been recorded up to the present time can be reached. This is proved by the fact that such a mixed gas gives, under the conditions, a paste or jelly of solid nitrogen, evidently giving off hydrogen because the gas coming off burns fiercely. Even when hydrogen containing only some 2 to 5 per cent. of air is similarly treated the result is a white, solid matter (solid air) along with a clear liquid of low density, which is so exceedingly volatile that no known device for collecting has been successful." *

In Professor Olszewski's paper "On the Liquefaction of Gas," † after detailing the results of his hydrogen experiments, he says :—"The reason for which it has not hitherto been possible to liquefy hydrogen in a static state is, that there exists no gas having a density between that of hydrogen and nitrogen, and which might be, for instance, 7—10 ($H = 1$). Such a gas would be liquefied by means of liquid oxygen or air as cooling agent, and afterwards used as a recognised menstruum in the liquefaction of hydrogen. Science will probably have to wait a very long time before this suggestion of how to get "static" liquid hydrogen is realised. The proposal Wroblewski made in 1884 of using the expansion of hydrogen as a cooling agent to effect the change of state, is far more direct and practicable.

Liquid Hydrogen Jet and Solid Hydrogen.—Hydrogen, cooled to $-194°$ ($80°$ abst. *t.*), the boiling point of air, is still at a temperature which is two and a half times its critical temperature, and its direct liquefaction at this point would be comparable to that of air taken at $60°$, and liquefied by the apparatus just described. In other words, it is more difficult to liquefy hydrogen (assuming it to be supplied at the temperature of boiling air) than it is to produce liquid air starting from the ordinary atmospheric conditions. Now, air supplied at such a high temperature greatly increases the difficulty and the time required for liquefaction. Still it can be done, even with the air supply at $100°$, in the course of seven minutes, and this is the best proof that hydrogen, if placed under really analogous conditions, namely at $-194°$ must also liquefy with the same form of apparatus. It is almost needless to say that hydrogen under high compression at the temperature of $15°$ C. passed through such a regenerating coil, produced no lowering of temperature. Hydrogen cooled to $-200°$ was forced through a fine nozzle under 140 atmos. pressure, and yet

* The compressed gas mixture at above $-210°$ was expanded into a large cooled vacuum vessel.

† Phil. Mag. 1895.

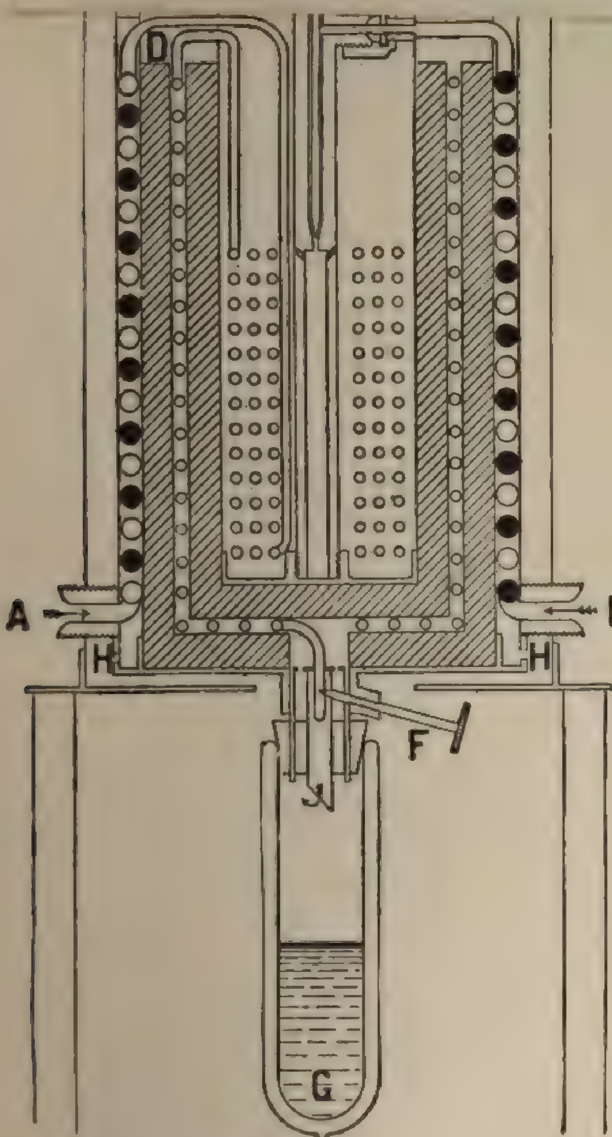

FIG. 1.

LABORATORY LIQUEFACTION APPARATUS FOR THE PRODUCTION OF LIQUID OXYGEN, &c.

A, air or oxygen inlet; B, carbon dioxide inlet; C, carbon dioxide valve; D, regenerator coils; F, air or oxygen expansion valve; G, vacuum vessel with liquid oxygen; H, carbon dioxide and air outlet; ○, air coil; ●, carbon dioxide coil.

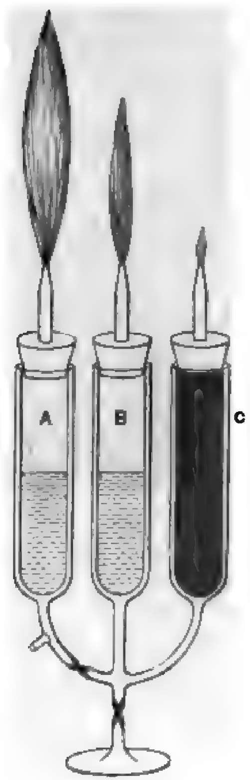

Fig. 2.

Liquid Ethylene Flame Calorimeter.

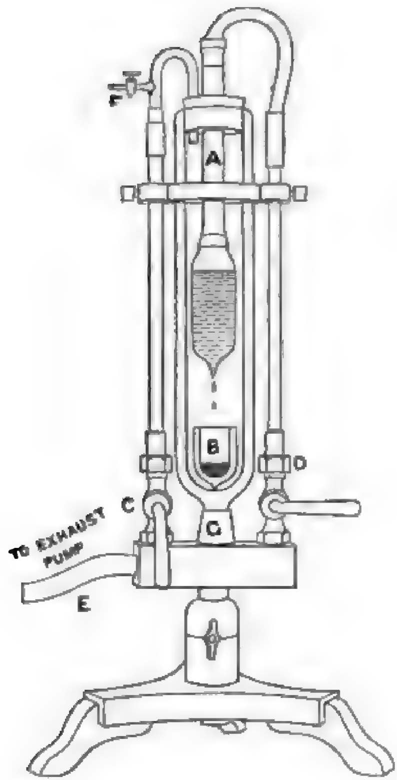

Fig. 3.

Lecture Apparatus for Projecting the Liquefaction of Air at Atmospheric Pressure, and its Solidification.

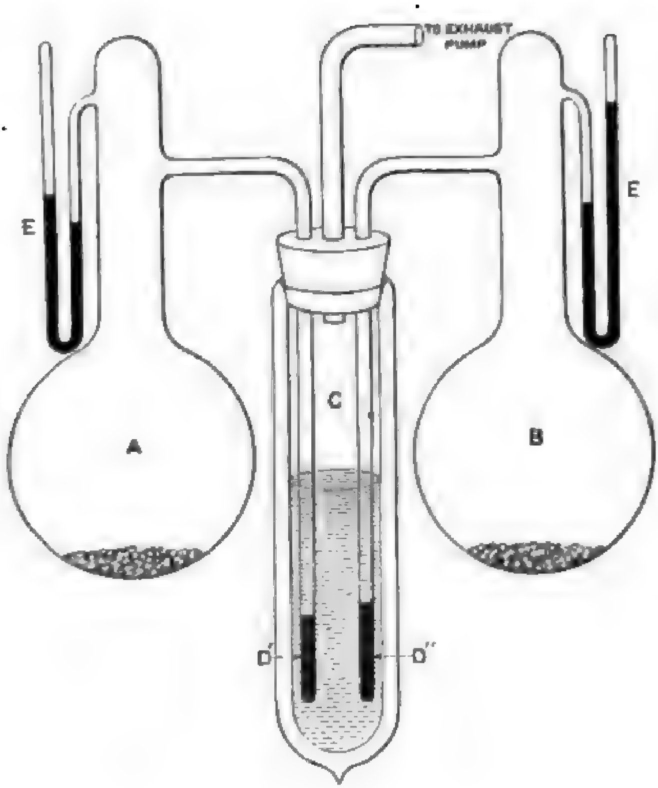

Fig. 4.

Plan of comparing Relative Temperatures of Liquefaction and Small Vapour Pressures.

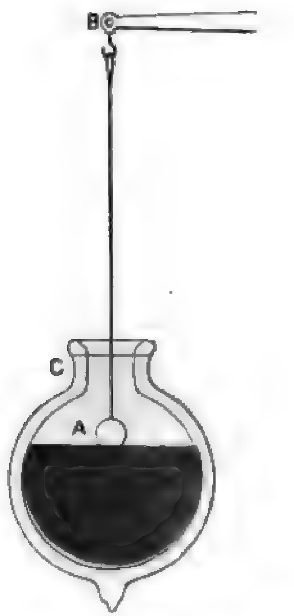

Fig. 5.

Specific Gravity Vacuum Globe.

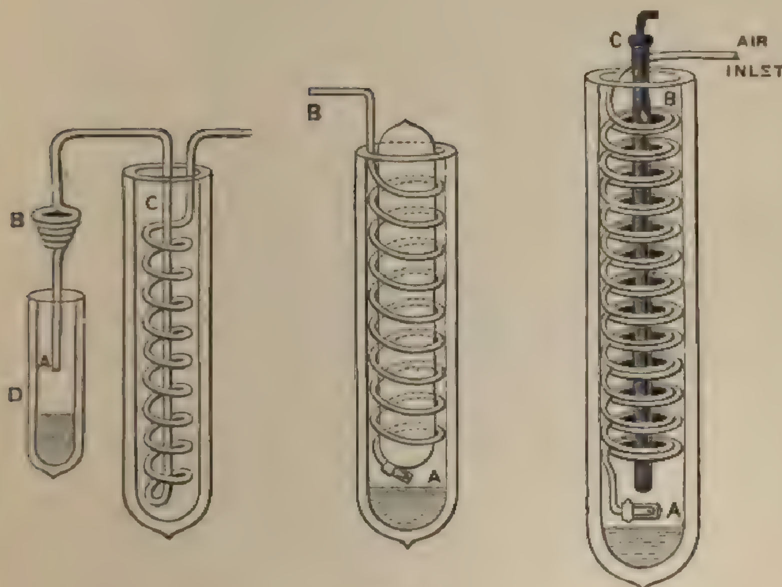

FIG. 6.

DIFFERENT ARRANGEMENTS OF REGENERATING COILS.

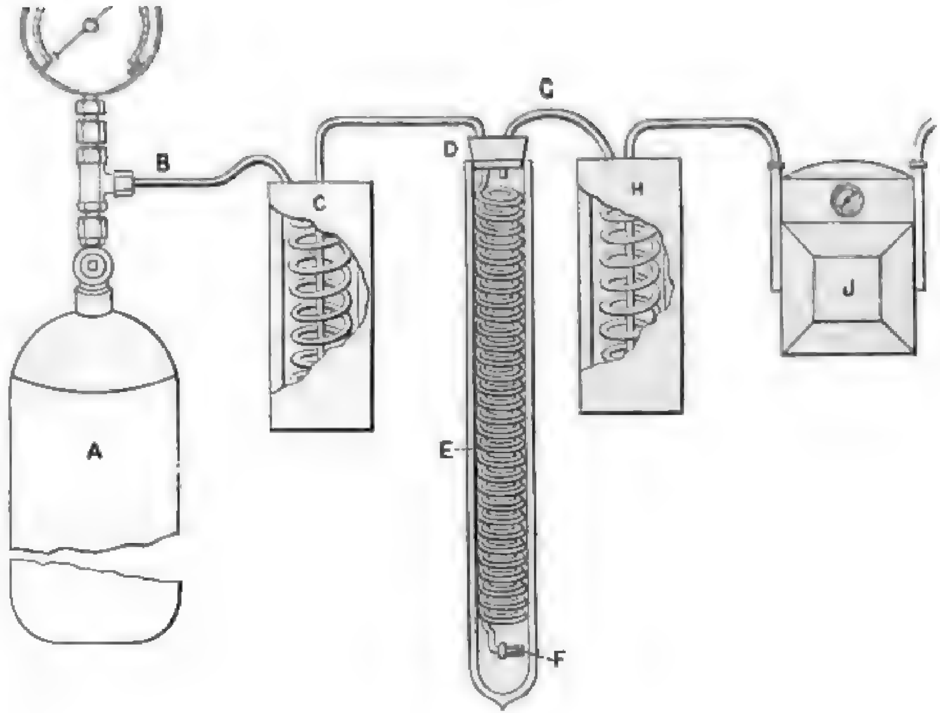

Fig 7.

Plan of Apparatus used in Measuring Rate of Passage of Gas at High Pressure through a Small Pin Hole.

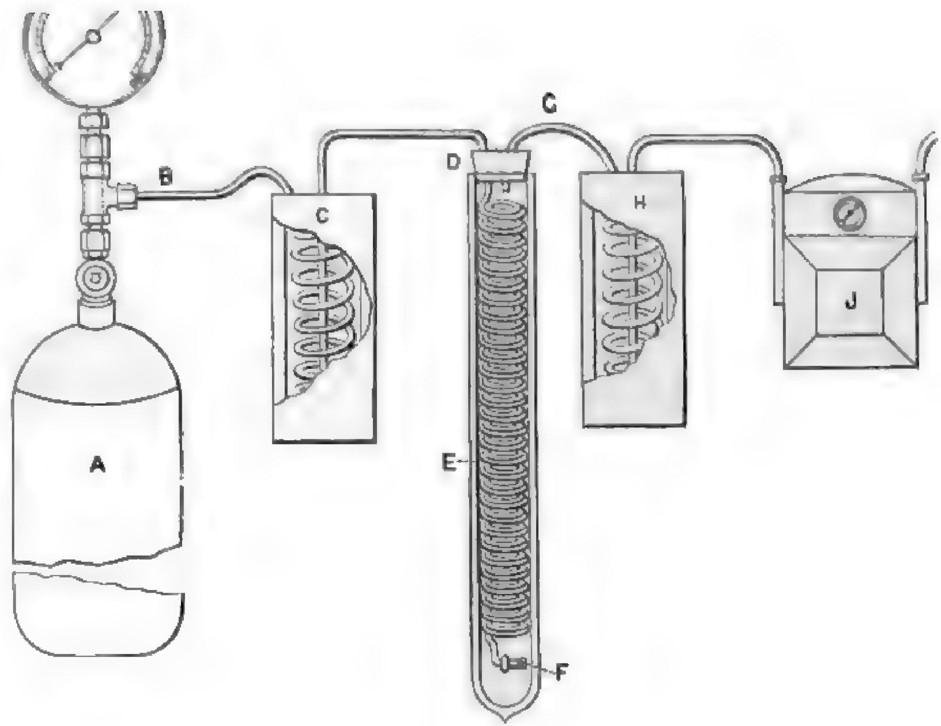

Fig 7.

Plan of Apparatus used in Measuring Rate of Passage of Gas at High Pressure through a Small Pin Hole.

no liquid jet could be seen. If the hydrogen contained a few per cent. of oxygen the gas jet was visible, and the liquid collected, which was chiefly oxygen, contained hydrogen in solution, the gas given off for some time being explosive.

If, however, hydrogen, cooled by a bath of boiling air, is allowed to expand at 200 atmos. over a regenerative coil previously cooled to the same temperature, and similar in construction to that shown in Fig. 8,* a liquid jet can be seen after the circulation has continued for a few minutes, along with a liquid which is in rapid rotation in the lower part of the vacuum vessel. The liquid did not accumulate, owing to its low specific gravity and the rapid current of gas. These difficulties will be overcome by the use of a differently shaped vacuum vessel, and by better isolation. That liquid hydrogen can be collected and manipulated in vacuum vessels of proper construction cannot be doubted. The liquid jet can be used in the meantime (until special apparatus is completed for its collection) as a cooling agent, like the spray of liquid air obtained under similar circumstances, and this being practicable, the only difficulty is one of expense. In order to test, in the first instance, what the hydrogen jet could do in the production of lower temperatures, liquid air and oxygen were placed in the lower part of the vacuum tube just covering the jet. The result was that in a few minutes about 50 c.c. of the respective liquids were transformed into hard white solids resembling avalanche snow, quite different in appearance from the jelly-like mass of solid air got by the use of the air pump. The solid oxygen had a pale, bluish colour, showing by reflection all the absorption bands of the liquid. The temperatures reached, and other matters, will be dealt with in a separate communication. When the hydrogen jet was produced under the surface of liquid air, the upper part of the fluid seemed to become specifically lighter, as a well marked line of separation could be seen travelling downwards. This appearance is no doubt due in part to the greater volatility of the nitrogen and the considerable difference in density between liquid oxygen and nitrogen. In a short time solid pieces of air floated about, and the liquid subsequently falling below the level of the jet, hydrogen now issued into a gaseous atmosphere containing air, which froze solid all round the jet. There is no reason why a spray of liquid hydrogen at its boiling point in an open vacuum vessel should not be used as a cooling agent, in order to study the properties of matter at some 20° or 30° above the absolute zero.

Fluorine.—This is the only widely distributed element that has not been liquefied. Some years ago Wallach and Heusler pointed out that an examination of the boiling points of substituted halogen organic compounds led to the conclusion that, although the atomic weight of fluorine is nineteen times that of hydrogen, yet it must in the

* In the figure, A represents one of the hydrogen cylinders; B and C, vacuum vessels containing carbonic acid under exhaustion and liquid air respectively; D, regenerating coil; G, pin-hole nozzle; F, valve.

free state approach hydrogen in volatility. This view is confirmed by the atomic refraction which Gladstone showed was 0·8 that of hydrogen, and from which we may infer that the critical pressure of fluorine is relatively small like hydrogen.* If the chemical energy of fluorine at low temperatures is abolished like that of other active substances, then some kind of glass or other transparent material could be employed in the form of a tube, and its liquefaction achieved by the use of hydrogen as a cooling agent. In any case a platinum vessel could be arranged to test whether fluorine resists being liquefied at the temperature of solid air, and this simple experiment, even if the result was negative, would be of some importance.

During the conduct of these investigations, I have gratefully to acknowledge the able assistance rendered by Mr. Robert Lennox, my chief assistant. Valuable help has also been given by Mr. J. W. Heath.

[J. D.]

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* On the other hand, the exceptionally small refractivity value observed by Lord Rayleigh in the case of helium shows that the critical pressure of this body is proportionately high. It would therefore be more difficult to liquefy than a substance having about the same critical temperature, but possessing a lower critical pressure, like hydrogen.

GENERAL MONTHLY MEETING,

Monday, April 13, 1896.

Sir James Crichton-Browne, M.D. LL.D. F.R.S. Treasurer and Vice-President, in the Chair.

Robert James Forrest, Esq.
Major-General Sir Francis Grenfell, G.C.M.G. K.C.B.
Marcus Warren Zambra, Esq.

were elected Members of the Royal Institution.

The Managers reported, That they had re-appointed Professor James Dewar, M.A. LL.D. F.R.S. as Fullerian Professor of Chemistry.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz. :—

FROM

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WEEKLY EVENING MEETING,

Friday, April 17, 1896.

SIR JAMES CRICHTON-BROWNE, M.D. LL.D. F.R.S. Treasurer and Vice-President, in the Chair.

PROFESSOR G. LIPPMANN, Membre de l'Institut (France).

Colour Photography.

THE problem of colour photography is as old as photography itself. The desire of fixing the colours as well as the design of the beautiful image thrown on the screen of the camera, very naturally occurred to the earliest observers. Since the beginning of this century three distinct solutions of the problem have been realised.

The first solution, not quite a complete one, is founded on the peculiar properties of a silver compound, the violet subchloride of silver. E. Becquerel (1860) converted the surface of a daguerreotype plate into this silver compound, and by projecting on it the image of the solar spectrum, and other objects, obtained good coloured impressions Poitevin substituted paper for the silver plate as a substratum. No other substance has been discovered that can play the part of the subchloride of silver. Moreover the image is not fixed, in the photographic sense of the word; that is, the coloured impression is retained for any length of time in the dark, but it is blotted out by the action of daylight. The reason of it is this: the Becquerel images are formed by coloured silver compounds, which remain sensible to light; so that they are destroyed by the continued action of light, in virtue of the same action which gave them birth. Despite the numerous experiments made by Becquerel, Poitevin, Zenker and others, no substance has been found that is capable of destroying the sensibility of the subchloride for light without at the same time destroying its colour.

The second method for colour photography is an indirect one, and may be called the three-colour method. It was invented in France by Ch. Cros, and at the same time by M. Ducos du Hauron (1869). German authorities claim the priority of the idea for Baron Bonstetten. Three separate negatives (colourless) are taken of an object through three coloured screens. From these three positives (equally colourless) are made; and, lastly, the colour is supplied to these positives by means of aniline dyes or coloured inks. Thus three coloured monochromatic positives are obtained, which by superposition give a coloured image of the model. In the ingenious process lately invented by Prof. Joly, the three negatives, and appa-

rently the corresponding three positives, are obtained interwoven on one and the same plate. The three-coloured method can give a very good approximation to the truth, and has probably a great future before it. We may call it, nevertheless, an indirect method, since the colours are not generated by the action of light, but are later supplied by the application of aniline dyes or other pigments. Moreover, the choice of these pigments, as well as of the coloured screens through which the negatives have been obtained, is in some degree an arbitrary choice.

The third and latest method by which colour photography has been realised is the interferential method, which I published in 1891, and the results of which I beg to lay before you this evening. It gives fixed images, the colours of which are due to the direct action of the luminous rays.

For obtaining coloured photographs by this method, only two conditions are to be fulfilled. We want (1) a transparent grainless photographic film of any kind, capable of giving a colourless fixed image by the usual means; and (2) we want a metallic mirror, placed in immediate contact with the film during the time of exposition.

A mirror is easily formed by means of mercury. The photographic plate being first enclosed in a camera slide, a quantity of mercury is allowed to flow in behind the plate from this small reservoir, which is connected with the slide by a piece of india-rubber tubing.* The slide is then adapted to the camera, and the action of light allowed to take place. After exposure the slide is separated from the camera, the mercury reservoir lowered so as to allow the mercury to flow back into it; the photographic plate is then taken out, developed and fixed. When dry, and examined by reflected light, it appears brilliantly coloured.

The sensitive film may be made either of chloride, iodide or bromide of silver, contained in a substratum either of albumen, collodion or gelatine. The corresponding developers, either acid or alkaline, have to be applied; the fixation may be cyanide or bromide of potassium. All these processes I have tried with success. For instance, the photograph of the electric spectrum now projected before your eyes, has been made on a layer of gelatino-bromide of silver, developed with amidol, and fixed with cyanide of potassium.

As you see, bright colour photographs may be obtained without changing the technique of ordinary photography: the same films, developers and fixators have to be employed; even the secondary operations of intensification and of isochromatisation are made use of with full success. The presence of the mirror behind the film during exposure makes the whole difference. From a chemical point of view nothing is changed, the result being a deposit of reduced silver left in the film, a brownish, colourless deposit. And yet the

* The glass of the photographic plate has to be turned towards the objective, the film in contact with the metallic mirror.

presence of a mirror during exposure causes the colourless deposit to show bright colours. Of course we want to know how this is done; we require to understand the theory of those colours.

We all know that colourless soap-water gives brilliant soap-bubbles; the iridescence of mother-o'-pearl takes birth in colourless carbonate of lime; the gorgeous hues of tropical birds are simply reflected from the brownish substance which forms the feathers. Newton discovered the theory of these phenomena, and subjected them to measurement; he invented for the purpose the experiment called by the name of Newton's rings. Newton showed, as you know, that when two parallel reflecting surfaces are separated by a very short interval, and illumined by white light, they reflect only one of the coloured rays which are the constituents of white light. If, for instance, the interval between the reflecting surfaces is only $\frac{2}{10000}$ of a millimetre, violet rays are alone reflected, the rest being destroyed by interference: that is, the two surfaces send back two reflected rays whose vibrations interfere with one another, so as to destroy every vibration except that which constitutes violet light. If the interval between the reflecting surfaces be augmented to $\frac{3}{10000}$ millimetre, the destruction of vibration takes places for every vibration except that of red light, which alone remains visible in this case.

If we consider now this photograph of the spectrum, and especially the violet end of the image, we find that this is formed by a deposit of brown reduced silver. In the case of an ordinary photograph, this deposit would simply be a formless cloud of metallic particles; here the cloud has a definite, stratified form; it is divided into a number of thin, equidistant strata, parallel to the surface of the plate, and $\frac{2}{10000}$ millimetre apart. These act as the reflecting surfaces considered by Newton, and as they are at the proper distances for reflecting violet rays, and these alone, they do reflect violet rays.

The red extremity of the photograph is equally built up of strata which act in a like manner; only their distance intervals here amount to $\frac{3}{10000}$ millimetre, and that in the proper interval for reflecting red light. The intermediate parts of the spectral image are built up with intermediate values of the interval, and reflect the intermediate parts of the spectrum.

The appearance of colour is therefore due to the regular structure above described, imprinted on the photographic deposit. The next question is—How has this very fine, peculiar and adequate structure been produced?

It is well known that a ray of light may be considered as a regular train of waves propagated through the ether, in the same way as waves on the surface of water. The distance between two following waves is constant, and termed the wave-length; each sort of radiation, each colour of the spectrum, being characterised by a particular value of the wave-length. Now when a ray of light falls on a sensitive film, this train of waves simply rushes through the film with a velocity of about 300,000 kilometres per second; it impresses

the film more or less strongly, but leaves no record of its wave-length, of its particular nature or colour, every trace of its passage being swept out of form by reason of its swift displacement. The impression therefore remains both uniform and colourless. Things change, however, as soon as we pour in mercury behind the plates, or otherwise provide for a mirror being in contact with it. The presence of the *mirror* changes the propagated waves into *standing* waves. The reflected ray is, namely, thrown back on the incident ray, and interferes with its motion, both rays having equal and opposite velocities of propagation. The result is a set of standing waves—that is, of waves surging up and down, each in a fixed plane. Each wave impresses the sensitive film where it stands, thus producing one of these photographic strata above alluded to. The impression is latent, but comes out by photographic development. Of course the distance between two successive strata is the distance between two neighbouring waves; this, theory shows, is exactly half the wave length of the impressing light. In the case of violet, for instance, the wave-length being $\frac{4}{10000}$ millimetres, half the wave-length in the above-quoted distance of $\frac{2}{10000}$ millimetres; this, therefore, is at the same time the interval between two standing waves, in the case of violet light the interval between two successive photographic strata, and at last it is the interval required to exist, according to Newton's theory, for the said strata reflecting voilet rays, and making these alone apparent when illuminated by white light.

The colours reflected by the film have the same nature and origin as those reflected by soap-bubbles or Newton's rings; they owe their intensity to the great number of reflecting strata. Suppose, for instance, the photographic film to have the thickness of a sheet of paper (one-tenth of a millimetre), the fabric built in it by and for a violet ray is five hundred stories high, the total height making up one-tenth of a millimetre. Lord Rayleigh, in 1887, has proved *à priori* that such a system is specially adapted to reflect the corresponding waves of light.

How are we now to prove that the above theory is really applicable to the colour photograph you have seen? How can we demonstrate that those bright colours are due not to pigments, but to the interference, as in the case of soap-bubbles? We have several ways of proving it.

First of all, we are not bound to the use of a peculiar chemical substance, such as Becquerel's subchloride of silver; we obtain colour with a variety of chemicals. We can, for instance, dispense entirely with the use of a silver salt; a film of gelatine or coagulated albumen impregnated with bichromate of potash, then washed with pure water after exposure, gives a very brilliant image of the spectrum.

Secondly, the colours on the plate are visible only in the direction of specular reflection. The position of the source by which we illumine the photograph being given, we have to put the eye in a corresponding position, so as to catch the regularly reflected rays. In

every other position we see nothing but a colourless negative. Now, as you are aware, the colours of pigments are seen in any direction. By projecting again a photograph of the spectrum, and turning it to and fro, I can show you that the colours are visible only in one direction.

Thirdly, if we change the incidence of the illuminating rays, that is, if we look at the plate first in a normal direction, then more and more slantingly, we find that the colours change with the incidence exactly as they do in the case of soap bubbles, or of Newton's rings; they change according to the same law and for the same reasons. The red end of the spectrum turns successively to orange, yellow, green, blue and violet. The whole system of colours, the image of the spectrum, is seen to move down into the part impressed by the infra-red. This is what we expect to happen with interference colours, and what again we cannot obtain with pigments.

Fourthly, if while looking at the film normally, we suffer it to absorb moisture—this can be done by breathing repeatedly on its surface—we see that the colours again change, but in an order opposite to that above described. Here the blue end of the spectrum is seen to turn gradually green, yellow, orange, red, and finally infra-red, that is, invisible. The spectrum this time seems to move up into the ultra-violet part of the improved film. By suffering the water to evaporate, the whole image moves back into its proper place; this experiment may be repeated any number of times.

The same phenomenon may be obtained with Newton's apparatus, by slowly lifting the lens out of contact with the plane surface. The explanation is the same in both cases. The gelatine swells up when imbibing moisture. If we consider, for instance, the violet of the spectrum, the small intervals between the strata corresponding to violet rays, gradually swell up to the values proper for green, and for red, and for infra-red; green, then red, then infra-red are therefore successively reflected.

We will wet this photograph of the spectrum with water, project it on the screen, and watch the colours coming back in the order prescribed by theory.

It is necessary to use a transparent film, since an opaque one, such as is commonly in use, would hide the mirror from view; the sensitive substance must be grainless, or at least the grains must be much finer than the dimensions of the strata they are intended to form, and therefore wholly invisible. The preparation of transparent layers gave me at first much trouble; I despaired for years to find a proper method for making them. The method, however, is simply thus: if the sensitive substance (the silver bromide, for instance) be formed in presence of a sufficient quantity of organic matter, such as albumen, gelatine or collodion, it does not appear as a precipitate; it remains invisible; it *is* formed, but seems to remain dissolved in the organic substratum. If, for instance, we prepare a film of albumeno-iodide in the usual way, only taking care to lessen the proportions of

iodide to half per cent. of the albumen, we get a perfectly transparent plate, adapted to colour photography.

We want now to go a step further. It is very well for physicists to be contented with working on the spectrum, since that contains the elements of every compound colour; but we all desire to be able to photograph other objects than the spectrum—common objects with the most compound colours. We have again but to take theory as a guide, and that tells us that the same process is able to give us either simple or compound colours. We have then to take a transparent and correctly isochromatised film, expose it with its mercury backing, then develop and fix it in the usual way; the plate, after drying, gives a correct coloured image of the objects placed before the camera. Only one exposure, only one operation is necessary for getting an image with every colour complete.

A plausible objection was offered at first to the possibility of photographing a mixture of simple colours. The objection was this: a ray of violet gives rise to a set of strata separated by a given interval; red light produces another set of strata with another interval; if both co-exist, the strata formed by the red are sure to block out here and there the intervals left between the strata formed by the violet. Is it not to be feared that one fabric will be blurred out by the other, and the whole effect marred? The confusion would be still worse if we consider the action of white light, which contains an infinity of simple components; every interval here is sure to be blocked up.

Mathematical analysis, however, shows this objection to be unfounded; we have great complexity, but not confusion. Every compound ray, both coloured and white, is faithfully rendered. As an experimental proof of this, we will project on the screen photographs of very different objects, namely, stained glass windows, landscapes from nature, a portrait made from life, and vases and flowers.

That the colours here observed are due to interference, and not to the presence of pigments, can be shown in the same way as with the spectrum. Here, again, we observe that the colours are visible only in the direction of specular reflection, that they change with the angle of incidence, that they change and disappear by wetting, and reappear by drying. Pigments remain equally visible and unaltered in colour under every incidence. If we attempted to touch up one of our photographs with oil or water-colours, the adulterated place would stand out on a colourless background by merely observing by diffused light. It is therefore impossible either to imitate or touch up a colour photograph made by the above-described interferential method.

[G. L.]

WEEKLY EVENING MEETING,

Friday, April 24, 1896.

Basil Wood Smith, Esq. F.R.A.S. F.S.A. Vice-President, in the Chair.

Professor G. V. Poore, M.D. F.R.C.P.

The Circulation of Organic Matter.

It is quite impossible to define "organic matter," or to indicate the line, if there be any, between organic and inorganic.

Organic matter is the material of which living things are made. When a chemist analyses anything which is the product of life, whether vegetable or animal, he often speaks of his incombustible residue or ash as "inorganic matter," but this is clearly an arbitrary use of the term, for this incombustible residue has formed an indispensable part of one living thing, and may in due time be incorporated with other living things as something which they cannot do without.

It may well be that everything of which we have knowledge (even including the igneous rocks) has at one time or another formed part of a living organism, and it is certain that a large proportion of the commoner chemical elements may form a part, more or less indispensable, of the bodies and framework of plants or animals.

Oxygen, hydrogen, nitrogen, carbon, chlorine, sulphur, phosphorus, iron, sodium, potassium and calcium seem to be indispensable to almost every living thing. Many more of the elements are constantly found in some organisms, while others, such as lead, mercury, silver, &c., may be temporarily incorporated with living bodies.

We shall deal to-night mainly with those elements which are pre-eminently mobile, which are constantly changing and exchanging, combining and separating, and which are readily combustible. For practical purposes one might indeed use the terms "organic" and "combustible" to signify the same thing.

With regard to solid matter, the power of readily circulating implies a readiness of combustibility, but it must be remembered that there is no hard line between combustible and incombustible. This is a matter of temperature, and many things which are incombustible here are said to be blazing in the sun.

The combustion of organic matter may take place slowly or with moderate rapidity, or with explosive violence.

When we burn coal, which is a vegetable product, we find that the carbon and hydrogen escape as carbonic acid and water, accom-

panied by nitrogen, sulphuric acid and volatile hydrocarbons. The residue consists mainly of silica and alumina, which are removed from the furnace in the form of clinker and ash. The water ultimately returns to the earth in the form of rain or dew, the carbonic acid is ultimately absorbed by green plants, and, by stimulating the growth of these, helps to furnish us with more combustible material, while the residue is almost a waste product. Thus, in this example we find that the carbon and watery vapour readily "circulate," while the residue can only do so after a long interval of time, and is practically lost. The volatile hydrocarbons and sulphuric acid, being poisonous to herbage, are a source of practical loss rather than gain.

Let us take next the case of an animal, which is really a living furnace, browsing in a field; as it browses we may often see the breath, which is the smoke of this furnace laden with carbonic acid and water, escaping from its mouth and nostrils, and it is probable that the green leaves of the herbage absorb this carbonic acid almost as soon as it escapes, and, appropriating the carbon, return oxygen to the animal to help its respiration and combustion. The animal as it eats continues to grow and increase in bulk and value, whereas the artificial furnace in which the coal is burnt tends steadily to wear out and decrease in value. As it browses and grows, the droppings of the animal nourish the herbage which here and there, by patches of more vigorous growth and deeper green, afford sure evidence of the value of these waste products.

In this arrangement there is no waste, for both the animal and the herbage, by a process of mutual exchange and the circulation of organic matter, increase in value.

Not only is there no waste, but, strange as it may seem, there is a positive gain, with no loss whatever. The furnace and the fuel are both increased! This increase can only be apparent, and not real, for it is well known that although we may alter the form of matter, we can add nothing to and subtract nothing from the sum total of the world.

One would say that this apparent increase is due to the stimulating effect of the excreta upon the soil, which enables us to draw something extra from that inexhaustible storehouse of plant-food and water, and enables the animal to use these materials, instead of allowing them to drain to the springs, and so find their way to the sea. We know that a far greater proportion of the rainfall percolates through barren soil than through soil bearing crops. If this be so, there is a practical increase of the land at the expense of the water.

Again, we must remember that our knowledge of the sources of the gases of the atmosphere is not complete. It may be that all the oxygen of the air is furnished by the green leaves of plants, and all the carbonic acid by processes of respiration and combustion, but we are by no means sure of this. Of the sources of the atmospheric nitrogen we know nothing. Now it is certain that much of the carbon of the atmosphere is appropriated by the plants, and much of the

oxygen by the animal. If among the herbage there be plants of clover, it is now certain that much of the atmospheric nitrogen will be drawn into the soil to nourish these plants and generally to increase its fertility. Whether the return of oxygen, carbon and nitrogen is, in the long run, equal to the intake we cannot tell.

When, however, we ponder upon the gradual increase of vegetable soil or humus with which the bare rocks have been clothed in the course of ages, it is almost impossible not to come to the conclusion that the humus and with it the fertility of the soil has steadily increased at the expense of the sea on the one hand, and, possibly, of the atmosphere on the other. To put the matter in the form of question and in other terms, "Does the *Lithosphere* increase at the expense of the *Atmosphere* and the *Hydrosphere*?" Does the land increase at the expense of sea and air? Be this as it may, it seems certain that by scrupulous return to the soil of all that comes out of it the resources of nature are made increasingly available for the benefit of man.

When organic matter is mixed with water, a process of putrefaction and fermentation is started, and the organic matter, instead of undergoing oxidation, is reduced, and among the commoner products of this process are ammonia with sulphuretted hydrogen and marsh-gas, which are both combustible. These processes furnish us with other combustible matters among the commonest of which are the alcohols, the familiar products of fermentation.

It is interesting to note the tendency of organic matter, when mixed with water, to give rise to explosive and combustible products. Explosions in cesspools and sewers have occurred many times. When wet hay is stored in stack it catches fire. When we stir the mud at the bottom of a pond or river, bubbles of combustible marsh-gas rise to the surface. The coal measures are due to the storing under water of semi-aquatic plants which have been preserved by being silted up, and we know that coal is full of olefiant gas, marsh-gas, sulphuretted hydrogen and carbon monoxide, which are all combustible, and that the carbonaceous residue, charged with volatile and combustible hydro-carbons, forms the chief fuel of the civilised world. Peat is formed in ways analogous to that of coal, and the so called mineral oils are certainly the products of organic matter which has been silted up.

These subterranean stores of combustibles, all of organic origin, are, as we know, prodigious in quantity. Nobody can predict the time which it will take to exhaust the coal measures of the world, and we know for a fact that the sacred fires of Baku on the Caspian, fed by subterranean reservoirs of naphtha, have been burning for centuries.

When we see the end of a tin of "preserved meat" bulged, we know that the gas-forming organisms have been at work within, and when the bed of the lower reaches of the Mississippi rises as a small mud mountain, spluttering with carburetted hydrogen, we know

that analogous forces have been in operation. It seems, indeed, to be a law of nature that the ultimate destiny of organic matter is to "circulate," and that if it do not do so quietly, as in the ordinary processes of nutrition in plants and animals, it merely bides its time and ultimately attains its end with more or less destructive violence.

Nitre (nitrate of potash or nitrate of soda) is an organic product, and sulphur is an essential constituent of all or nearly all organisms. Of the three ingredients of gunpowder, two (charcoal and saltpetre) are, it is certain, of exclusively organic origin, and the third, sulphur, may be so also.

All the common combustibles with which we are familiar are certainly of organic origin, and one is almost forced to the conclusion that in this world life must have preceded combustion. If we are to explain what *has been* by what *is*, such a conclusion is irresistible. Are we quite sure that volcanoes, which are seldom far from the sea, are not fed by old deposits of organic matter which has collected in the primeval ocean, and like the more recent coal measures, have been silted up.

What has been the destiny of the protoplasm of the countless animals and plants which are found in geologic strata? What part have ancient microbes had in the formation and disruption of the successive layers of which this earth is formed? These are questions which force themselves upon the mind, but which I will not now attempt to answer. This biological view of the cosmogony which subjects the world equally with all that is upon it to the laws of development, evolution and decay, does not, I believe, present so many difficulties as might at first sight appear.

"Omne vivum ex vivo" is a law of nature, and all organic bodies spring from organic antecedents. Organic matter is our capital in this world, and the more frequently we can turn it over, and the more quickly and efficiently we can make it circulate, the more frequent will be our dividends. If we burn organic matter, we may get a good dividend of energy, but nothing further is to be expected. The construction of the furnace involves an outlay of capital which steadily diminishes as the furnace wears out by frequent use. If we burn organic matter merely to be rid of it, we spend our money for the sole purpose of dissipating our capital. The function of fire is to destroy and sterilise.

If we mix organic matter with large quantities of water, we have to encounter all the evils and annoyance of putrefaction, and if, when so mixed, we send it to the sea, we have no material gain of any kind. We spend our money for the purpose of dissipating our capital.

We may place the water containing the organic matter upon the land, and in tropical countries this is done with excellent effect for the production of rice, a semi-aquatic plant which, according to Professor Georgeson, Professor of Agriculture in the Imperial University of Tokio, is said to prefer its nitrogen in the form of ammonia. The

same authority states that nitrification does not take place under water, and careful experiments carried out at Tokio show that sulphate of ammonia is a much better manure for irrigated rice than nitrate of soda.

In our damp climate sewage farming has proved a dismal failure, and the difficulties seem to increase with the quantity of water which has to be dealt with. Excess of water drowns the humus, and nitrification cannot go on in a soil the pores of which are closed by excess of moisture.

The living earth, teeming with aerobic microbes, must be allowed to breathe. It needs for this purpose a certain amount (about 30 per cent.) of moisture, but it stands drowning no better than a man does, and if it be drowned, agricultural failure is inevitable.

If we carefully return to the upper layers of the humus, in which air and microbes exist in plenty, the residue of everything which we extract from it, we inevitably increase the thickness of the humus and its fertility. Our capital increases, and our dividends increase and recur with a frequency which depends upon the climate.

With thrifty and high cultivation it may, indeed, prove profitable to compensate defects of climate by the use of glass and artificial heat.

The part played in the economy of nature by fungi and bacteria—the new learning of the last half-century—is an addition to human knowledge which is destined to revolutionise our views of many natural phenomena. It has already exercised enormous propulsive power on human thought, and has stimulated our imaginations scarcely less than when, to use the words of Froude, "the firm earth itself, unfixed from its foundations, was seen to be but a small atom in the awful vastness of the universe."

This knowledge has provided us with a new world peopled with organisms in numbers which, like the distances of the astronomers and the periods of the geologists, are really unthinkable by the human mind. Their variety also, both in form and function, is, for practical purposes, infinite.

When, with the help of the many inventions of the optician and the dyer, we catch a glimpse of things which a few years back were "undreamt of in our philosophy," and when we reflect that these organisms are certainly the offspring of "necessity," and are probably mere indications of infinities beyond, we cannot be too thankful for the flood of light which these discoveries have shed upon the enormity of human ignorance.

The lower animals and the lower vegetable organisms (fungi and bacteria) co-operate in a remarkable way in the circulation of organic matter.

In the autumn the gardener, with a view to what is called "leaf mould," sweeps the dead leaves into a heap where they are exposed to air and rain. This heap when thus treated gets hot, and last

autumn I found that the temperature of such a heap had risen in the course of a week or so to 104° F., and remained at a temperature considerably above that of the surrounding air during the whole winter. On turning it over after a month or so one found in it a large number of earth worms and endless fungoid growths visible to the naked eye, and one felt sure that it was swarming with countless millions of bacteria, invisible except to the highest powers of the microscope. In the beginning of March this heap, much reduced in size, was spread loosely over a patch of ground which was previously dug. If one examined that ground to-day one would scarcely recognise the structure of leaves, and in a few weeks more it will have become nothing but ordinary garden mould, and anything planted in it will grow with vigour. This is a familiar every-day fact.

We know also that noisome filth spread over a field by the farmer in the autumn or winter loses its offensiveness in a few days, and by the spring neither our eyes or noses give us any clue to the cause of the fertility of the field which is covered with ordinary "mould." This process of "humification" is largely due to earth worms and other earth dwellers, which pass the earth repeatedly through their bodies, and in doing so reduce it to a very fine powder. I have upon the table some worm castings picked off a lawn, and which, after being slowly dried, have been gently sifted through muslin. Those who have never examined a worm casting in this way will be interested to see of what an impalpable dust the greater part is composed, and will also note the considerable size of the pieces of flint and grit which the animal has used in its living mill, and which have been separated by the muslin sieve.

These castings are full of microbes, and those who will take the trouble to scatter the smallest conceivable pinch of this impalpable dust upon a sterilised potato, after the manner and with all the precautions familiar to bacteriologists, will obtain an abundant and varied growth of bacteria and moulds, which will completely baffle their powers of enumeration and discrimination.

The greatest hindrance in the bacterial examination of the soil is this *embarras de richesses*, which makes the isolation of different species a matter of extreme difficulty.

The bacteria exist in the soil in countless millions, but it must be remembered that they get fewer as we go deeper. The first few inches of the soil are, in the matter of bacterial richness, worth all the rest, and at a depth of five or six feet they appear to be almost non-existent. The practical lesson which we have to lay to heart in applying this knowledge is that the upper layers of the soil are the potent layers in bringing about the circulation of organic matters, and that if we wish to hasten this process we must be careful to place our organic refuse near the surface and not to bury it deeply, a process by which the circulation is inevitably delayed or practically prevented. If we bury it deeply we not only get no good, but we may get harm by poisoning our wells and springs.

It is the same with organic liquids. If these be poured on the surface, the "living earth" (i.e. the humus stuffed with animal and microbial life) purges them of their organic matter, and transmits a relatively pure liquid to the deeper layers. If they be taken to the barren subsoil direct, as in underground sewers and cesspools, they escape the purifying action of air and aerobic organisms, and inevitably poison the water. Filthy liquids accumulating in cesspools and leaking *under pressure* to our wells have cost us health and money incalculable.

Liquids poured upon the surface cannot, owing to the crumby nature of the humus, exert any appreciable hydraulic pressure. This is a fact of huge importance in the practical management of organic refuse.

All effete organic matter instantly becomes the prey of animals and plants. The dead body of an animal teems with life—"Le roi est mort, vive le roi." M. Megnin, a skilled entomologist and a member of the French Academy of Medicine, has made a study, which is full of gruesome interest, of the living machinery which makes away with the bodies of animals not buried but exposed to the air and protected from beasts of prey.

M. Megnin shows that the destruction of the animal is accomplished in no haphazard fashion, but that successive squadrons of insects are attracted by the successive stages of putrefaction.

The first squadron which arrives, sometimes before death and always before putrefaction, consists entirely of dipterous insects, house-flies and their relative the blow-fly.

The next squadron are also diptera, and are said to be attracted by the commencing odour of decomposition. These squadrons use the carcase as a procreant cradle, and thus ensure the nourishment of the larvæ so soon as they are hatched. Amongst these flesh-seeking flies there are said to be specialists which prefer the flesh of particular animals.

The third squadron is attracted when the fat begins to undergo an acid fermentation. These consist of coleoptera and lepidoptera, beetles and butterflies, and among them is *Dermestes Lardarius*, the Bacon Beetle.

When the fats become cheesy, the diptera reappear, and among them is *Pyophila Casei*, the fly which breeds jumpers in cheese, who is accompanied by a beetle the larvæ of which are connoisseurs of rancidity.

When the carcase becomes ammoniacal, black and slimy, it is visited by a fifth squadron of flies and beetles.

And these are succeeded by the sixth squadron, consisting of acari or mites, whose function it is to dry up the moisture and reduce the carcase to a mummy-like condition.

The dried carcase proves attractive to the seventh squadron, consisting of beetles and moths, some of which are the familiar pests of

the housewife, the furrier, and the keepers of museums. These animals gnaw the softer parts, such as ligaments, and leave nothing but a fine powder behind them, which is in fact their dung.

The last and eighth squadron consists solely of beetles, which clean up the debris, in the shape of dung, shells, pupa cases, &c., of the seven squadrons which have preceded them.

M. Megnin, being an entomologist and not a bacteriologist, deals exclusively with the insects concerned in making away with a carcase, but it is evident that bacteria work hand in hand with them.

There are many other instances which may be quoted of the co-operation of fungi with other organisms, and it is only of late years that we have appreciated the fact of *symbiosis* or the living together of two organisms for the mutual benefit of each. This fact was first pointed out in so-called lichens, which are now shown to be complex bodies consisting of a fungus and an alga, living in symbiotic community for the mutual benefit of each.

It was next shown that the Papilionaceous Leguminosæ are unable to flourish without certain bacterial nodules which grow upon their roots, and by the instrumentality of which they can appropriate the nitrogen of the air, and thus the fact, familiar for centuries, that the leguminosæ leave the ground in a state of great fertility, while they are singularly independent of nitrogenous manures, has been explained.

But if the plants themselves are independent of dung, it is not so, apparently, with the symbiotic nodules, which seem to flourish far more vigorously in rich garden ground than they do in comparatively poor farm land. Thus Sir John Lawes has grown clover in a rich old garden for forty-two years, and has had luxuriant crops every year.

According to my own observation on the scarlet runner bean these nodules are more plentiful upon the roots which grow superficially than upon those which run deeply.

Symbiosis is observable in many plants other than leguminosæ, and it is certain that many of our big forest trees depend for their nourishment upon fungi which grow upon their roots.

By the kindness of my colleague, Professor F. W. Oliver, I am able to show you upon the screen the so-called Mycorhiza as it grows upon the rootlets of the beech.

In the upper left-hand corner is a portion of root showing its characteristic fungoid covering (natural size). To the right is a portion enlarged—the thinner strands behind, being parts of the fungus in the soil without an axis of root. Below is a root apex with fungal sheath enlarged.

The next slide is from a drawing, by Professor Oliver, of *Sarcodes Sanguinea*, the Californian snow plant, a remarkable saprophyte which is destitute of chlorophyll.

The drawing shows the fungal sheath, and, to the right, the epidermis and one cortical layer of the root. The black scales in the

sheath are dead cells in the root cap which remain held in the fungal matrix.

All animals appear to be symbiotic, for we all carry about millions of microbes which must fairly be regarded as junior partners in our economy, and which we cannot do without. The microbe which has been chiefly studied—the *Bacterium Coli commune*—appears to be essential for certain digestive processes which go on in the intestines while we live; and when we die this microbe is active in starting the dead body upon that cycle of events which is one form of the "Circulation of Organic Matter."

Now it is certain that the dung of all animals swarms with bacteria and allied organisms when it leaves the intestines, and it seems highly probable that excrement carries with it the biological machinery which is necessary for its dissolution and ultimate humification.

My friend, Mr. George Murray, the keeper of the Botanical Department of the British Museum, whose learning in fungology is well known, has kindly furnished me with an elaborate list of 139 genera of fungi which flourish on excrement.

Of these 139 genera Mr. Murray has tabulated no less than 628 species which are known to flourish on excrement.

Of the 628 species 226 have been found on the dung of more than one genus of animals, but no less than 402 species of fungi are peculiar to the excrement of only one genus of animals.

Of these 402 species of fungi 91 are peculiar to the dung of the ox; 78 to the horse; 68 to the hare and rabbit; 30 to the dog; 25 to the sheep; 28 to birds; 21 to man; 16 to the mouse; 9 to the deer; 7 to the pig; 7 to the wolf, and 22 to other animals.

This marvellous list is on the table for the inspection of those who are learned in such matters.

This search for fungi in excrement is necessarily incomplete. In Mr. Murray's list it is evident that the greatest number of species have been found in the dung of animals which are domesticated and common, and which offer facilities to the fungologist. The numbers are startling, but when we consider that the dung of every living thing which crawls or burrows, or swims or flies, has properties which are peculiar to it, and which fit it to become the nidus of some peculiar fungoid or bacterial growth, the part played by fungi in the distribution and circulation of organic matter cannot be over-estimated.

The facts which have been recounted, and which seem to show that fungi and bacteria are necessary for the growth and development of even the highest plants and animals, and that fungi and animals are equally necessary for the dissolution of organic matter, seem to point to the conclusion that the correlation of the biological forces in this world is no less exact than the correlation of the physical forces. The uniform composition of the atmosphere, except under special and local conditions, is a fact which points in the same direction.

While it is impossible to over-estimate the debt which agriculture owes to chemistry, we have, nevertheless, learnt from the bacteriologist that there are biological problems underlying the question of fertility, and that a mere chemical estimation of the constituents of organic manure is insufficient, by itself, to fix its manurial value. It is by the agency of bacteria that organic matter is changed into nitrates and other soluble salts, which are absorbed by the roots of plants and serve to nourish them. This change only takes place provided the temperature and moisture are suitable and the ground be properly tilled. Drought and frost arrest the change, and excess of moisture, by closing the pores of the soil, does the same thing.

Organic manures are economical in the long run, because if the weather is adverse they bide their time until the advent of "fine, growing weather." If one season prove unfavourable a large amount of the organic matter remains in the soil to nourish the next crop. This is not the case when soluble chemical manures are used.

That it is necessary to put dung upon the ground if we are to maintain the fertility of the soil, has been the experience of all peoples in every age.

I will now display a diagram which represents by a curve the yearly produce of barley in bushels per acre, grown continuously on the same plots of ground for forty years, but with this difference, that one plot (represented by the upper curve) received 14 tons per annum per acre of farmyard manure, while the other, represented by the lower curve, has been unmanured continuously (Fig. 1). This diagram has been constructed from figures given by Sir John Lawes and Sir Henry Gilbert in the 'Transactions of the Highland and Agricultural Society of Scotland' for 1895. I have replaced fractions by the nearest whole figure. The fluctuations of both these curves are very great, and it will be noticed that they are exactly parallel to each other. This teaches us that weather is the most important factor in agricultural success, and shows the extreme danger to the farmer of "placing all his eggs in one basket," as has been done by the so-called farmers of the far West, who have attempted to grow wheat *only* by the process of scratching the prairie without returning any dung to the soil, and many of whom have been financially swamped by the first bad season.

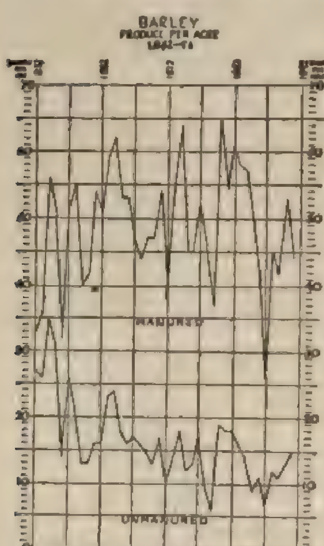


Fig. 1.

Taking the average of the forty years, it will be found that the produce of the manured land averaged 49 bushels per acre per annum, while the unmanured land gave only 16½ bushels.

I might have added to the diagram a third curve showing the produce of that plot of ground which, of all those manured with artificials, gave the highest yield. The yield of this plot for the whole forty years averaged 46 bushels, or only 3 bushels short of the average yield of the plot treated with farmyard manure. If, however, we take the average yield of the three plots for each of the four decades comprising the forty years, the value of the organic matter becomes very manifest. Thus the yield for each decade was with

Farmyard dung ..	44·9	51·5	50·0	51·6
Artificial manure ..	48·7	49·4	42·8	41·5
Unmanured	22·2	17·5	13·7	12·6

It will be observed that the yield from artificial manuring only exceeded the yield from the farmyard plot in the first decade, when it showed an excess of 3·8 bushels. In the other three decades it was deficient by 2·1, 7·2, and 10·1 bushels.

The deficiency of the unmanured plot in each decade, as compared with the farmyard plot, was 22·7, 34·0, 37·3, and 39·0.

These figures are very convincing, and, as practical agriculturalists seem to be now agreed that farming is hopeless without an adequate amount of live-stock to furnish dung, no more need be said upon this head.

But is there no danger in using organic refuse, which may be infective and dangerous, as an application to the land? To this I should say emphatically "No," provided it be put in the upper layers of the soil, and the soil be tilled. Our organic refuse, when allowed to putrefy in water, and to trickle *under pressure* to our wells, or run direct into our sources of drinking water, has turned millions of pounds into the pockets of members of my profession, but when rationally used as a top dressing for the well-tilled soil, it has never, that I am aware of, produced any harm.

I have tried to investigate this matter. Some five years ago I constructed a well five feet deep in the middle of a garden which is plentifully manured with all that is most loathsome to our senses. This well is lined to the very bottom with concrete pipes, further protected by an external coating of concrete; the junctions of the pipes are securely closed by cement, and there is a good parapet and efficient cover.

This well is shown in plan and section in the diagram, which I will throw upon the screen (Figs. 2 and 3).

Now no water can possibly enter the well, except through the bottom. The water in it is clear and bright, and since its construction no mud has collected on the bottom. The sides of the pipes also remain absolutely clean, so much so that when, last summer, I

showed this well to a party of scientific friends, some of them dropped a hint that it had possibly been scrubbed in honour of their visit. This, however, was not the case.

The water from this well has been examined three times chemically, with the result that it has been pronounced free from organic impurities, and three bacteriological examinations have been made with the result of showing a bacterial purity, which is quite exceptional. The last examination was made by Dr. Cartwright Wood in November, 1895, and showed a very high degree of bacterial purity. The water was specially examined by Dr. Wood for the presence of *Bacterium Coli commune*, but with negative results. Dr. Wood writes: "The results are exceedingly satisfactory, and I must admit surprised me very much." A surface-well on this pattern has lately been constructed in a neighbouring village, and the results, as far as the appearance of the well and water are concerned, seem to be entirely satisfactory.

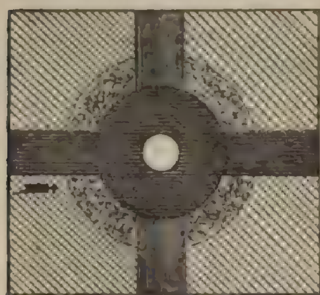


Fig. 2.
Plan of well, showing its relation to paths and hedge.

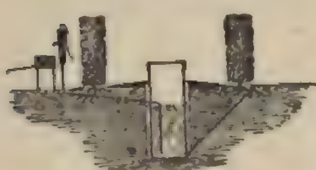


Fig. 3.
Section of well, showing concrete lining and position of pump.

When people live crowded together in cities, the difficulties connected with the cleaning of the houses is very great. After the invention of the steam-engine it was found possible to supply even the top floors of the highest houses with an ample supply of water. We accordingly abolished the scavenger, and adopted a complete system of water-carried sewage. In this way our houses have been cleansed, and our rivers and surface-wells have been fouled, and it is difficult to say whether at present there be a balance of advantage or disadvantage. We have had epidemics of cholera and of typhoid, and it is almost certain that there is no one here present but has suffered in some way or another from the "drains."

The greatest drawback of this system is the fact that it encourages overcrowding of houses on inadequate areas, and, unfortunately, it is this fact which has rendered the system so popular. With water under pressure there is no need to provide houses with any back-door or back-yard, and there is no inconvenience in having excessively high buildings. The speculative builder, who has been relieved of all responsibilities in connection with sewage and water

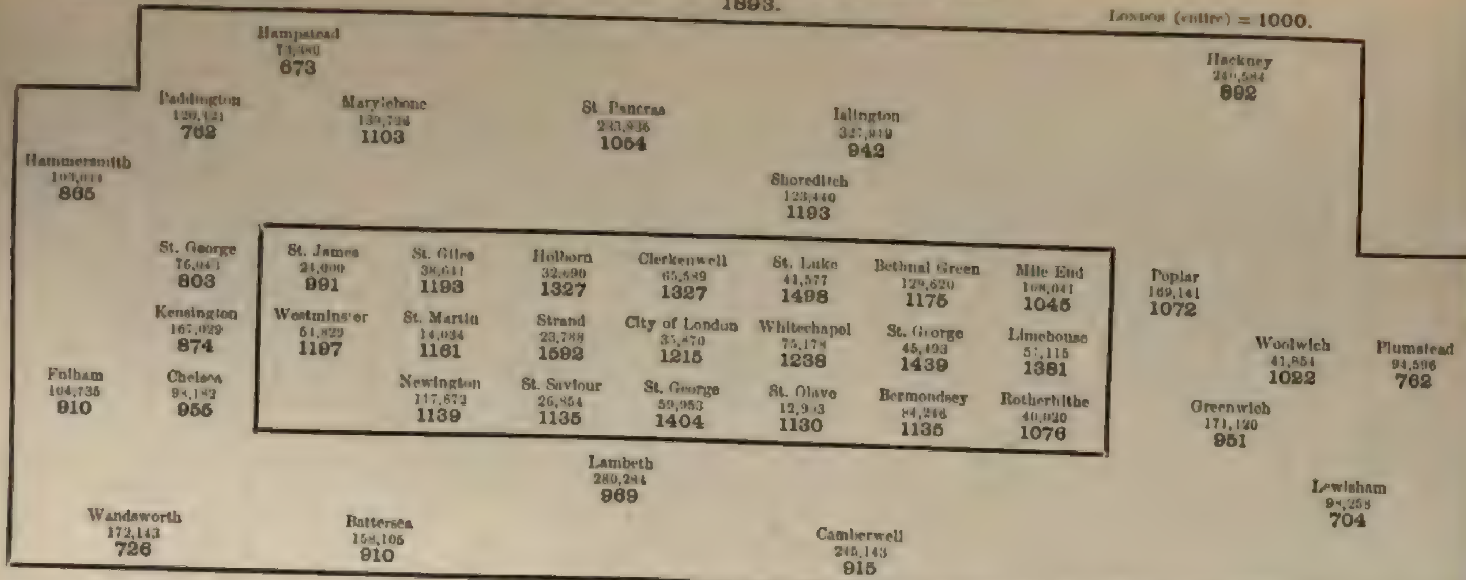
MORTALITY FIGURES.

(Figures in small type show the population of the Sanitary Areas.)

England and Wales = 880.

1893.

London (entire) = 1000.



DR = Corrected Death Rate.

1892.

MF = Mortality Figures.

	DR	MF
Hampstead	14·2	657
Lewisham .. .. .. } Plumstead .. .. .. }	15·7	727
Wandsworth	16·8	778
Hackney	18·1	838
England & Wales	**19·0**	**880**
Paddington .. .. .. } St. George's, Hanover Square .. .. .. }	19·3	894
Battersea .. .. .. } Kensington .. .. .. }	19·4	898
Greenwich	19·7	912
Camberwell	19·9	921
Islington	20·1	931
St. James's, W.	20·2	935
Lambeth	20·7	958
Hammersmith	20·8	963
Fulham	20·9	968
London (entire) ..	**21·6**	**1000**
Chelsea	22·0	1019
Rotherhithe	22·3	1024
Woolwich	22·8	1056
Poplar	23·2	1074
St. Marylebone.. ..	23·4	1083
St. Pancras	23·5	1088
Mile End	23·8	1102
Shoreditch	23·9	1106
Bethnal Green	24·1	1115
Bermondsey	24·3	1125
City of London	25·3	1171
Newington	25·5	1181
St. Giles	26·2	1213
Westminster	26·6	1231
St. Saviour, Southwark .. .. .. }	26·7	1236
Whitechapel	26·8	1241
Clerkenwell .. .. .. } St. George's, Southwark }	27·5	1273
Limehouse	27·8	1287
St. Martin's in the Fields	27·9	1292
St. Olave's	28·1	1301
St. Luke's	28·2	1306
St. George's East ..	28·8	1333
Holborn	29·7	1376
Strand	33·4	1546

supply, has abundantly used his opportunities, and the happy groundlandlord has sold his land at large prices per square foot. We are shutting out the light and air more and more from our cities, and the crowding in the streets is making locomotion in them difficult. This overcrowding is a serious matter, and I will show you what it means in London by throwing on the screen a table and diagrammatic plan of the sanitary areas of London, with the mortality figures in the years 1892 and 1893, as calculated by Mr. Shirley Murphy after due correction for abnormalities of age and sex distribution (see the preceding page).

This table and plan shows at a glance that the mortality of London as a whole (taken as 1000) is fourteen or fifteen per cent. higher than that of England and Wales, and that, while some of the outlying districts, such as Hampstead, Lewisham, and Plumstead, have a mortality below that of England and Wales, the areas near the centre of London are all considerably above it; and some, such as the Strand, Holborn, St. George's-in-the-East, and Whitechapel, have a mortality as high as that of the worst manufacturing towns.

The danger of overcrowding is well shown by the explosive outburst of small-pox in Marylebone in 1894.

I will throw upon the screen a photograph of part of the Asylums Board Map in which each case of notified small-pox is shown by a black dot (Fig. 4). This map shows that the outbreak was limited to two spots, one in Portland Town and one round Nightingale Street, Edgware Road, where the density of population, according to Mr. Charles Booth, is over 300 persons to the acre.

. The other maps show that, whereas the air-borne contagium, diphtheria, was confined more or less to the crowded districts, enteric fever, which is a water-borne contagium, was evenly spread over the whole parish. It need hardly be said that the enforcement of vaccination, notification, and isolation, are important in proportion to the density of population. The working of the sanitary laws is a great expense to the ratepayers. I find it stated, for instance, in the report of the Asylums Board, that for the removal of the 260 small-pox patients from Marylebone, the ambulances travelled nearly twenty miles for each patient, and collectively 5200 miles, or about the distance from here to Bombay. Overcrowding is not cheap, and I find, by a reference to the report of St. Marylebone, that whereas, in 1871, that parish, of about 1500 acres, and with a diminishing population, could be "run" for about 660*l.* a day, it now costs about 1100*l.* per day. It is right to add that the parish has no control over a great part of the expenditure, but, nevertheless, 440*l.* per diem is a fair sum to place upon the shrine of progressive municipalism.

If infectious disease occurs in our houses we have only to notify, and the parish does the rest. We have put a premium on fever, and the lucky man whose house is visited by a mild scarlatina is rewarded by having his family maintained for six weeks at the public expense

and has whitewashing done by the parish. If, on the return of a child from the hospital, another child catches the disease, he can recover damages.

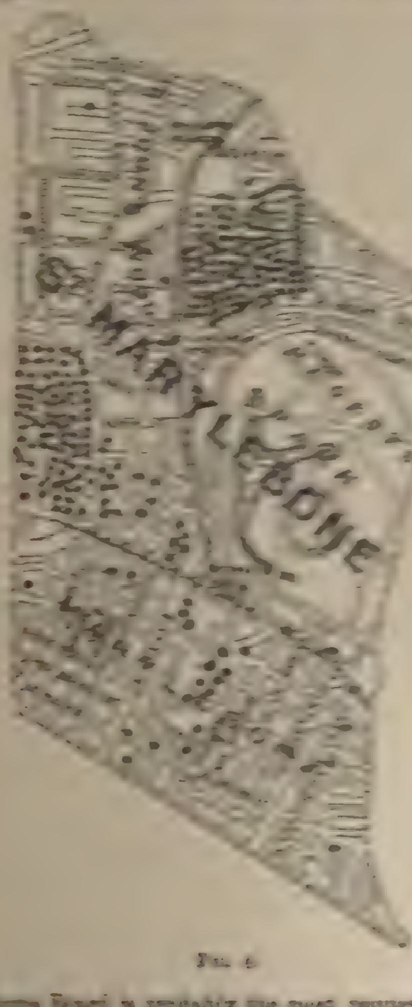


FIG. [illegible]

The Asylums Board is probably the most pauperising institution ever constructed, but we are such cowards in the presence of disease that financial and moral considerations have but little weight, provided the nuisance be removed.

Another great drawback to the water-carriage system of sewage is the increasing difficulty with regard to water supply. Our needs per

head per diem in the matter of water have gradually increased to something like forty gallons, which many experts consider to be none too much. In London the air is so foul that rain-water is valueless for domestic use, and the water of the surface wells is too poisonous to drink, because we have neglected what I believe to be the most important of the principles of sanitation, viz. the keeping of organic refuse, whether solid or liquid, on the surface. The humus is the most perfect purifier and the best of filters, in virtue of its physical conditions and the life that is in it. We deliberately take our filth to the under side of the filter, and then complain because our surface wells are foul. The Water Companies are masters of the situation. Water is not paid for, as a rule, in proportion to the quantity used, because Parliament in its wisdom has decided that thriftiness in the use of water is wicked. The grossly overburdened ratepayer is now pricking up his ears to listen to the prattle about Welsh water schemes at a cost of 38,000,000*l.*, and is congratulating himself that he is only a leaseholder, and that his bondage is terminable in seven, fourteen or twenty-one years at most. Water carriage, in which the carrier is some sixty times more heavy and twenty times more bulky than the thing to be carried, is economically ridiculous (except in places where nature has provided enormous quantities of water), and involves every place where it is tried in ruinous debt. Let us take an illustration.

A suburban district having 27,000 persons on 7000 acres of land, or a population of less than four to the acre, mainly engaged in market gardening, has in the last ten years borrowed 106,442*l.* for sewerage works. The only visible result to the inhabitants is that even country roads, with houses at ¼-mile or ½-mile intervals, have been dotted with foul smelling manholes.

In 1894–5 the sum of 18,534*l.* 14*s.* 1*d.* was raised from rates, and of this there was spent 6518*l.* 13*s.* 10*d.* for interest and repayment of sewerage loans, and 2542*l.* 3*s.* 11*d.* for current expenses in connection with sewage. If to this be added one-third of the establishment charges (say 700*l.*), we reach a total of 9860*l.*, or more than half the sum received from rates.

The provision and maintenance of all the patent domestic gimcracks which water carriage involves, together with the necessarily increased bills for water paid by the householder, would probably double that sum, and we shall not be far wrong in saying that these 27,000 persons are spending 20,000*l.* a year for the purpose of throwing their capital into the Thames.

This doubling of rates has most seriously crippled the chief industry of the district, and the market gardeners feel severely the heavy extra charges which they are called upon to pay. These gentlemen by putting much of the offal of great towns to its proper use, and converting it into food and wages for the poor, are doing a great work, but they are in a fair way to be ruined by the silly recklessness of our local governors.

On December 3, 1895, a writer in *The Times* pointed out that in 1895, as compared with 1890, 633,000 acres of land were either out of cultivation or had been converted to "permanent pasture," a term which implies a minimum cultivation. Of these lands there were in Essex over 31,000 acres, in Kent nearly 30,000, in Surrey 15,000, in Sussex 29,000, in Berks 20,000, in Bucks 11,500, Herts 7600, Middlesex 5500.

It is a noteworthy fact that in the eight counties nearest London which provides for them an insatiable market, nearly 150,000 acres of land should have glided out of cultivation in the last five years. It is impossible not to believe that the local rates in places near London are the last straw upon the back of the agriculturist, who is ruinously taxed in order that his land may be starved. To show what suburban agriculturists have to bear in the way of local taxation I will quote from my little book, 'Essays on Rural Hygiene,'* a few figures showing what is paid by a gentleman who farms 200 acres of land, of which 15 are grass:—

	£	s.	d.
Income Tax (at 6*d.*)	47	4	9
Land Tax	24	16	8½
Poor Rate	123	0	5
Burial Rate	19	13	8
District Rate	83	1	11
Tithe (considered low)	15	11	4
	£313	8	9½

The social problems of the present day are many and complicated, and all of us have heard of "Distressed Agriculture," "Pauperism," "The Aged Poor," and the "Unemployed."

The agriculturist, who is being burdensomely taxed in order that his land may be starved, is apparently to have his rates paid for him out of the Imperial Exchequer. No one who knows the straits he is in will grudge him this relief. But the paying of local charges out of Imperial taxes has the inevitable result of making our "Local Boards" more and more extravagant, because they have the spending without the trouble of raising money.

The reform most needed in the interest of the agriculturists and others is to put an effectual check upon the extravagance and ostentation of Local Boards and District Councils, and to see that they spend no more money in any one year than they can raise in their districts. These bodies are now obliged to submit their accounts to a proper audit and to publish them, and it is hoped that the ratepayer will subject them to close criticism.

The policy of allowing persons who are elected for three years to raise loans and plunge a district into debt for a period of thirty years

* 'Essays on Rural Hygiene,' 2nd ed. 1894, Longmans.

without one iota of personal responsibility is obviously dangerous. To allow reckless borrowing for the construction of works which are a source of expense and waste and never of profit, would be called madness in private life.

Doubtless a seat on a Council which borrows money in lots of 100,000*l.* at a time affords a delightful amusement to the idle man, the busy-body, the faddist, the philanthropist with a mission for fumbling in other persons pockets, and the prophet who is ever anxious to borrow in order to provide for the future of which he is ignorant. Your prophet is the most dangerous of these persons, and instances will occur to the minds of most of us of municipalities which have been half ruined by over sanguine persons endowed with speculative minds and persuasive tongues. The risks run by these persons is so small, be it remembered, that if an aggrieved ratepayer makes them defendants in an action they enjoy the unique privilege of paying part of their costs and damages out of the successful plantiff's pockets.

Most of the local borrowing in this country has been for works of sewerage, and although such works are financially ruinous we are told that we get a dividend of "Health." This, however, is not true, at least in London, and nobody could expect health to emerge from a system of which putrefaction and overcrowding are the chief characteristics.

The application of organic matter to well-tilled soil leads to positive gain and definite increase. The soil is the only permanent source of wealth in this world. And we are all of us absolutely dependent upon it for existence and happiness. The soil, if properly tilled, provides health as well as wealth, and be it remembered that in proportion to its productiveness so is the need of labour; and further, be it remembered that long after the eye is too dim and the hand too slow to keep time with steam machinery, the physical powers are amply sufficient for the cultivation of the land.

Many of our pressing social problems are inextricably linked with our duty to the soil, and any country in which the fertility of the soil does not increase cannot be rightly regarded as really in the van of civilisation and scientific progress. We are probably the wealthiest country on the globe, because for some time past we have been the hub of the entire financial world. Our success in one direction is no excuse for neglecting the more certain sources of wealth, and it is to be hoped that it will soon be regarded as evidence of neglect of our moral obligations to allow the land to drift out of cultivation.

[G. V. P.]

ANNUAL MEETING,

Friday, May 1, 1896.

Sir James Crichton-Browne, M.D. LL.D. F.R.S. Treasurer and Vice-President, in the Chair.

The Annual Report of the Committee of Visitors for the year 1895, testifying to the continued prosperity and efficient management of the Institution, was read and adopted.

Seventy-two new Members were elected in 1895.

Sixty-three Lectures and Nineteen Evening Discourses were delivered in 1895.

The Books and Pamphlets presented in 1895 amounted to about 260 volumes, making, with 594 volumes (including Periodicals bound) purchased by the Managers, a total of 854 volumes added to the Library in the year.

Thanks were voted to the President, Treasurer, and the Honorary Secretary, to the Committees of Managers and Visitors, and to the Professors, for their valuable services to the Institution during the past year.

The following Gentlemen were unanimously elected as Officers for the ensuing year:

President—The Duke of Northumberland, K.G. D.C.L. LL.D.
Treasurer—Sir James Crichton-Browne, M.D. LL.D. F.R.S.
Secretary—Sir Frederick Bramwell, Bart. D.C.L. F.R.S. M. Inst. C.E.

Managers.

Sir Frederick Abel, Bart. K.C.B. D.C.L. LL.D. F.R.S.
Sir Benjamin Baker, K.C.M.G. LL.D. F.R.S.
John Wolfe Barry, Esq. C.B. F.R.S. M. Inst. C.E.
The Right Hon. Lord Halsbury, M.A. D.C.L. F.R.S.
Charles Hawksley, Esq. M. Inst. C.E.
John Hopkinson, Esq. M.A. D.Sc. F.R.S.
Victor Horsley, Esq. M.B. F.R.S. F.R.C.S.
William Huggins, Esq. D.C.L. LL.D. F.R.S.
The Right Hon. Lord Kelvin, D.C.L. LL.D. F.R.S.
Alfred B. Kempe, Esq. M.A. F.R.S.
George Matthey, Esq. F.R.S.
Ludwig Mond, Esq. Ph.D. F.R.S.
Sir Andrew Noble, K.C.B. F.R.S. M. Inst. C.E.
The Right Hon. Earl Percy, F.S.A.
Basil Woodd Smith, Esq. F.R.A.S. F.S.A.

Visitors.

Gerrard Ansdell, Esq. F.C.S.
Sir James Blyth, Bart.
Arthur Carpmael, Esq.
Sir William James Farrer, M.A. F.S.A.
Carl Haag, Esq. R.W.S.
Sir Francis Laking, M.D.
Hugh Leonard, Esq. M. Inst. C.E.
James Mansergh, Esq. M. Inst. C.E.
Lachlan Mackintosh Rate, Esq. M.A.
Felix Semon, M.D. F.R.C.P.
Henry Virtue Tebbs, Esq.
John Isaac Thornycroft, Esq. F.R.S. M. Inst. C.E.
Thomas Tyrer, Esq. F.C.S. F.I.C.
John Westlake, Esq. Q.C. LL.D.
James Wimshurst, Esq.

WEEKLY EVENING MEETING,

Friday, May 1, 1896.

William Crookes, Esq. F.R.S. Vice-President, in the Chair.

Colonel H. Watkin, C.B. R.A. *M.R.I.*

Chronographs and their Application to Gun Ballistics.

The lecture I have had the honour of being asked by the Council of this Institution to give to-night, is on a subject in which I have taken great interest and worked at for the last twenty-five years. There is a fascination in being able to record minute portions of time which our senses are not able to discriminate. It is easy to talk about the millionth of a second, but it is hard to realise how small this is. To try and convey some idea of this, supposing a man were to work eight hours every day, Sundays excepted, for close upon seven years, one-millionth of his working time during that period would be represented by one minute. The instrument which I hope to show you at work this evening records to that accuracy when working at the highest speed. The objects I had in view in designing the apparatus are twofold. First, the measurement of the velocity of a projectile outside the gun, or *external* ballistics. Secondly, the measurement of the velocity of a projectile at different parts of the bore of a gun, or *internal* ballistics. The first is useful for comparing the relative power of different guns, merits of different powders, and for determining the resistance of the air. The second for ascertaining the pressure exerted at different parts of the bore by different natures of powder, from which the shape of the gun is determined. I dare say you have all noticed the very different shapes of modern guns from those of a few years ago. This difference is due to the very different behaviour of the powder, or rather propellant, now employed, as one can hardly talk of cordite as powder.

I propose this evening to very briefly describe some of the older forms of chronographs, and more minutely describe those on the table, which I have designed for experiments in ascertaining the velocity of a shot passing through the bore of a gun.

The subject divides itself into two principal parts:—

1. The apparatus for measuring minute portions of time.
2. The appliances for utilising these instruments for ballistic purposes.

The first I will further subdivide into two parts :—

(*a*) Instruments depending upon the action of gravity.
(*b*) Instruments having revolving drums

The latter into—

(*c*) Appliances for ascertaining external ballistics.
(*d*) Appliances for ascertaining internal ballistics.

The lecturer here described, with the aid of lantern slides, several instruments which had been used for ballistic work, such as Navez-Leur, Boulenge, &c.

About the same time as the Boulenge was introduced, I designed the instrument shown in Fig. 1. In this a weight drops freely in air, and the registration does not, as in the Boulenge chronograph, commence from the moment of its liberation, but during its fall, thus avoiding any inaccuracy of residual magnetism in the electro-magnets, from the fact that registration takes place during the fall. When small portions of time have to be measured, the experiments may be so arranged that the weight under the accelerating force of gravity shall have acquired a considerable velocity before registration commences. Also the time of passing several screens can be recorded.

The instrument consists essentially of two upright brass cylinders revolving on pivots, those at the bottom being fixed, while the two at the top consist of screws to allow of the cylinders being removed. The cylinders are carefully insulated from one another, and connected with two binding screws on the base board. On the bed of the instrument are two levels at right angles to one another, by which, with the aid of three levelling screws, the cylinders may be placed truly vertical. Close to but not quite touching the cylinders are scales divided into thousandths of a second, which by means of a peculiar vernier subdivide these into hundred-thousandths of a second. On the top is an electro-magnet which serves to hold up the weight equidistant between the two cylinders.

The weight has two sharp points which nearly, but not quite, touch the surface of the cylinders.

The action of the instrument is simply this. The weight being released a short time before the gun is fired, descends between the cylinders; the shot on passing through the first screen breaks the continuity of the primary wire of an induction coil, thus causing an induced spark to pass from one cylinder to the other through the brass wire of the weight. As the cylinders are smoked, a minute spot registers the exact position of the weight at that moment. The weight continuing to fall, as the shot passes the second screen (the primary current in the meantime having been re-established) the same result follows; and so on for any number of screens. The distances between the spots, as read off from the velocity scale, give the time of the shot passing the various screens.

By means of a calculating scale the velocity may be determined for any distance between the screens. For a second experiment the

drums are slightly revolved so as to present a fresh smoked surface for the records, and the weight again suspended, and so on.

The instrument can be used for accurately determining the speed

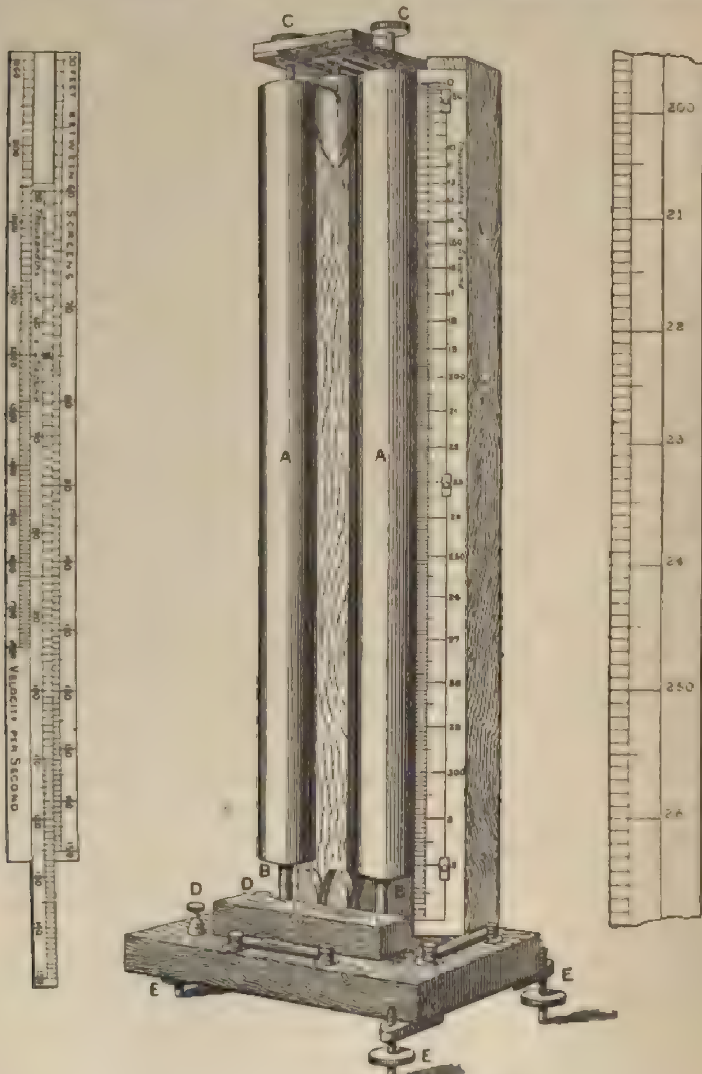


Fig. 1.

of revolving cylinders; also to demonstrate the accelerating force of gravity. Thus, having attached the secondary wires of a coil to the binding screw, and set the vibrating spring in action, a stream of sparks passes through the suspended weight, the rapidity, which is due to the note given out by the vibrating spring, being so great that to the eye it appears as one continuous stream of light. But if the weight be now dropped the sparks appear down each cylinder, opening out as the weight descends. Each of these sparks gives its record on the cylinders, and if they are read off by means of the velocity scale, you will see that they are equi-distant as regards time but vary as to linear distance. They follow the well-known law,

$$S = \frac{1}{2} g t^2.$$

An interesting experiment is simply made to test one's personal equation, and to show the comparatively long time it takes for a message to be sent from the brain to the fingers. Thus, if I press this key, which breaks the primary circuit, the moment I see the weight begin to fall, the induced spark will record the time it has taken to perform this operation.

We now pass on to instruments having revolving drums, the circumferential speed of which can be made much greater than the dropping weight, or plumb-bob, of the instrument I have described. Prof. Bashforth's is a notable example, and one which did much good work in experiments for ascertaining the resistance of the air to projectiles.*

After many years' work, designing and constructing chronographs for experimental purposes, I devised the instrument shown in Fig. 2, and the system of plugs, &c., with which I have been taking the travel of shot up different guns during the last two or three years. In this a large drum, made as light as possible consistent with strength, is carefully mounted between coned centres. And here I may mention an incident for the benefit of others, which might have had serious consequences to myself. In the smaller and lighter instruments I had previously employed, I had hard steel bearings working into hard steel centres, and found no difficulty with them, and I therefore employed the same in this instrument. But one day, notwithstanding careful lubrication, the two metals seized, and the drum, which was revolving at a high speed, was quickly brought to a standstill and pulled out of its bearings. I of course turned off the current at the first alarm, but it was fortunate for me that the support held the drum. I now employ No. 7 phosphor bronze, and all works smoothly; at the same time I do not neglect lubrication. This drum is revolved by means of a motor, and this I consider a great advantage over any other method, inasmuch as the drum can be driven at a very high speed, and

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* Description was here given of Professor Bashforth's chronograph and the Noble chronoscope.

kept for some time running uniformly. With geared mechanism this is impossible, even though the greatest care is taken, as in the case of

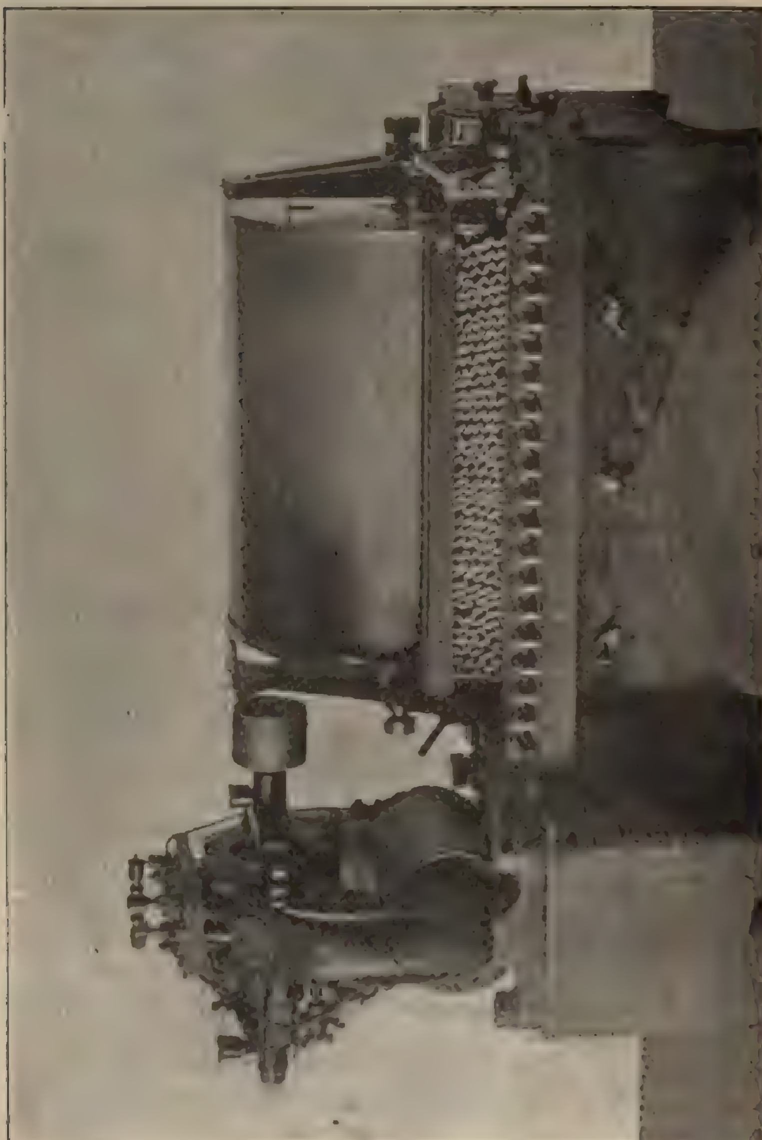

FIG. 2.

the Noble chronograph, to grind the roughness out of the mechanism by running it for some time.

On a hinged frame of ebonite are placed a row of forty steel-pointed pins, screwed into the ebonite so as to allow of accurate adjustment. The frame is brought up to a fixed stop, and clamped by means of two cam clutches. Each pin is carefully adjusted, to be at a uniformly small distance of about $\frac{1}{200}$ inch from the surface of the drum. The ebonite frame is capable of traversing from right to left, so that each point is opposite a different surface of the drum, for the convenience of making a series of experiments without re-smoking the drum. Each pin is connected by insulated wires with a binding screw on the bed-plate. On the left edge of the drum is a carefully divided circle, reading by means of a vernier to minutes of angle, and with care to half this accuracy.

Wires run from the secondary poles of a series of induction coils to these binding screws. Thus 1 and 2 binding screws are connected with No. 1 coil, 3 and 4 to No. 2 coil, &c. In this way I have two records on the drum for each primary circuit. The primary circuits of the coils are connected with plugs (which I shall presently describe) screwed into the gun.

Now we come to a very important part of the instrument, viz. the means of timing the speed of the revolution of the drum. In my first experiments, years ago, I employed the usual method so much in vogue then and now, viz. tuning forks. A tuning fork, as you know, vibrates so many times a second according to its note. Thus, for instance, the middle C corresponds to 256 double vibrations in a second. To employ these a small stylus is fixed to the tuning fork, which presses lightly on the drum; as the drum revolves a sinuous line is formed by scratching off the smoked surface. I found, however, by careful trials that you could not depend on these records, owing to different atmospheric conditions and the varying surfaces of the drum. Nor does this seem unreasonable when we look into the matter. In the first place the vibrations of a fork are affected by temperature and barometric pressure; these are more or less known and could be allowed for. We might also correct for the additional weight of the stylus, but it seems to me more difficult, nay impossible, to say what the vibrations are under the friction of the stylus on the surface of the drum with varying thickness of carbon deposit. Moreover the trouble of working out of the tuning fork records is considerable; and with the circumferential speed necessary for recording millionths of a second, forks with a very high note have to be employed A fork giving the middle C, before mentioned, would be useless for this purpose, but the higher the rate of vibration the greater would be the retarding effect of the stylus recording the vibrations.

The stop watch arrangement employed by Sir Andrew Noble is not applicable to this instrument, nor is it, I think, a very accurate method of timing.

I have, after many failures, worked out a method which experience

shows is very reliable. In this I depend on a very constant quantity, viz. gravity. A weight is dropped from a given height, and in its fall breaks two screens one after the other. Knowing the distance the weight has to fall to the first screen, and the distance between the screens, it is easy to calculate the time it has taken for the weight to pass from the one screen to the other.

The screen is made thus—see Fig. 3. A piece of hardened watch spring A B, is pivoted in a brass frame B C, and capable of being held up and pressed very lightly against the support D, so that two pieces of platinum, one on the spring and one on the support, are kept in contact. The fall of the weight E breaks the contact. An exactly similar arrangement, A′ B′ C′ D′ is placed about 3·77 inches below the top spring. Each screen is connected to the primary wire of an induction coil, the secondary being led to the recording points opposite the drum of the chronograph. It follows, then, that the moment the weight touches the first screen, a spark passes on the drum from the steel points. The drum goes on revolving, and the weight continues to fall until the second screen is reached, when again a spark passes. The distance between the two spots measured on the graduated circle, and the known time taken by the weight to pass the screens, gives the speed of the drum. The time taken for the weight to fall below the screens was ·018948 second. As the result of a trial before a committee, in which the record of two weights was made on a rapidly revolving cylinder, the variation did not exceed 0·16 per cent. To test whether the weights were appreciably retarded by breaking the screens, a third screen was inserted between the weight and the first screen, and it was found that there was no appreciable retardation. As a precaution I always employ two drop weights with entirely independent circuits, so as to avoid the chance of an experiment being wasted, from the possible failure of one of the screens not acting through a bad contact; but I nearly always obtain the double record.

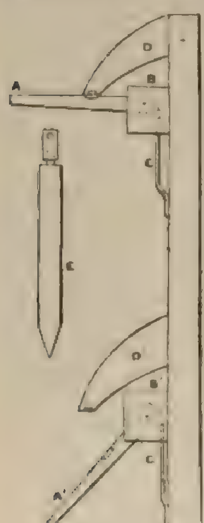

Fig. 3.

The next difficulty I encountered in my experiments was the means of reading the record of the sparks. Some days we might get nice small records by carefully adjusting the strength of the current. Another day the records would be much too large for any accuracy. I tried every conceivable method of smoking, from the carbon deposited by gas flame, to that deposited by various kinds of oils, and also that of burning camphor, but could not be certain of my records being readable. I may here mention that for accurate experiments,

covering the drum with paper is out of the question; for the spark, taking the line of least resistance, goes through the thinnest part of the paper, which may or may not be directly opposite the points at the moment the spark occurs. When extreme accuracy is not required, paper may conveniently be employed, as the paper, on being removed and varnished on the back, may be kept as a record of the experiment for future reference and measurement.

The difficulty of obtaining a uniformly smoked surface giving a minute centre for exact reading has been overcome by the following simple means. A small lump of paraffin wax about the size of the tip of one's little finger is dissolved in half a pint of benzole; a rag saturated with this solution is rubbed over the drum. The drum is smoked with a large flat wick saturated with a moisture of equal parts of paraffin oil and rape seed oil. The nature of the records obtained can be varied at will, according to the amount of wax dissolved in the benzole, but all have a distinctly defined minute centre, which can be read to the greatest nicety. The method adopted of reading the records, is to stretch a fine hair in the centre of a brass frame, fitting with steady pins the supports of the drum centre. The hair is so arranged as only just to clear the surface of the drum. A magnifying glass enables one to bring the record marks exactly under the stretched hair.

We now come to the application of these instruments for measuring gun ballistics. For external ballistics the usual screen is a series of copper wires stretched across a wood frame. The cutting of this wire breaks the circuit and gives the record. There is no doubt that the cutting of a wire in this manner is not perfectly accurate, and to a slight extent would vary with the size of the screen; but for ordinary work of getting the muzzle velocity of a shot, when the screens are placed 120 feet or more apart, it is sufficiently good. Bashforth employed a different form of screen, as he required the circuit to be remade immediately the shot had passed through. In this a spring, whose play was limited by a hole in a copper plate, was held down to the lower surface of the hole by a weighted thread. As the thread was cut the spring, rising to the top of the hole, broke the circuit and remade it. In this method, as I have experimentally proved, the breaking of the circuit is not very exact, but near enough when the screens are far apart. I employed a somewhat similar arrangement with my drop-weight chronograph, only using broad flat springs to avoid the rubbing of the spring against the side of the hole, which sometimes occurred in the Bashforth screens.

For internal ballistics when we have to measure the passage of a shot over plugs placed two inches apart, the utmost accuracy of break is required. Sir A. Noble used a cutter plug which severed a wire as the shot forced up an inclined plane. Unless the shot exactly fits the bore, which of course with the ordinary projectile it does not, considerable errors arise from the use of such cutter plugs, as we never know the exact position of the shot when the wire is actually

severed. After trying several methods, the following, which has proved most satisfactory, was worked out. A soft steel wire, Fig. 4, A B C, bent as shown in this diagram, has the bent portion B hardened at two points, where it projects from the plug into the gun. The wire is covered with india-rubber tubing to insulate it from the plug, and a plug of asbestos packing D, pressed hard by a screw piston E, prevents any escape of gas. After the first experiment we found the compressed air in front of the projectile pressed the wire away from the breech and altered its position very slightly; so now boxwood ferrules are placed over the wire instead of the rubber tube, for a short distance from the bottom of the plug. The holes in the gun are bored spirally round the gun, so as not to weaken it in one line. The gun we have been experimenting with is really a 7-inch gun, with a bore of 4·7 inch diameter, and 60 calibres long. The great length gives us the opportunity of ascertaining what gain in muzzle velocity is obtained by additional length. Some of the plugs at the breech end where the rise of pressure is very rapid, are only 2 inches apart, the distance increasing towards the muzzle where they are 20 inches apart. Here the pressure is comparatively small, but the velocity of the shot is very great.

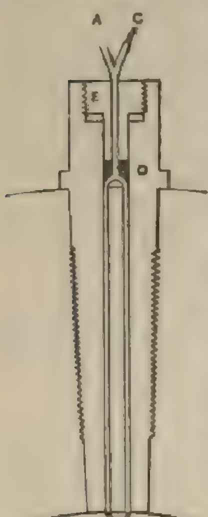

FIG. 4.

The sketch, Fig. 5, shows the arrangement of wires from the different parts of the apparatus. Only a few wires are shown to avoid confusion.

Here the lecturer showed the working of the whole apparatus, firing a pistol to break a series of screens representing the bore of a gun. Records were obtained on the drum of the breaking of the screens by the bullet, and the speed of the drum was determined by a drop weight, similar to that shown at Fig. 3.

The readings obtained on the divided circle are translated into time, and plotted on a very large scale in the Royal Gun Factory, and the velocity and pressure curves calculated. Here is a specimen of the curve. The working out of a round is a laborious affair, taking about a fortnight.

Now it may be rightly asked—How do we know that the records on the drum are true? Are the cutter plugs reliable, and the records given by the induction coil accurate? To test the question of the cutter plugs, two plugs were placed, one on the top side of a gun, and one on the bottom side, but at exactly the same distances from the muzzle. The circuits for these were entirely distinct. On firing the gun identical records were obtained. Now as regards the records of the sparks, whether they vary, and how long after the rupture of the

primary does the secondary occur, might I suppose be tested by means of the revolving mirror—but this would not have been entirely satisfactory, inasmuch as it would not have tested the actual record on the drum. So I devised the following, which, though apparently very simple, requires care to get good results. On the rim of the drum I insert a piece of ivory. Fitted to the bed-plate is a hinged piece of brass whose far end presses against the rim of the drum. The circuit from the primary wire of an induction coil runs through the brass piece and the drum, except when it is interrupted by the ivory. A sharp break here occurs, which leaves its record on the drum by means of the steel pins and secondary current, as before described. If the drum is revolved slowly, the spark will give the true position at which the ivory breaks the circuit. If, now, there is any retardation or delay in the record of the spark, it will be shown on the drum when it is rotated rapidly—the record lagging behind that obtained by the slow break. Knowing the speed of the drum, the time of retardation can be obtained.

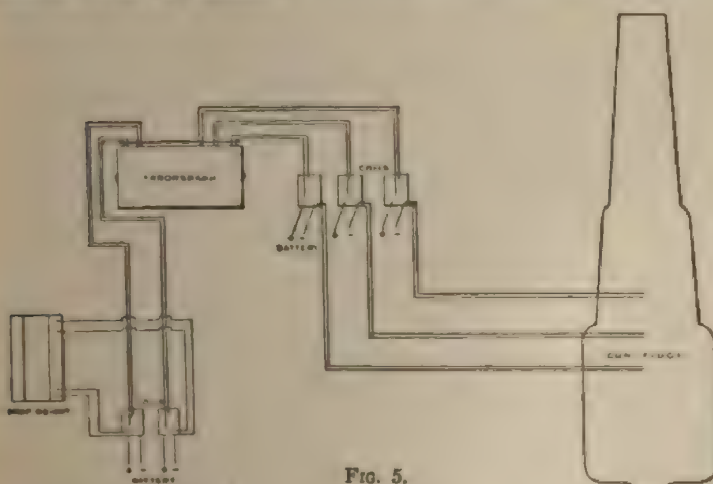

Fig. 5.

I have made several of these experiments. On the table are some of the records. To test the variability of the records, it suffices to move the recording points along the drum at each break, when the records should be in a straight line. These specimens will show you how accurate they are. Great care must be exercised to turn the rim perfectly true and smooth; also the brass piece rubbing against it must be often smoothed up.

The measurement of these retardations is a delicate matter, as we are dealing with a retardation of only 10 millionths of a second. I think that the improvements I have carried out in these instru-

ments now enable us to obtain records of the passage of a shot up the bore of a gun to an accuracy closely approaching the millionth of a second.

There is one thing, however, we have failed so far to get, and that is the velocity of a shot immediately outside the muzzle of the gun. There is no doubt that for a short space of time the shot is accelerated, but how far the acceleration extends is not known.

To try and obtain this we had a strong steel bar fastened to the muzzle and projecting some 10 feet from it. In this were screwed the same kind of plugs as those I have already described, only that the steel wire was of much stouter gauge. The experiment was, however, a failure. The two plugs that were cut just before the tail end of the shot left the bore were properly recorded, but the moment the shot cleared the bore, the blast rushing past the shot caused irregular results.

Three years ago I proposed another method, which is just about to be tried, viz. that the drum of the chronograph be covered with a sensitive photographic film, the whole apparatus to be enclosed in a box and fitted with a lens. In the gun is a shot filled with magnesium composition; this is ignited electrically just before the gun is fired. As the drum with the film will be in rapid revolution, I hope to get a streak of light impressed on the film by the magnesium shot as it leaves the gun. This will form a curve which, from the known speed of the drum, will give the exact speed of the shot at every moment from leaving the muzzle to a distance of 20 or 30 feet in front of the gun. From a small experiment I made in my workshop this seems hopeful,* as I obtained a streak of light across a photograpic plate, from a magnesium torch fired from a pistol.

A useful adaptation of the revolving drum is to ascertain the velocity of recoil of rifles and guns, &c. Across the drum is a slide, which runs along a groove and presses lightly on its smoked surface. As the slide is pulled by the recoil, the drum at the same time revolving at a known speed, we get a curve which gives the velocity of the recoil at every moment.

I tried in this way to get the curve of the first start of a shot in the 12 pr., a steel wire being fastened to the shot, and led through the breech block to the chronograph placed on the carriage immediately behind. The result was a failure, as the wire broke almost immediately. This possibly might have been got over by thicker wire had the experiment been carried on.

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* Since the lecture some of the experiments have taken place, and show that most distinct records can be obtained in this way. The twist of the shot is also shown, as there were two exits in the shell.

[H. W.]

GENERAL MONTHLY MEETING,

Monday, May 4, 1896.

Sir James Crichton-Browne, M.D. LL.D. F.R.S. Treasurer and Vice-President, in the Chair.

The following Vice-Presidents for the ensuing year were announced :—

Sir Frederick Abel, Bart., K.C.B. D.C.L. LL.D. F.R.S.
The Right Hon. Lord Kelvin, D.C.L. LL.D. F.R.S.
George Matthey, Esq. F.R.S.
Ludwig Mond, Esq. Ph.D. F.R.S.
The Right Hon. Earl Percy, F.S.A.
Basil Woodd Smith, Esq. F.R.A S. F.S.A.
Sir James Crichton-Browne, M.D. LL.D. F.R.S. *Treasurer.*
Sir Frederick Bramwell, Bart. D.C.L. LL.D. F.R.S. *Hon. Sec.*

J. H. Badcock, Esq. M.R.C.S.
F. J. Bennett, Esq.
Dugald Clerk, Esq. F.C.S.
William John Gow, M.D. M.R.C.S.
John Cameron Graham, Esq.
Mrs. Edward Patten Jackson,
Sir John Jackson, F.R.S.E.
Lady Jackson,
William L. Jordan, Esq.
J. William Mackean, Esq. F.C.S.
John S. Mackintosh, Esq.
Julius Moeller, Esq.
Thomas Oliver, M.D. F.R.C.P.
Sir Frederick Pollock, Bart. M.A. LL.D.
Harry F. Pollock, Esq. M.P.
Colonel Sir Charles Euan-Smith, K.C.B. D.C.L.
James Swinburne, Esq. M.Inst.C.E. F.C.S.
Arthur J. Walter, Esq. LL.B.
Edward Weldon, Esq.

were elected Members of the Royal Institution.

The Right Hon. Lord Rayleigh was re-elected Professor of Natural Philosophy in the Royal Institution.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Lords of the Admiralty—Greenwich Meteorological Reductions. Part 3, Temperature, 1841-90. 4to. 1895.

Accademia dei Lincei, Reale, Roma—Classe di Scienze Fisiche, Matematiche e Naturali. Atti, Serie Quinta: Rendiconti. 1° Semestre, Vol. V. Fasc. 7. 8vo. 1896.

Andom, R. (the Author)—Industrial Explorings in and around London. 8vo. 1896.

Asiatic Society of Bengal—Journal, Vol. LXIV. Part 1, No. 3; Part 2, No. 3. 8vo. 1895-96.

Asiatic Society, Royal—Journal for April, 1896. 8vo.

Astronomical Society, Royal—Monthly Notices, Vol. LVI. No. 6. 8vo. 1896.

Bankers, Institute of—Journal, Vol. XVII. Part 4. 8vo. 1896.

Birmingham Natural History and Philosophical Society—Proceedings, Vol. IX. Part 2. 8vo. 1895.

British Architects, Royal Institute of—Journal, 3rd Series, Vol. III. No. 12. 4to. 1896.

British Astronomical Association—Memoirs, Vol. IV. Part 3. 8vo. 1896.
Journal, Vol. VI. No. 6. 8vo. 1896.

Camera Club—Journal for April, 1896. 8vo.

Chemical Society—Journal for April, 1896. 8vo.
Proceedings, No. 163. 8vo. 1895-96.

Cracovie, Académie des Sciences—Bulletin, 1896, Nos. 2, 3. 8vo.

Dax, Société de Borda—20e Année (1895), 3e Trimestre. 8vo. 1895.

East India Association—Journal for April, 1896. 8vo.

Editors—American Journal of Science for April, 1896. 8vo.
Analyst for April, 1896. 8vo.
Anthony's Photographic Bulletin for April, 1896. 8vo.
Athenæum for April, 1896. 4to.
Bimetallist for April, 1896.
Brewers' Journal for April, 1896. 8vo.
Chemical News for April, 1896. 4to.
Chemist and Druggist for April, 1896. 8vo.
Electrical Engineer for April, 1896. fol.
Electrical Engineering for April, 1896. 8vo.
Electrical Review for April, 1896. 8vo.
Electric Plant for April, 1896. 4to.
Electricity for April, 1896. 8vo.
Engineer for April, 1896. fol.
Engineering for April, 1896. fol.
Engineering Review for April, 1896. 8vo.
Homœopathic Review for April, 1896. 8vo.
Horological Journal for April, 1896. 8vo.
Industries and Iron for April, 1896. fol.
Invention for April, 1896.
Ironmongery for April, 1896. 4to.
Law Journal for April, 1896. 8vo.
Lightning for April, 1896. 8vo.
London Technical Education Gazette for April, 1896. 8vo.
Machinery Market for April, 1896. 8vo.
Monist for April, 1896. 8vo.
Nature for April, 1896. 4to.
Nuovo Cimento for Feb. 1896. 8vo.
Photographic News for April, 1896. 8vo.
Science Siftings for April, 1896.
Scientific African for April, 1896. 8vo.

Scots Magazine for April, 1896. 8vo.
Technical World for April, 1896. 8vo.
Terrestrial Magnetism for April, 1896. 8vo.
Transport for April, 1896. fol.
Tropical Agriculturist for April, 1896.
Work for April, 1896. 8vo.
Zoophilist for April, 1896. 4to.

Electrical Engineers, Institution of—Journal, Vol. XXV. No. 121. 8vo. 1896.
Field Columbian Museum, Chicago—Archæological Studies from the Ancient Cities of Mexico. By W. H. Holmes. Part 1. 8vo. 1895.
Florence, Biblioteca Nazionale Centrale—Bolletino, No. 247. 8vo. 1896.
Franklin Institute—Journal for April, 1896. 8vo.
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Holmes, Basil, Esq.—Life of Sir David Baird. 2 vols. 8vo. 1832.
Sketch of the Civil Engineering of North America. By D. Stevenson. 8vo. 1838.
Holmes, Basil, Esq.—An Epitome of the Elementary Principles of Natural and Experimental Philosophy, together with an Account of the Steam Engine. (Lectures delivered at the Royal Institution.) By J. Millington. 8vo. 1823.
A Manual of Botany. By R. Bentley. 8vo. 1861.
Imperial Institute—Imperial Institute Journal for April, 1896.
Johns Hopkins University—University Studies, Fourteenth Series, No. 3. 8vo. 1896.
American Chemical Journal, Vol. XVIII. No. 4. 8vo. 1896.
American Journal of Philology, Vol. XVI. No. 4. 8vo. 1895.
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Madras Literary Society—Madras Journal of Literature and Science for 1889–94. 8vo. 1894.
Manchester Geological Society—Transactions, Vol. XXIV. Parts 5–7. 8vo. 1896.
Manchester Literary and Philosophical Society—Memoirs and Proceedings, Vol. X. No. 2. 8vo. 1896.
Mexico, Sociedad Cientifica "Antonio Alzate"—Memorias y Revista, Tomo IX. Nos. 1–6. 8vo. 1895–96.
New York Academy of Sciences—Annals, Vol. VIII. Nos. 6–12. 8vo. 1895.
North of England Institute of Mining and Mechanical Engineers—Transactions, Vol. XLV. Part 3. 8vo. 1896.
Odontological Society of Great Britain—Transactions, Vol. XXVIII. No. 5. 8vo. 1896.
Paris, Société Française de Physique—Bulletin, No. 77. 8vo. 1896.
Pharmaceutical Society of Great Britain—Journal for April, 1896. 8vo.
Philadelphia Academy of Natural Sciences—Proceedings, 1895, Part 3. 8vo. 1896.
Physical Society of London—Proceedings, Vol. XIV. Part 4. 8vo. 1896.
Queen's College, Galway—Calendar for 1895–96. 8vo.
Quekett Microscopical Club—Journal, Nos. 36–38. 1895–96.
Radcliffe Library, Oxford—Catalogue of Books added to the Radcliffe Library Oxford University Museum, during the year 1895. 8vo. 1896.
Rochechouart, Société des Amis des Sciences et Arts—Bulletin, Tome V. Nos. 3, 4. 8vo. 1895.
Rome, Ministry of Public Works—Giornale del Genio Civile, 1896, Fasc. 1. And Designi. fol.
Royal Society of London—Philosophical Transactions, Vol. CLXXXVI. B. No. 134. 4to. 1896.
Saxon Society of Sciences, Royal—
Philologisch-Historische Classe—
Abhandlungen, Band XVII. Nos. 2, 3. 8vo. 1896.
Selborne Society—Nature Notes for April, 1896. 8vo.
Sidgreaves, The Rev. W. S.J. F.R.A.S. (the Author)—Results of Meteorological, Magnetical and Solar Observations for 1896. 8vo. 1896.

Smithsonian Institution—The Composition of Expired Air and its effects upon Animal Life. By J. S. Billings, S. Weir Mitchell and D. H. Bergey. 4to. 1895. (Smith Cont. to Knowledge, 989.)
Society of Arts—Journal for April, 1896. 8vo.
Tacchini, Professor P. Hon. Mem. R.I. (the Author)—Memorie della Società degli Spettroscopisti Italiani, Vol. XXV. Disp. 3ª. 4to. 1896.
Toulouse, Société Archéologique du midi de la France—Bulletin, No. 16. 8vo. 1895.
United Service Institution, Royal—Journal, No. 218. 8vo. 1896.
United States Department of Agriculture—Monthly Weather Review for Oct. 1895. 8vo.
United States Department of Interior—Report on Crime, Pauperism and Benevolence in the United States at the Eleventh Census, 1890, Part 2. 4to. 1895.
Report on Wealth, Debt and Taxation at the Eleventh Census, 1890, Part 2. 4to 1895.
United States Patent Office—Official Gazette. Vol. LXXIV. Nos. 2–9. 8vo. 1896.
Upsal, l'Observatoire Météorologique—Bulletin Mensuel, Vol. XXVII. 8vo. 1895-96.
Verein zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, 1896, Heft 3. 4to. 1896.
Victoria Institute—Journal of the Transactions, Vol. XXVIII. No. 110. 8vo. 1896.
Vienna, Imperial Geological Institute—Verhandlungen, 1896, Nos. 1–3. 8vo.
Zoological Society of London—Proceedings, 1895, Part 4. 8vo. 1896.
Transactions, Vol. XIV. Part 1. 4to. 1896.

WEEKLY EVENING MEETING,

Friday, May 8, 1896.

Ludwig Mond, Esq. Ph.D. F.R.S. F.C.S. Manager and Vice-President, in the Chair.

Professor Silvanus P. Thompson, D.Sc. F.R.S. *M.R.I.*

Electric Shadows and Luminescence.

The early days of the year 1896 were marked by the announcement telegraphed from Vienna to the effect that Professor Roentgen, a man whose name though little known outside the world of science was well known and highly esteemed by those who were initiates in physics, had discovered the existence of rays of a new and extraordinary kind. Taking a Crookes tube, excited of course by a proper electric spark, and covering it up within a case of black cardboard, he found it to produce in the surrounding space some entirely unexpected effects. Black cardboard is impervious not only to ordinary light and to radiant heat, but also to all those other known kinds of invisible light beyond the violet end of the spectrum, known as actinic waves, which are such active agents in the production both of fluorescence and of photographic actions. Yet the invisible emanations of the Crookes tube, which passed freely through the opaque cardboard, were found by Roentgen to be capable of revealing their presence in two ways. In the first place he had seen them to project shadows upon a luminescent screen of paper coated with the highly fluorescent substance called platino-cyanide of barium, and in the second place he had been able to photograph these shadows by letting them fall upon an ordinary photographic plate. The discovery was singular. It revealed the existence of a remarkable and hitherto unexpected species of radiation. It added another to the many puzzling phenomena attendant upon the discharge of electricity in vacuo. It proved that something which in the ordinary sense in which those terms are used is neither light nor electricity was generated in the Crookes tube, and passed from it through substances opaque alike to both.

But that which took the imagination of the multitude by storm, and aroused an interest the intensity the like of which has not been known to be aroused by any other scientific discovery in our times, was not the fact that Professor Roentgen had seen luminescent shadows from a Crookes tube, or had obtained a photograph of those

shadows; it was the entirely subsidiary and comparatively unimportant point that to these mysterious radiations flesh is more transparent than bone.

Let me begin by showing you as a first experiment that same fact which Roentgen announced of the production of luminescent shadows by these invisible rays. Before you there stands a Crookes tube, of the most modern kind,* for this particular purpose. We have here an induction coil † capable of giving 6-inch sparks, with which we can send electric discharges through the tube, illuminating it with its characteristic golden-green glow. I now cover over the tube and exclude all ordinary light, not with a box of black cardboard but with a black velvet cloth. And now in the darkness I am able to show you on a sheet of paper covered with the highly fluorescent platino-cyanide of barium—the well known substance which Roentgen himself was using—the shadows of objects placed behind. See how this sheet shines in the light of the tube transmuting the invisible radiations into visible light. I hold my purse behind the screen—you see the shadow of the metal clasp, and of the metal contents (two coins and a ring), but you see not the shadow of the leather purse itself, for leather is transparent to these rays while metal is opaque. I hold my hand behind and you see—or at least those of you who are within a few yards of me—the shadow of my hand, or rather of the bones of my hand, surrounded by a fainter shadow of the almost transparent flesh.

Now the second fact that Roentgen announced was that these same rays which escape through the opaque covering and excite fluorescence are also capable of taking photographic impressions of the shadows. There is nothing whatever new about this part of the subject: it is the old photography; there is no "new photography." Here is a common camera back, and here inside it is a photographic dry-plate—quite a common dry-plate, such as has been known for ten years. This plate is covered with a black card, so that it may not become fogged by the light of the room when I draw the slide. All I have to do is to lay it upon the table below the Crookes tube so as to cast the shadow upon it, and after due exposure develop the plate in the ordinary well-understood way. Now it may be interesting to see the proof of the fact that bone is less transparent than flesh. So, with your permission, I will ask my little daughter to have her hand photographed. (Experiment made.)

At the time of Roentgen's announcement, the exposure required with the Crookes tubes that were then in existence was from twenty minutes to, I think, two or three hours. Very shortly improvements were made; and with these modern tubes one minute is quite suffi-

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* A Crookes "focus" tube (Jackson pattern), constructed by Messrs. Newton & Co., of Fleet Street, London.

† An Apps coil capable of giving sparks 25 centimetres in length, but on this occasion excited with only 5 cells, giving sparks about 6 inches in length.

cient for an exposure. Indeed one minute is too much for many objects. I have not previously tried this particular tube, though I judge by its appearance that it is in good condition. As soon as the exposure of one minute is over we will have the plate taken into the dark room and developed in the ordinary way; and when it is developed we will have it brought back into this room and put into the lantern, that you may see what has been done.

Now, while we are taking photographs, I may as well take a second to illustrate another point. Roentgen investigated in the most careful and elaborate way the relative transparency of different materials for these mysterious rays. He noticed that wood, and many substances which are opaque to ordinary light, are transparent to these rays; whilst, on the contrary, several substances that are transparent to light, such as calc-spar and heavy glass, are very opaque toward them. Many experimenters have examined this question of relative transparency. I devoted a day or two to the study of gems, and found that imitation rubies made of red glass are much more opaque than real rubies, and that paste diamonds are much more opaque than real diamonds. Real diamonds and rubies are indeed very transparent, and scarcely cast any shadows on the luminescent screen, though I have found diamond to be more opaque than an equal thickness of black carbon. There are laid upon this piece of card two rubies, one being only a glass ruby. There is also a row of four small diamonds. I will leave you to find out whether they are false or real. And then there are three larger diamonds, one of which is uncut and is a genuine South African stone. I lay them down upon a photographic plate and expose them to the Roentgen rays so that we may test their relative transparency. (The two photographs thus taken were projected upon the screen at the close of the lecture.)

Amongst the things which Roentgen told us was the fact that different kinds of glass are unequally transparent: that lead-glass, for instance, is much more opaque than soda-glass, or potash-glass, or, indeed, any glass which does not contain a heavy metal like lead. He found that practically the transparency was governed by the density; that the heavy or the dense substances were the more opaque. There is now some reason to correct that statement, though in the main as a first approximation it is perfectly true. Professor Dewar has shown that you must take into account, not the density in gross but the atomic weight. Taking any homologous series, for example, such as a number of sulphides, or oxides, or chlorides, that one which contains the atomically heavier metal will be the more opaque. Again, the bromide of sodium is more opaque than the chloride of the same metal, and the iodide is more opaque than the bromide. But as the correspondence between relative opacity and molecular or atomic weight breaks down when we try to pass from one series of compounds to a different series, there is some reason to carry the matter to a further degree of approximation. We must go

beyond the suggestion of atomic weight. The nearest approach to a law that I have been able to get at yet, on comparing tables of statistics, is that the transparency is proportional to the specific heat. For homologous series this is, of course, the same as saying that the transparency is inversely proportional to the molecular weight.

Roentgen found all the heavy metals to be remarkably opaque, while light metals like sodium and aluminium, and even zinc, are remarkable for their transparency. Aluminium, which is opaque to every known kind of light, is transparent, even in sheets half an inch thick, to these rays. Lithium, the lightest of solid metals, and with an atomic weight 7 as against aluminium 27, is so transparent that I have not been able yet even to see its shadow. Of all liquids water is the most transparent, and it has the highest specific heat of all of them.

Roentgen further found these rays to be incapable either of refraction by lens or prism,* or of reflection by any polished mirror. Reflection there is in one sense, that of diffuse reflection, such as white paper exercises on common light. No lens can concentrate these rays: they are also apparently incapable of being polarised. One difficulty in experimenting on these strange properties is that air itself acts as a turbid medium, reflecting back diffusely, as a smoky cloud would do for ordinary light, a portion of the rays.

Finding that these radiations differed in so many ways from ordinary light, and while resembling and even surpassing ultra-violet rays in their strong actinic properties, yet differed entirely from them in respect of the properties of refraction, reflection and polarisation, he named them "X-rays." To judge by his own writing, he appeared to wish that they might prove to be longitudinal vibrations in the ether, the possibility of the existence of which has been a subject of speculation on the part of some of the most learned of mathematical physicists. Others have suggested that these X-rays are transverse vibrations of a much higher frequency and shorter wave length than any known kind of ultra-violet light. Others again see in them evidence that radiant matter (i.e. kathodic streams of particles) can traverse the glass of a Crookes tube, and regard them as material in their nature. Lastly, it has been suggested that they may be neither waves nor streams of matter, but vortex motions in the ether.

To follow out the bearings of these speculations, as well as to trace the development of discovery, let us go back a little and consider what was the starting point of Roentgen's research. He was using a Crookes tube. It is one of the difficulties of my task to-night that I have to speak in the presence of him who is the master of us all in

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* Perrin in Paris, and Winkelmann in Jena, have independently found what they believe to be evidence of refraction through an aluminium prism. Both observers detected a slight deviation, but in a direction toward the refracting angle, showing aluminium to have for these rays a refractive index slightly less, with respect to air, than unity.

this subject of electric discharges in the vacuum tube. But to understand the discoveries of Crookes let us first witness a few experimental illustrations of the phenomena of electric discharges in vacuum tubes. Many of them have been known for half a century. We all know of the researches made in England by Gassiot, and by Varley and others, and the tubes of Geissler of Bonn are a household word. But there is one set of researches which deserves to be known far better than it is, that made by Dr. W. H. Th. Meyer, of Frankfort, whose pamphlet * I hold in my hand. In it he depicts a number of tubes in various stages of exhaustion, including one in that highest stage of exhaustion which one is prone to think of modern origin.

In order to illustrate the successive phenomena which are produced when electric discharges are sent through a tube during progressively increasing exhaustion, there is here exhibited a set of identical tubes. Each is a simple straight tube, having sealed in at each end an electrode terminating in a short piece of aluminium wire. The electrode by which the electric current enters is known as the anode, that by which it leaves the tube as the kathode. The only difference between these eight tubes lies in the degree of rarefaction of the interior air. The first one contains air at the ordinary pressure. As its electrodes are about 12 inches apart I am unable with the Apps induction coil (excited to throw an 8-inch spark) to send a spark through it. From the second tube about four-fifths of the air has been abstracted, and here we obtain a forked brush-like spark between the electrodes. The third tube has been exhausted to about one-twentieth part, and shows as the discharge a single thin red linear spark like a flexible luminous thread. When, as in the fourth tube, the exhaustion is carried so far as to leave but one-fortieth, the red line is found to have widened out into a luminous band which extends from pole to pole, while a violet mantle makes its appearance at each end and spreads over both of the electrodes. On carrying the exhaustion to the stage shown by the fifth tube, where only about $\frac{1}{300}$ of the original air is left behind, we note that the luminous column has broken up transversely into flickering striæ, that the violet mantle round the kathode has become more distinct, and is separated by a dark interval from the luminous red column, while a second and very narrow dark space appears to separate the violet mantle from the surface of the kathode. In the sixth tube the exhaustion has been carried to about $\frac{1}{10000}$. The flickering striæ have changed shape and colour, being paler. The light at the anode has dwindled to a small bright patch. The violet glow surrounding the kathode has expanded to fill the whole of that end of the tube; the dark space has become more distinct, and within it the kathode now shows on its surface an inner mantle of dull red light. There is a slight tendency for the

* Beobachtungen über das geschichtete electrische Licht, sowie über den merkwürdigen Einfluss des Magneten auf dasselbe: von Dr. W. H. Theodor Meyer. Berlin, 1858.

glass to show a greenish fluorescence near the kathode end. In the seventh tube the luminous column has subsided into a few greyish-white nebulous patches, the dark space round the kathode has greatly expanded, and the glass of the tube has now begun to show a yellow-green fluorescence. The exhaustion has been pushed so that only about $\frac{1}{30000}$ or less of the original air is present. In the eighth and last tube only one or two millionths of the original air have been left, with the result that the tube now offers an enormously increased resistance to the passage of the discharge. All the internal flickering nebulosities have vanished; the tube looks as though there were no residual air within. But now the glass itself shines with a fine yellow-green fluorescence which is particularly bright in the region around the kathode. Were the exhaustion to be carried much further the spark from this induction coil would no longer pass, so high would the resistance become. All these successive stages up to the last can be shown in one and the same tube attached to a modern rapid air pump. But for the proper production of the high vacua of the last stages, where electric shadows are alone produced, nothing short of a mercurial pump, either in the form invented by Sprengel or in that used by Geiesler (or one of the recent modifications) will suffice.

The phenomenon of fluorescence of the glass, which manifests itself when the exhaustion has become sufficiently high, was known in a general way as far back as 1869 or 1870. The tube next to be shown is a modern reproduction of a tube used at that time by Hittorf, of Münster. It differs from the tubes last shown by having a bend in it. Hittorf observed that when such a tube is exhausted sufficiently highly to give at the kathode the characteristic greenish-yellow fluorescence, this greenish-yellow fluorescence refused to go round the bend. It might appear at one end or the other, according to the direction in which the discharge was being sent, but would not go round the bend. The effect was as if the discharge went in straight lines from the bit of wire that served as kathode to the walls of the tube. Indeed shadow effects were observed by him, and by Wright, of Yale, and afterwards independently by Crookes, who greatly extended our knowledge of the facts. We may take this fact, that the fluorescence caused by the kathode will not go round a corner, as the starting point of the memorable researches of Crookes on radiant matter a score of years ago.

Before you are several tubes which illustrate the researches made by Crookes. The first is a simple glass bulb into which are sealed the two electrodes—the anode, by which the current enters, terminating in a bit of stout aluminium wire; the other, by which the current leaves, called the kathode, terminating in a small flat aluminium disk. The glass bulb was itself highly exhausted—how highly we shall presently see. From the flat front surface of the kathode, when sparks are sent through the bulb, a sort of back-discharge takes place in a direction normal to the surface. This

discharge, which only occurs at a very high degree of exhaustion, possesses several properties which distinguish it from all other kinds of discharge. It is propagated in straight lines, causes a brilliant luminescence wherever it strikes against the glass walls of the tubes, casting shadows of intervening objects, it heats the surface on which it impinges, and strikes them with a distinct mechanical force. Singular to relate, it is also capable of being deflected by a magnet as though it were a flexible conductor carrying the current. Struck by the singularity of these kathode rays or kathode discharges, which formed the subject of several beautiful researches, Crookes advanced the hypothesis that they consisted of flights of negatively electrified particles or "radiant matter." The particles he sometimes spoke of as molecules, sometimes as dissociated atoms, or, as we should now say, ions. He studied the wanderings of these flying particles by inserting within the bulb at different points auxiliary electrodes. He found the interior of the bulb to be positively electrified in all parts except within the dark space which surrounds the kathode, that is to say, except within the range of the actual kathode discharge. The kathode discharge itself was found to be possessed, to an extent exceeding any other known agency, of the power of exciting fluorescence and phosphorescence in minerals and gems. The kathode rays were themselves discernible as they crossed the interior of the tube. In such a bulb the kathode rays would form a blue streak impinging straight upon the anode. The kathode used in the next Crookes tube, is of a concave shape. Crookes found that, since the kathode rays left the surface normally, the result of curving the kathode was to focus the rays toward the centre of curvature. By so focussing the rays upon a bit of platinum foil, it was found possible to fuse and even melt the metal.

Unlike the discharges obtained at lower stages of rarefaction, the direction of these kathode rays was found to be independent of the position of the anode. He found kathode rays to be produced even when no internal electrodes were inserted, and when, instead, external patches of tinfoil were attached to the glass. Their mechanical action he studied by causing them to impinge upon the vanes of a pivoted fly which was thereby set into rotation. In a later experiment he caused the fly of a "molecule mill" to be set into rotation, not by the impact of the kathodic discharge but by the kinetic energy of the particles returning back toward the anode after they had impinged against the walls of the tube and lost their negative electric charges. A mere resumé of Crookes's work in those years beginning about 1869 or 1870, and extending not only for ten years actively, but going on at intervals until a year or two ago, would of itself fill a whole course of lectures. Into the controversy which has arisen between Crookes and the English physicists on the one hand and the German physicists on the other, there is no need to enter. Suffice it to say that while the German physicists mostly reject Crookes's hypothesis of radiant matter, and regard all these various phenomena as the

result of mere wave motions within the tube, the British physicists, including Lord Kelvin and Sir George Stokes, accept Crookes's view of the material nature of the kathode rays. Who, indeed, that has seen the molecule mill at work can doubt that, whether vibrations are present or not (and doubtless there are vibrations present), there are actually streams of moving particles as an essential feature of the kathodic discharge? For the moment the victory undoubtedly rests with the views of Crookes.

But of all these phenomena the one which concerns us most is that of the production of electrical shadows. Erecting in the path of the kathode rays an obstacle cut out in sheet metal—a cross of thin aluminium is the favourite object—a shadow of it is observed to be cast upon the wall of the tube behind it; the glass phosphorescing brilliantly except where shielded from the impact of the kathode rays, so that the shadow comes out dark against a luminous background. Common soda-glass gives this greenish-golden tint, while lead-glass exhibits a blue phosphorescence. Not glass alone, but diamonds, rubies, emeralds, calc-spar and other earthy materials, such as alumina, and notably yttria, produce the most brilliant effects under the kathode discharge, some of them only fluorescing transiently, others with a persistent phosphorescence. As a sample is shown a tube in which a sea shell, slightly calcined to remove organic matter, is made to emit a brilliant luminescence under the impact of rays from a kathode placed above it. The shell itself casts a shadow against the lower part of the tube. Some of the shadow effects are very mysterious and have recently claimed much of my attention. The size of the kathodic shadows is affected by the electrical state of the object. Electrifying it positively makes its shadow shrink to smaller dimensions. Electrifying it negatively causes a singular enlargement of the shadow. There seems to be no difference between the shadow of a metallic body and that of a non-metallic body of the same size. All bodies cast shadows, however thin. Even a film of glass $\frac{1}{10000}$ inch thick—so thin that it showed iridescence like a soap bubble—was found by Crookes to cast its shadow.

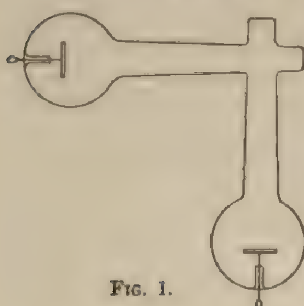

FIG. 1.

Another point noticed by Crookes was that if the exhaustion is carried very far, and the tube is stimulated by a sufficiently strong electromotive force, the phosphorescence may occur at points not in the line of discharge but round a corner. Not that the kathode rays turn the corner, however. Apparently some of the more quickly moving or perhaps more highly charged particles—atoms, molecules or ions—those, in fact, described by Crookes as "loose and erratic" —would manage to get round the corners and produce effects of a

more or less directly kathodic kind in places where they could not have penetrated by any motion in a straight line.

Here (Fig. 1) is a tube—a variation on one of Hittorf's, having two branches that cross one another at right angles. There are two small disks of aluminium in the bulbous ends to serve as electrodes. When either of these is made the kathode, the whole limb in which it is situated fluoresces brilliantly of a golden-green tint, particularly at the distant end. But the other limb remains dark, save for a little nebulous blue patch, near the anode, due to residual gas. Another tube (Fig. 2) is made as a zigzag, and here again only the end branch shines. On reversing the current the luminescence shifts to the other end. But when the tube is more highly exhausted, the phosphorescence is observed not only in the end branch where the kathode is, but also slightly at the end wall of each branch of the zigzag. Apparently the residual gas will act partly as its own kathode, and throw off something which causes the glass beyond to phosphoresce.

And now let me remark that not one of all the tubes shown since the first one, is capable of showing a shadow upon the fluorescent

FIG. 2.

screen outside, or of taking a photograph through a sheet of aluminium. Even the brilliant tube which showed so excellently the shadow of the cross, fails to show any result after hours of vain waiting. It yields no rays that will penetrate aluminium. For experiments with Roentgen rays it is absolutely necessary that the process of exhaustion should be carried beyond the stage that suffices for the production of kathode shadows; it must be pushed to about that limit which Crookes himself described as his unit for the degree of vacuum, namely, one-millionth of an atmosphere. I do not say that with long exposures photographs cannot be taken when the degree of exhaustion is lower. Something depends, too, upon the degree to which the electric discharge is stimulated, and something also depends upon the shape and structure of the tube and upon the size and shape of the kathode. But on none of these things does the emission of X-rays depend so much as upon the degree of vacuum. The highly exhausted vacuum is the one real essential.

So soon as Crookes's researches upon electric shadows had become known, electricians set to work to try to produce electric shadows in ordinary air without any vacuum. One of the ablest of experimenters,

Professor W. Holtz, was successful, using as a source of electric discharge the electrified wind which is given off by a metal point attached to the pole of an influence machine. If in a perfectly dark room such a point is placed opposite and at a few inches from a wooden disc covered with white silk and connected at its back or edges to the other pole of the machine, it will be observed to show a pale luminosity over a circular patch where it is struck by the electric wind. If then the object is brought between the disc and the point a shadow will be observed to be cast upon the white surface. Non-conductors do not cast shadows as well as conductors do. A piece of thin mica scarcely casts a shadow at all until it is moistened. Double shadows can be got by using two disks covered with silk facing one another: any conducting object introduced between them casts a shadow on

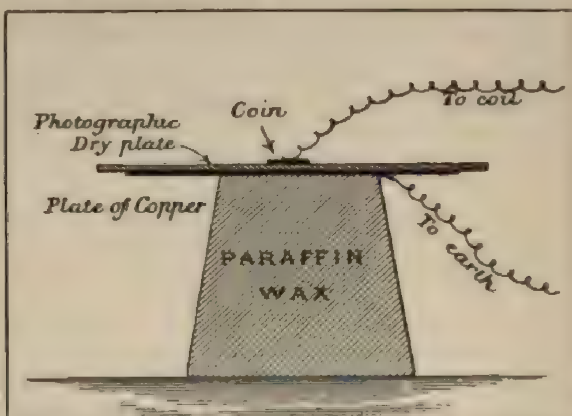

Fig. 3.

both. If such a shadow from an electrified point is cast downward upon a sheet of ebonite or pitch, the parts not shaded are found afterwards to remain electrified, and can be discovered by scattering over them Lichtenberg's mixed powders of red lead and lycopodium, thus perpetuating the shadow.

But now it is possible to produce electric shadows in another way, photographically, as has been known for some years,* from metal objects such as coins, by simply laying them down upon a photographic dry-plate (a gelatino-bromide plate) and sending an electric spark (from an induction coil) into them.

Fig. 3 shows the arrangement adopted by the Rev. F. J. Smith, who is kind enough to exhibit in the library to-night some scores of his

* 'Proceedings Physical Society of London,' vol. xi. p. 353, 1892.

beautiful "inductoscript" photographs. Upon the screen I throw a few samples, including a print of one of the jubilee coins (Fig. 4). These curious photographs are produced simply by the chemical action of the electric discharges which stream off from all the projecting portions, and so roughly reproduce an image of the coin. Since Roentgen's discovery many persons have announced their supposed discovery of the production of electric shadow-pictures without the aid of a Crookes tube. What they have really observed is, however, totally different. They have not been producing X-rays at all, but have merely rediscovered these inductoscript shadows.

FIG. 4.

Between the researches of Crookes, however, and those of Roentgen there came in a very remarkable body of researches in Germany. I have but to name Goldstein,* Puluj,† Hertz,‡

* Goldstein, in his researches on the Reflection of Electric (i.e. Kathode) Rays in 'Wiedemann's Annalen,' xv. 246, 1882, came very near to the discovery of the Roentgen rays. After pointing out that Hittorf had held the opinion that the kathode rays end at the place where they strike upon a solid wall, and that they are unable to proceed in any direction at all from thence, Goldstein directs attention to the circumstance that fluorescent patches are sometimes seen at the end of crooked tubes, where they could not have been caused by the direct impact of kathode discharges. He discusses the question whether this is due to reflection or to a deflection caused by the spot where impact first took place having become electrified negatively, and therefore acting as a secondary kathode. The latter hypothesis is rendered untenable by his observation that if the spot of first impact is made an anode the effect still occurs. He then shows that the phenomena are inconsistent with a specular reflection, but are explained by supposing that there is a diffuse reflection. He then sums up as follows:—"A bundle of kathode rays does not end, at least under those circumstances under which it excites phosphorescence, at the place where it strikes upon a solid wall, but from the place of impact on the wall there proceed electric rays in every direction in the gaseous space. These rays may be considered as reflected. Any solid wall of any property whatever may serve as a reflecting surface. It is immaterial whether or not it is capable of phosphorescence, or whether it consists of an insulator or of a conductor. The reflection is diffuse, no matter whether the surface is dull or most highly polished. An anode reflects the kathode rays sensibly as well as a neutral conductor or an insulator. The reflected rays have, like the direct kathode rays, the property to excite phosphorescence at their ends. They are subject to deflection, and their ends are deviated in the same sense as the ends of kathode rays, which would extend from the reflecting surface toward the place hit by the reflected rays."

† Puluj. "Radiant Electrode Matter and the so-called Fourth State." Published in vol. i. of 'Physical Memoirs,' by the Physical Society of London, 1889. These are translated from papers published in 1883 in the Memoirs of the Imperial Academy of Sciences of Vienna.

‡ H. Hertz. Researches on the Glow-Discharge, Wied. Ann. xix. 782, 1883. Hertz regards the kathode rays as a property of the ether, not as consisting of

Wiedemann,* and Lenard,† amongst the workers, to show what interest has been concentrated on the subject. Hertz, whose loss science has not ceased to lament, observed that a part at least of the kathode rays were capable of passing through thin aluminium sheet, a property which confirmed him in his previous doubt as to the material nature of the kathodic discharge. His pupil, Philipp Lenard, now Professor Lenard, of Aachen, took up the point. He fitted up a tube with a small window of aluminium foil opposite the kathode,

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moving particles. He finds the kathode rays to consist of a heterogeneous variety of kinds which differ from one another in their properties of causing phosphorescence, of being absorbed, and of being deflected by the magnet. On the Transmission of the Kathode Rays through Thin Layers of Metal, xlv. 28, 1892. Hertz finds that glass fluoresces in kathode rays, even if covered with gold leaf or thin films of various metals, though not if covered with thin mica. Aluminium was found best, and allowed fluorescence to occur even when a sheet of aluminium leaf was used so thick as to be opaque to light. A diaphragm of thin aluminium leaf on a metal frame placed inside a Crookes tube at 20 cm. from the kathode, permitted enough rays to pass to give a tolerably bright and even fluorescence over the whole of the further end of the tube. These rays, after passing through the leaf of metal, still showed rectilinear propagation (with some diffusion) and had not lost the property of being deflected by the magnet.

* E. Wiedemann's papers which are of special importance have mostly appeared in 'Wiedemann's Annalen.' The following are the chief of them. Some of the later have been written in collaboration with Prof. H. Ebert.

On the Phosphorescent Light excited by Electric Discharges, Wied. Ann. ix. 157, 1880.

On Electric Discharges in Gases, xx. 756, 1881.

On Fluorescence and Phosphorescence, Pt. I. xxxiv. 446, 1888.

On the Mechanism of Luminosity, xxxvii. 177, 1889.

On Kathodo- and Photo-Luminescence of Glasses, xxxviii. 488, 1889.

On Electric Discharges in Gases and Flames, xxxv. 209, 220, 234, 237, 255, 1888.

On Electric Discharges, xxxvi. 643, 1889.

On the Apparent Repulsion of Parallel Kathode Rays, xlvi. 158, 1892.

On Electric Discharges; Excitation of Electric Oscillations and the Relation of Discharge-tubes to the same, xlviii. 549, and xlix. 1, 1893.

Researches on Electrodynamic Screening-Action and Electric Shadows, xlix. 32, 1893.

Luminous Phenomena in Electrode-less rarefied Spaces under the Influence of rapidly alternating Electric Fields, l. 1, 221, 1893.

With J. B. Messerschmitt, on Fluorescence and Phosphorescence, Pt. II. Validity of Talbot's Law, xxxiv. 463, 1888.

With H. Ebert, on the Transparency of Kathode Deposits, Sitzber. d. phys.-med. Soc. zu Erlangen, Dec. 14, 1891.

† Lenard's papers are :—

Note on a Phosphoroscope, with spark illumination, Wied. Ann. xxxiv. 918, 1888.

With M. Wolf, Luminescence of Pyrogallic Acid, xxxiv. 918, 1888.

With V. Klatt, on the Phosphorescence of Copper, Bismuth, and Manganese in the Sulphides of Alkaline Earths, xxxviii. 90, 1889.

On Kathode Rays in Gases at Atmospheric Pressure, and in the most extreme vacuum, li. 225, 1894.

On the Magnetic Deflexion of the Kathode Rays, lii. 22, 1894.

On the Absorption of the Kathode Rays, lvi. 255, 1895.

its form being that shown in Fig. 5. The kathode was a flat disk on the end of a glass-covered wire stem. The anode was a cylindrical tube of brass surrounding the kathode. Upon the further end of the tube a brass cap was fixed by means of vacuum-tight cement. Over a small orifice in this brass cap was set the aluminium window of foil only $\frac{1}{400}$ millimetre thick. By this means he was able to do what had previously been supposed impossible, bring the kathode rays out into the open air. Or, at least, that is what he appears to have considered that he was doing. Certainly he succeeded in bringing out from the vacuum tube rays that, if not actual prolongations of the kathode rays, were closely identified with them. He examined their properties both in the open air and in gases contained in a second chamber beyond the window, and found them to be capable of producing photographic impressions on sensitive plates. He further examined the question whether they can be deflected by a magnet. Fig. 6, which is copied from Lenard's paper, shows the results. The row of spots on the left side shows the photographic effect under various different conditions of experiment when there was no magnet present. The spots in the right-hand row show the effects obtained when a magnet was present. For example, in the third row from the top it is seen that the bundle of rays when subjected to the influence of the magnet is partially dispersed, the spot being enlarged sideways and having a kind of nebulous tail. This proves that through the aluminium window there came some rays which were deflected by a magnet, and some rays also which were not deflected by a magnet. The question naturally arises whether the rays which Lenard had thus succeeded in bringing out into the open air are the same thing as the rays with which Crookes

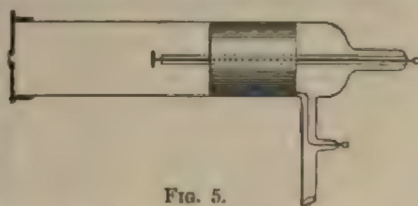


FIG. 5.

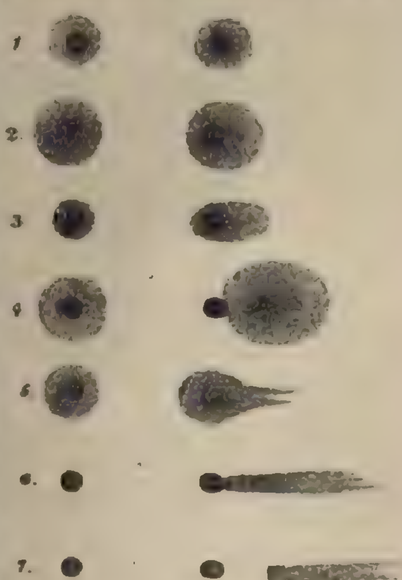


FIG. 6.

had been working with inside the vacuum. To that question the final answer cannot yet be given. Certainly some of the Lenard rays resemble the interior kathode rays: but some differ in the crucial respect of deflectability by the magnet. The higher the degree of vacuum, the less are the rays deflected.

Having touched all too briefly upon the researches of Lenard, it remains for me to speak of those of Wiedemann, of Erlangen, who for many years has made a study both of the phenomena of electric discharge and of those of fluorescence and phosphorescence. In a research made in the year 1895 he attained some results of singular interest. He had been making electric discharges, in collaboration with Professor Ebert, by a special apparatus for producing electric

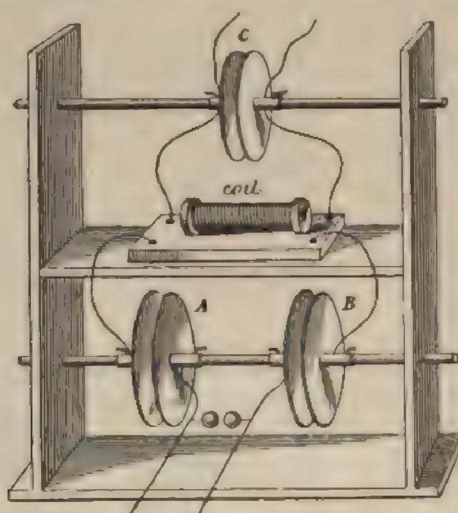


FIG. 7.

oscillations of high frequency. This apparatus, in the modified form given to it by Ebert,* stands on the table before you. It is an apparatus of the same class as that described here some years ago by Oliver Lodge, for producing Hertzian waves. An oscillating spark is produced between two polished balls set between two condensers A and B, each made of plates of sheet zinc (Fig. 7) a few millimetres apart. Their external circuit is, however, led into the primary of a small induction coil, the secondary of which goes to a third condenser C. When sparks from the Apps coil are sent to the spark-gap, the oscillations of the two primary condensers set up secondary oscilla-

* 'Wiedemann's Annalen,' liii. p. 144, 1894.

tions in the third condenser, to which a vacuum tube can be connected. If, now, by adjusting the distances between the plates of condensers we tune the primary and secondary circuits together, the electric oscillations that result will persist much longer than if the circuits are not so tuned. Though each oscillation may last less than the 100-millionth of a second, there will be at each spark some 20,000 or 30,000 oscillations before they have died out. Wiedemann and Ebert have found that these persistent oscillations are specially adapted to excite luminescence. To illustrate the point I select here an old Geissler tube with a comparatively poor vacuum. When stimulated by ordinary sparks directly from the Apps coil through the platinum electrodes at its ends, it shows the usual features of Geissler tubes: there is a luminous column extending through the central bulb with stratifications along its length, while around the kathode is the usual violet glow. The glass shows no fluorescence. I now charge the connections, uniting the wires from Ebert's apparatus, not to the terminal electrodes of the tube but to two patches of tin-foil stuck upon the outside of the central bulb. Under these conditions the electric oscillations illuminate the central bulb with a glow quite different from that previously seen. Beneath each patch of foil you can discern the bluish kathode discharge, and the glass now shines with characteristic apple-green fluorescence. By moving one plate of one of the condensers in or out I alter the conditions of resonance in the circuit; and when the tuning is best the fluorescence is at its brighest. Now Wiedemann observed * that the light so generated is capable of exercising a photographic action and of other effects, but is incapable either of passing through a thin plate of fluor-spar or of being deflected by a magnet. These rays differed therefore both from ultra-violet light and from kathode rays; hence Wiedemann pronounced them to consist of a new species which he named "Entladungsstrahlen" or discharge-rays. It is again a matter for research to determine whether Wiedemann's rays are the same as Lenard's, or as Roentgen's rays. Wiedemann's coadjutor Ebert went on with the research and produced on this principle a little "luminescence lamp" having two external rings of foil as electrodes; and within the vacuum bulb a small pastile of phosphorescent stuff, which, when excited by the oscillations of the tuned circuits, glows with a small bright light. Ebert claims that its efficiency is many times greater than that of the ordinary glow lamp.

Returning now to Roentgen's researches, we will take a glance at the kind of tube (Fig. 8) which he was employing when he made his discovery of the X-rays. Its general resemblance to previous tubes † is self-evident. The anode was a piece of aluminium tube through which passed the glass-covered kathode wire, with a small

* 'Zeitschrift für Elektrochemie,' July 1895, p. 159.

† It is, in fact, identical with the form described by Hertz in 1883, see 'Wiedemann's Annalen,' xix. p. 810.

flat aluminium plate on its extremity. From this flat plate the kathode rays shot forward against the bulging end of the tube; and, without any aluminium window rays which were capable of exciting fluorescence, found their way through the glass walls. Lenard had so boxed up his tube with brass cap and metal case, that if anything in the way of rays struggled through the glass walls of his tube he might not notice it. Possibly he never looked for it. Roentgen made the fortunate observation that when his tube was closely covered with opaque black card it still could cause fluorescence on a screen covered with platino-cyanide of barium on which shadows were cast. From seeing the shadows thus to securing their imprint permanently on a photographic plate was but a small step, and the discovery that they could pass freely through a sheet of the metal aluminium was a natural result of an inquiry as to the transparency of different materials. Aluminium is to these rays much more transparent than ordinary glass. No lens can focus them, nor mirror reflect them; and, unlike the kathode rays within the tube, they are not deflected by the magnet.

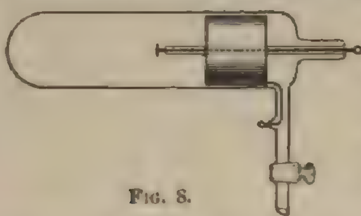


FIG. 8.

The criterion which we have at present as to whether any rays from any other source are or are not the same as the X-rays is that they shall be able to fulfil the following four-fold test:—They must be capable of exciting luminescence; they must be capable of impressing an image on a photographic plate; they must be capable of passing through aluminium; and they must be incapable of being deflected by a magnet. In addition they must—so far as present evidence goes—be incapable of being either refracted or polarised. Any rays that will fulfil these tests must for the present be considered identical with X-rays.

Now it has been suggested that the X-rays are the same as ultra-violet light. This is certainly not so, for ultra-violet light, as known to us by the researches of Stokes, Tyndall, Becquerel and Cornu, will not go through aluminium and is not deflected by a magnet, though it will excite luminescence and take photographs. Furthermore ultra-violet light can be refracted and polarised.

It has also been suggested that the X-rays are merely invisible heat-rays. But this is certainly untrue also, because although Abney has succeeded in taking photographs by heat rays, they will not go through aluminium, are not deflected by the magnet, and instead of exciting phosphorescence they destroy it, as Goethe found out nearly a hundred years ago.

Neither are they Hertzian waves of longer period than the heat waves.

So far as is at present known there is no other way of producing

the X-rays than that of employing the highly exhausted vacuum tube. They are not found in the light of ordinary electric sparks in air. They are not discoverable amongst the rays emitted by ordinary Geissler tubes with a low exhaustion. They are not found in sunlight or any artificial light. The arc light, though it yields rays that will give photographic shadows through a thin pine-wood board, yields no rays that will pass through aluminium. The only other rays that seem to come within reasonable possibility of being X-rays are the Lenard rays, some of which are probably identical with Roentgen's; the Wiedemann rays, which are, so far as yet investigated, entirely similar; and the Becquerel rays, to which some allusion will presently be made. It will, however, be convenient here to present a synoptic table (see p. 208) of the various kinds of rays and their respective physical properties.

One other physical property of the X-rays has been discovered since the publication of Roentgen's research. It was discovered simultaneously in Cambridge (by Professor J. J. Thomson), in Paris, in Bologna, and in St. Petersburg, that these X-rays will cause the diselectrification of an electrified body, no matter whether it is positively or negatively charged.* That ultra-violet light can diselectrify bodies that have been negatively charged was previously known from the researches of Hertz, and of Elster and Geitel. This fresh discovery that X-rays will also discharge a positive electrification sets up a new physical test. Let me show you a simple piece of apparatus which I have found very convenient for the purpose of demonstrating this discovery. It is an aluminium-leaf electroscope (Fig. 9) entirely shielded from all external electrostatic influences by being enclosed in transparent metallic gauze. It is so well shielded that even when the cap is removed it cannot be charged in the ordinary inductive way, but must be electrified by direct conduction. The aluminium leaves hang at the side of a fixed central plate as in Exner's electroscope. The containing vessel is of thin Bohemian glass. On exciting the instrument positively from a rod of rubbed glass, or negatively from a rod of rubbed celluloid, the leaves diverge. In either case as soon as the X-rays are caused to shine upon the instrument the leaves fall.

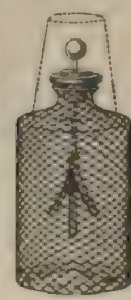


Fig. 9.

It occurred to me that by the aid of this property of diselectrification it might be possible to produce electric shadows without having resort to any photography. You are aware that if the surface or any part of the surface of a body is electrified, the fact that it

* It is of great interest to note that this identical property had been observed by Lenard a year previously as an effect of his rays. He found they would discharge an electroscope enclosed in a metal chamber, with an aluminium sheet in front, whether positively or negatively charged, and at a distance of 30 centimetres from his tube.

TABLE I.

Kind of Rays.	Cause Luminescence.	Penetrate Aluminium.	Cause Photographic Action.	Cause Combination of H and Cl.	Deflected by Magnet.	Discharge Electrification.	Affect Spark Length.	Change Nature of Electric Discharge.	Restore Thermo-luminescence.	Capable of being Polarised.	Capable of Refraction, &c.
Ultra-violet light	Yes	..	Yes	Yes	No	If —	Yes	..	Yes	Yes	Yes
Infra-red light	No	No	..	No	No	No	..	..	No	Yes	Yes
Hertzian waves	No	No	..	No	No	..	Yes	..	No	Yes	Yes
Kathode rays	Yes	If thin	Yes	..	Yes	{ charge violently }	..	..	..	..	..
Lenard rays	Yes	Yes	Yes	..	Partly	Yes	Yes	Yes	..	..	..
Wiedemann rays	Yes	Yes	Yes	..	No	..	..	..	Yes	..	..
Roentgen rays	Yes	Yes	Yes	..	No	Yes	Yes	Yes	Yes	No	No
Becquerel rays	..	Yes	Yes	..	No	Yes	..	..	..	Yes	..
Electric effluve	Yes	No	Yes	Yes	?	..	..	..	Yes	No	No

is electrified can be ascertained by dusting over it mixed powders of red lead and sulphur (or red lead and lycopodium). With the aid of Mr. Miles Walker, who has worked with me all through this matter, I have succeeded in producing, on this plan, well-defined shadows which will now be demonstrated to you. A clean sheet of ebonite freed from all traces of previous electrification by being passed through a spirit flame is laid on a properly prepared metal table. On it stands a small tray of thin aluminium supported on four insulating legs. In this tray is placed the object whose shadow is to be cast, for example a pair of scissors or an object cut out in sheet lead. Over this again is placed a leaden cover with an opening above the tray: the leaden cover being designed to cut off electrostatic influences which might interfere. The tray is then electrified by a small influence machine, and while it is so electrified X-rays are sent downwards from a Crookes tube placed above. They pass down through the aluminium tray and carry its electrification to the ebonite sheet, which therefore becomes electrified all over except in the parts which are shielded by the scissors or other metallic object. The sheet of ebonite is then removed from the leaden enclosure, the aluminium tray lifted off, and the mixed powders are dusted over, adhering to the surface of the ebonite and revealing the otherwise invisible electric shadow. Fig. 10 is a shadow taken in this way. It is but right to mention that Professor Righi, of Bologna, has independently obtained electric dust shadows in a very similar way since these experiments of mine were begun.

This will be a convenient place to mention a new effect of X-rays which I have recently observed and which is set down in the table. When X-rays fall upon a metal object electrified by an influence machine, they produce some curious changes in the nature of the discharge into the air. If the body is already discharging itself from some edge or corner in an *aigrette* or brush discharge (visible in darkness only) the size and form of the aigrette is much altered. Under some circumstances not yet investigated, the incidence of X-rays causes the aigrette to disappear; under others the X-rays provoke its appearance.

Since the publication of Roentgen's research the most notable advance that has been made has been in the direction of improving the tubes. Roentgen himself has mostly employed a pear-shaped tube with a flat circular kathode near the top, producing a beautiful fluorescence of the lower part of the tube. He carefully verified the circumstance that the X-rays originate at that portion of the glass surface which receives the impact of the kathodic discharge. They appear in fact to be generated at the place where the kathode discharge first impinges upon the surface of any solid body. It is not necessary that the substance which is to act as emitter of the X-rays should become fluorescent. On the contrary, it appears that the best radiators are substances that do not fluoresce, namely the

metals. I have found zinc, magnesium, aluminium, copper, iron and platinum to answer—the last two best.* Mr. Porter, of University College, and Mr. Jackson, of King's College, have independently found out the merits of platinum foil, the former using an old Crookes tube designed for showing the heating effect of the kathode discharge when

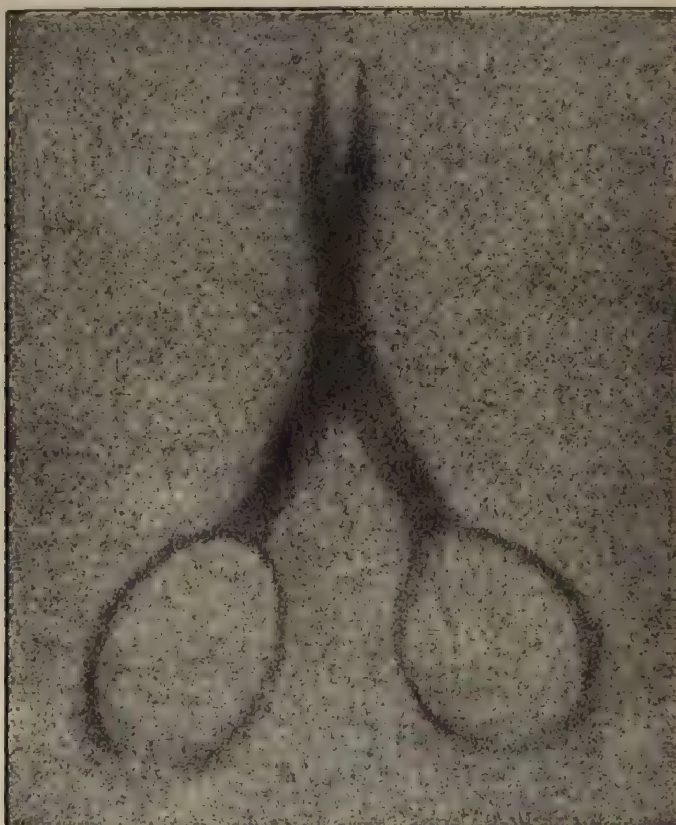


FIG. 10.

concentrated by a concave kathode. On the table are some of the experimental forms † of tubes I have used. The best results are found when the kathodic discharge is directed against an interior

* [The author has since found metallic uranium to surpass all other metals.]
† See 'Philosophical Magazine,' August 1896, p. 162.

piece of metal—preferably platinum—which I term the antikathode * set obliquely opposite the kathode, and which serves as a radiating surface from which the X-rays are emitted in all directions. When experimenting with various forms of tube, I have spent much time in watching, by aid of a fluorescent screen, their emissive activity during the progress of exhaustion. As already mentioned, X-rays are not emitted until the stage of minimum internal resistance has been passed. As the exhaustion advances, while resistance rises and spark length increases, there is noticed by aid of the screen a luminosity in the bulb, which, faint at first, seems to come both from the front face of the bit of platinum that serves as antikathode, and from the back face; an oblique dark line (Fig. 11), corresponding to the plane of

FIG. 11. FIG. 12.

the antikathode, being observed in the screen to separate the two luminous regions. On slightly increasing the exhaustion the emission of X-rays from the back of the antikathode ceases while that from the front greatly increases (Fig. 12), and is quite bright right up to the angle delimited by the plane of the antikathode. There is something mysterious, needing careful investigation, in this lateral emission of X-rays under the impact of the kathode discharge.

Of all the many forms of tube yet produced none has been found to surpass the particular pattern devised by Mr. Sydney Jackson

* Comptes Rendus, cxxii. p. 807.

(Fig. 13), and known as the "focus tube." It was with such a tube that I showed you at the outset the fundamental experiments of Roentgen. A concave polished kathode of aluminium concentrates the kathodic discharge upon a small oblique sheet of platinum, which, while acting as antikathode, serves at the same time as anode. Not only does the concentration of the kathodic discharge upon the metal cause it to emit X-rays much more vigorously, but it also has the effect of causing them to be emitted from a comparatively small and definite source, with the result that the shadows cast by opaque objects are darker. [Photographs were then thrown upon the screen, those taken with "focus" tubes showing remarkable definition of detail. Some of these were by Mr. J. W. Giffen; others, showing diseased bones, &c., taken by the lecturer, and some by Mr. Campbell-Swinton and by Mr. Sydney Rowland, were also projected.]

The objection has been taken that in these shadow photographs it is impossible to distinguish the parts that are behind from those that

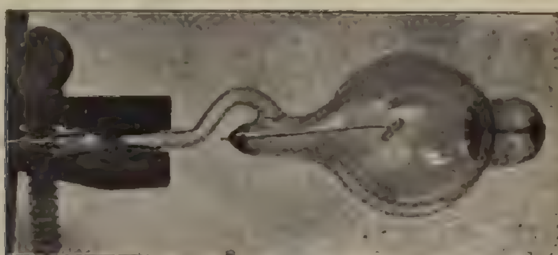


Fig. 13.

are in front. In a sense that is so. But I venture to say that the objection not only can be got over but has been got over. I cannot show the proof of my assertion upon the screen, because I cannot put upon the screen a stereoscopic view. But here in my hand is the Roentgen stereograph of a dead tame rabbit. Two views were taken, in which the X-rays were thrown in two different directions at an angle to one another. When these two views are stereoscopically combined, you observe the rabbit's body with the lungs and liver inside in their relative positions. The soft organs, which cast faint shadows almost indistinguishable amid the detail of ribs and other tissues, now detach themselves into different planes and are recognisable distinctly. I now send up for projection in the lantern the two photographs that were taken at the beginning of my discourse, and which have in the meantime been developed.

Turning back to the phenomena of luminescence,* permit me to

* This very convenient term was suggested some six years ago by Wiedemann, to denote the many phenomena known variously as fluorescence or

draw your attention to the accompanying table of the different kinds of luminescence with which the physicist has to deal.

TABLE II.

Phenomenon.	*Substance in which it occurs.*
1. Chemi-luminescence	Phosphorus oxidising in moist air; decaying wood; decaying fish; glow-worm; fire-fly; marine organisms, &c.
2. Photo-luminescence:	
(*a*) *transient* = Fluorescence ..	Fluor-spar; uranium-glass; quinine; scheelite; platino-cyanides of various bases; eosin and many coal-tar products.
(*b*) *persistent* = Phosphorescence	Bologna-stone; Canton's phosphorus and other sulphides of alkaline earths; some diamonds, &c.
3. Thermo-luminescence	Scheelite; fluor-spar.
4. Tribo-luminescence..	Diamonds; sugar; uranyl nitrate; pentadacylparatolylketone.
5. Electro-luminescence:	
(*a*) Effluvio-luminescence	Many rarefied gases; many of the fluorescent and phosphorescent bodies.
(*b*) Kathodo-luminescence	Rubies, glass, diamonds, many gems and minerals.
6. Crystallo-luminescence	Arsenious acid.
7. Lyo-luminescence	Sub-chlorides of alkali-metals.
8. X-luminescence	Platinocyanides, scheelite, &c.

You will note the names given to discriminate from one another the various sorts of luminescence. Chemi-luminescence denotes that due to chemical action, as when phosphorus oxidises, or when the glow worm emits its cold light. Then there is the photo-luminescence of the bodies which shine when they are shone upon. There is the thermo-luminescence of the bodies which shine when heated. There is tribo-luminescence caused by certain substances when they are rubbed. There is the kathodo-luminescence of the objects placed

phosphorescence. It refers to all those cases in which light is produced, whether under the stimulus of electric discharge, of heat, of prior exposure to illumination, or of chemical action, and the like, in which the light is emitted at a lower temperature than that which would be necessary if it were to be emitted by means of incandescence.

in a Crookes tube. There is the crystallo-luminescence of certain materials when they become solid; and the lyo-luminescence of certain other materials when they are dissolved. Lastly, there is the X-luminescence set up by the X-rays.

Pausing on photo-luminescence, here is an experiment to illustrate the difference between its two varieties, phosphorescence and fluorescence. Light from an arc lamp, filtered from all rays except violet and ultra-violet, is thrown upon a disk to which rapid rotation is given by an electric motor. The disk is painted with two rings, one of sulphide of calcium, the other of tungstate of calcium. Though the light falls only on one patch you note that the sulphide shows a continuous ring of blue light, for the emission of light persists after the stuff has passed out of the illuminating rays. The tungstate, on the other hand, shows only a short trail of light, the rest of the ring being non-luminous, since tungstate has but little persistence. The light has in fact died out before the stuff has passed a quarter of an inch from the illuminating beam. This is a sort of phosphoroscope designed to show how long different materials will emit light after they have been shone upon. Those which show only a temporary luminescence are called fluorescent, while those with persistent luminescence are called phosphorescent. For many years it has been known that some diamonds are phosphorescent. Three such are here shown,* which, after exposure of one minute to the arc light, shine in the dark like glow-worms. The most highly phosphorescent material yet produced is an artificial preparation of sulphide of calcium manufactured by Mr. Horne. The specimen exhibited has a candle-power of about $\frac{1}{16}$ candle per square inch after exposure for a few seconds to direct sunlight; but the brilliancy rapidly dies away, though there is a visible luminescence for many days. This substance is also brightly luminescent in a Crookes tube, and less brightly under the influence of X-rays.

Many substances, notably fluor-spar, have the property of thermo-luminescence, that is they shine in the dark when warmed. Powdered fluor-spar dropped upon a hot shovel emits bright light. If, however, the spar is heated to a temperature considerably below red heat for some hours, it apparently comes to an end of its store of luminous energy and ceases to shine. Such a specimen, even after being kept for some months, refuses to shine a second time when again heated. It has, however, long been known that the property of luminescing when warmed can be restored to the spar by passing a few electric sparks over it, or by exposing it to the silent discharge or aigrette. Wiedemann having found that the kathode rays produce a similar effect, it occurred to me to try to find out whether any of these X-rays also would revivify thermo-luminescence. I have found that on exposure for twenty minutes to X-rays, a sample of fluor-spar

* Kindly lent by Dr. J. H. Gladstone, F.R.S.

which had lost its thermo-luminescent property by prolonged heating was partially though not completely revivified.

I referred earlier to the rays recently discovered by M. H. Becquerel. In February last M. Becquerel, and independently I myself,* made the observation that uranium salts emit some rays which very closely resemble the X-rays, since they will pass through aluminium and produce photographic action. It remains to be seen whether these rays are identical with those of Roentgen.

Finally, let me briefly exhibit two results of my own work. There is now shown (Fig. 14) the photographic shadow of two half-hoop ruby rings. One of them is of real rubies, the other of imitation stones. By artificial light it is difficult to distinguish one from the other, but when viewed by the X-rays there is no mistaking the false for the true. The real rubies are highly transparent, those of glass are practically opaque.

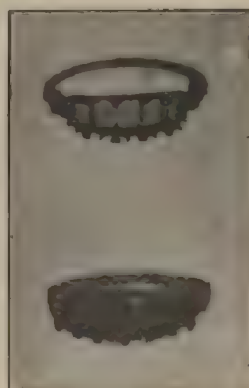


FIG. 14.

After gaining much experience in judging by photography of the relative transparency of materials, I made a careful research † to discover whether these rays can be polarised. At first I used tourmalines of various thicknesses and colours. More recently I have tried a number of other dichroic substances, andalusite, sulphate of nickel, of nickel and ammonium, sulphate of cobalt, and the like. The method used for all was the following. A slice of the crystal was broken into three parts. One part was laid down, and upon it were superposed the other two in such a way that in one the crystallographic axis was parallel, in the other perpendicular, to the crystallographic axis in the first piece. If there were any polarisation the double thickness where crossed in structure would be more opaque than the double thickness where the structure was parallel. Not the slightest trace of polarisation could I observe in any case. Of numerous other observers who have sought to find polarisation, none has yet produced a single uncontestable case of polarisation.

At the present moment interest centres around the use of luminescent screens for observing the Roentgen shadows, and in this direction some advances have been claimed of late. It should, however, not be forgotten that Roentgen's original discovery was made with a screen covered with platino-cyanide of barium. Here is a piece of card covered with patches of several different kinds of lumi-

* See 'Philosophical Magazine'; July 1896.
† *Ib.* August 1896.

nescent stuffs, several platino-cyanides. several sulphides, and some samples of tungstate of calcium. Of these materials the brightest in luminescence is the hydrated platino-cyanide of potassium employed by Mr. Sydney Jackson ; the next brightest is a French sample of platino-cyanide of barium; platino-cyanide of strontium coming third.

Using a focus tube of Mr. Jackson's improved pattern, enclosed in a box with a cardboard front, and taking a platino-cyanide screen, I am able in conclusion to demonstrate to all those of my audience who are within a few feet of the apparatus, the facts that have so startled the world You can see the bones of my hand and of my wrist. You can see light between the two bones of my forearm; while metal objects, keys, coins, scissors, &c., enclosed in boxes, embedded in wood blocks, or locked up in leather bags, are plainly visible to the eye.

Whatever these remarkable rays are, whether they are vortices in the ether, or longitudinal vibrations, or radiant matter that has penetrated the tube, or, lastly, whether they consist simply of ultra-violet light, their discovery affords us one more illustration of the fact that there is no finality in science. The universe around us is not only not empty, is not only not dark, but is, on the contrary, absolutely full and palpitating with light: though there be light which our eyes may never see, and sounds which our ears may never hear. But science has not yet pronounced its last word on the hearing of that which is inaudible and the seeing of that which is invisible.

[S. P. T.]

WEEKLY EVENING MEETING,

Friday, May 15, 1896.

Sir Frederick Bramwell, Bart. D.C.L. LL.D. F.R.S. Honorary Secretary and Vice-President, in the Chair.

Alexander Siemens, Esq. M. Inst. C.E. *M.R.I.*

Cable Laying on the Amazon River.

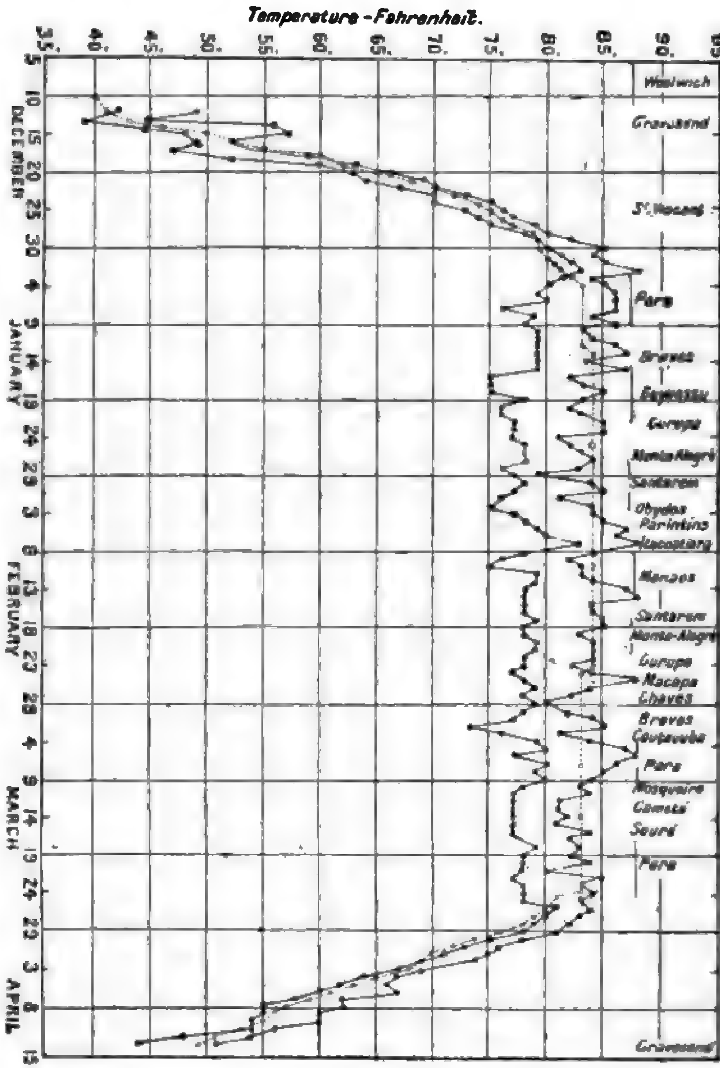
When it had been decided to connect Belem, the capital of the State of Pará, by means of a subfluvial cable with Manaos, the capital of the State of Amazonas, a preliminary journey became necessary, during which landing places at the various intermediate stations had to be selected, some reaches of the river explored, as no reliable charts exist, and various other details ascertained in order to facilitate the laying of the cable. This preliminary survey took place in October of last year during the hottest season, when the river was at its lowest; while the cable was laid during January and February of this year, when the rainy season had commenced and the river was rising.

The difference in temperature between the two journeys was on the average not more than about $5\frac{1}{2}^{\circ}$ Cent. (10° Fahr.), but a great advantage during the laying was the almost continuous presence of clouds, which mitigated the fierce heat of the sun and kept the temperature at a very pleasant level.

A diagram on the next page shows the curves of the variation in temperature during the cable-laying expedition, giving the daily maximum and minimum temperature registered by a thermometer hung up under the officer's bridge in the open air, but sheltered from the sun. The third curve represents the temperature of the water, which was measured by a thermometer on the refrigerating machine, fixed at a point where the water pumped in from outside first enters the machine. Besides the date, the places where the observations were taken are marked on the diagram, and it will at once be noticed how very equable the temperature was on the main river. The fluctuations in the air temperature mostly indicate the absence or presence of clouds, but the water temperature remained perfectly constant during the whole time spent on the upper reaches of the river, the proximity of the sea lowering the temperature only to a small extent.

A glance at the map of South America explains, without much

comment, how immense the volume of water must be which is collected from an area measuring more than $2\frac{1}{2}$ million square miles, for the most part covered with dense forests; and it follows that the



temperature of such a body of water cannot be seriously affected by the daily variations of temperature indicated by the first two curves. It is extremely difficult to realise the true proportions of this river, but the comparative table, in which the dimensions of the principal rivers of the various continents are contrasted with those of the Amazon, will help to show the importance of this great system of natural waterways.

Name.	Length in Statute Miles.	Watershed, Square Miles.	Average Discharge, Cubic Feet per Second.	Length of Navigable Waters in Miles.
Mississippi	2,616[1]	1,280,300[6]	675,000	35,000
La Plata	2,400	994,900[6]	700,000	20,000
St. Lawrence	2,200	565,200[6]	1,000,000[7]	2,536
Nile	3,370	1,293,050[6]	61,500	3,000[3]
Volga	2,325	592,300[6]	384,000[2]	14,600
Danube	1,735	320,300[6]	205,900	1,600[3]
Rhine	810	32,600[6]	..	550[3]
Thames	210	6,010	2,220[4]	200[3]
Amazon	2,730[5]	2,229,900[6]	2,400,000[8]	50,000[8]

	Square Miles.
Area of Great Britain and Ireland	120,626
„ British India	1,560,160
„ Brazil	3,219,000
„ Europe	3,790,000

With several other large rivers, the Amazon shares the fate that its name changes several times during its long course, and that at various times different affluents have been considered to be the true source of the main stream. Most geographers, however, regard the Marañon as the principal river, a branch of which, called Tunguragua, rises in Lake Lauricocha, in Peru, in 10° 30′ S. lat., and

[1] To source of Missouri, 4300 miles.
[2] At Saratoff.
[3] Exclusive of tributaries.
[4] At Teddington.
[5] To sources of Apurimac, 3415 miles.
[6] According to Dr. John Murray.
[7] According to Darby, the American hydrographer.
[8] According to Dr. Lauro Sodré.

76° 10' W. long., although the Ucayale, where it unites with the Marañon at Nauta (4° S. lat., 73° W. long.), is quite as important as the Marañon. If the greatest distance from the mouth is to decide the question, then the source of the Apurimac, an affluent of the Ucayale, can lay claim to being the origin of the Amazon, rising in Peru in 16° S. lat. and 72° W. long

From the Lake Lauricocha the main direction of the Tunguragua and the Marañon is to the N.N.W., until the river turns eastward, and shortly after passing Jaen breaks through the Andes, entering the plains of the Amazon valley by the Falls of Manseriche, a short distance west of Borja. Its further course is a little north of east, until it pours its yellow waters into the Atlantic under the equator between the Cabo do Norte and the Cabo Maguari, which are 158 miles apart. This distance is just about equal to the distance from Land's End to Cape Clear in Ireland, or from Brighton to Falmouth. Even west of the island of Caviana, which lies in the mouth of the river, together with the island of Mexiana and several smaller ones, the width of the main stream is over 50 miles, equal to the distance from Portland Bill to the Cap de la Hague. The part of the Amazon flowing north of the Island of Marajó may therefore be compared in width to the Channel, but in depth and volume of water it far surpasses it. It is a disputed question whether the water flowing south of Marajó, commonly called the Pará river, should be considered as part of the Amazon or not. A network of natural canals, "the narrows," connects the two waterways west of Marajó, but the influence of the tide makes it difficult to decide whether part of the water of the Amazon finds its way south of Marajó or not. Along the old course of the Amazon, commencing at the foot of the Andes, a similar network of islands and canals is formed on both sides of the river, as the whole country is almost level, and is consequently inundated during the rainy season for hundreds of miles by the rivers flowing through it. The most notable exception to this general state of things occurs at Obidos, where the whole volume of water is compressed into one channel a little over a mile wide, and said to be about forty fathoms in average depth.

A sounding taken opposite Obidos, about a third of the distance across the river, showed a depth of 58 fathoms, measured by a steel wire and Sir William Thomson's sounding machine. As the current of the river averages three knots in the main channel, it is not easy to take soundings by an ordinary lead line, and even with the steel wire an extra heavy weight (33 lbs.) has to be employed, or the results are not reliable. Besides the wire sounding machine, James's Submarine Sentinel was used on the preliminary voyage, wherever serious doubts existed about a channel through which the cable was to be laid. Usually the sentinel was set at five fathoms, and when it struck a bar the ship was stopped, and a series of soundings taken to ascertain the exact depth of water and the extent of the

shallow place. A further difficulty in sounding originated from the soft nature of the soil, which for the greater part of the Amazon valley is alluvial clay, and allows the lead to sink into it for several feet.

In the narrows there appears, however, a bank of hard clay, called Tabatinga, which unfortunately blocks nearly all the branches of the narrows and creates bars all along the course of the Tajipuru, the main westerly waterway connecting to the Gurupá branch of the main river. Occasionally the same hard clay forms shallows in the main river, but as a rule the section of all the channels resembles the capital letter, U, i.e. the sides are very steep and the bottom flat. In this respect, as in many others, the Amazon differs entirely from the Indian rivers, which build up their beds above the surrounding country, occasionally breaking through their natural banks and seeking a new bed. The Amazon, on the other hand, carries with it only the light clay sediment which forms the soil of the whole valley, and the inducement for the main stream to alter its course is therefore very small, and long straight reaches are the result.

Under these circumstances the largest vessels can ascend the river nearly to the foot of the Andes, but the constantly changing sandbanks at the mouth of the Amazon proper make this approach of the river dangerous, and the State of Pará is for obvious reasons not over anxious to have the deep channels properly buoyed and surveyed. This forces all the shipping to enter the Pará river, and to pass the narrows if the Amazon is the goal of the journey. In doing the latter the choice for large ships lies between one of the channels (called furos) with a bar, where it joins the Tajipuru, and a furo, the Macajubim, which has plenty of water, but which winds about in such a serpentine fashion that only ships with twin screws can pass it unassisted. These difficulties are, however, much diminished during the rainy season, when the river rises to such an extent as to drive all the inhabitants of its banks into the towns, which have been built wherever a natural eminence secures the inhabitant against the flood. Near the mouth the difference is naturally not so great as higher up, where the influence of the tide is felt less; but at Manaos the difference in level between low river and high river exceeds 40 feet.

With all rivers carrying sediment, the Amazon shares the peculiarity that its immediate banks are higher than the country lying behind them, and thus we have in the rainy season the spectacle of the main river flowing between two banks covered with dense forest, and immense lakes stretching out on either side of these banks. These do not entirely dry up during the remainder of the year, so that the whole of the Amazon valley really forms a huge swamp covered with a most luxuriant forest which, below Manaos, narrows to a broad belt close to the main river, with prairies, called campos,

at the back of the forest stretching out to the hills, where the forest recommences.

In such a country no land communication of any sort can be attempted, as the tropical vegetation and the annual inundations of the rivers destroy everything that man places in the way of the natural forces. By water, on the other hand, the intercourse between all habitable parts of the country is easy and expeditious, since steamers have been introduced in the year 1853. At that time the journey from Belem to Manaos was shortened from forty days to eight days, and at present the ocean-going steamers, which do not call at the intermediate places, accomplish the distance in three days. Belem lies on a branch of the Pará river called Guajará, which unfortunately does not share the characteristic shape of the Amazon and the furos, but forms a rather shallow basin in front of the town. The clothing of a good many inhabitants seems better adapted to a colder climate; it is only the airy costume of the ladies, and still more the absence of any costume on the children, that betrays the tropical climate. The harbour of Pará is very full of shipping, and the general build of the steamers is well adapted to navigate the broad waterway of the main river, as well as the smaller and shallower affluents, which become more and more inhabited from year to year. A number of these steamers, from a small tug, such as accompanied the cable steamer, to the ocean-going vessels, were photographed from time to time, and the views taken show at the same time something of the general features of the landscape.

As the cable steamer could not approach close enough to Pará, the shore ends were laid with the help of a barge and a tug, without anything occurring that need be mentioned. By the same means the sections from Para to Pinheiro and from there to Mosqueiro were laid, the large steamer laying the section to Souré across the Pará river. These three places are much resorted to by the inhabitants of Pará for their healthy situation, and because they imagine that salt water reaches at least Souré. The forest encircles all the houses, but the proximity of the sea, and the breeze blowing regularly every afternoon, make all these places extremely comfortable. At Souré the ss. "Faraday" was anchored at a convenient distance from the shore, so that the shore end might be landed direct from the ship, and as long as the tide was rising this plan appeared excellent. By the receding tide, however, a whirlpool was formed with the ship lying right across the centre, and when it had been turned seventeen times in one hour the captain was tired of it, and moved the ship to a safer anchorage.

Another branch of the cable was laid from Pará to Cametá on the River Tocantins, which is 1200 miles long, but unfortunately has some rapids not far from Cametá, which cut off the navigable upper portion of the river from direct communication with the general Amazon system. Cametá boasts of a fine old church and a number

of two-storied buildings, indicating the prosperous state of the township.

The first station on the main cable is Breves, the centre of the rubber trade of the islands of the lower Amazon, situate in the centre of "the narrows." Between Pará and Breves is only one shallow passage, near the lighthouse of Gujabal, and the pilot managed to run the ship aground there; luckily it was low tide, and with the rising tide the ship could be turned. At Breves the ship was anchored close to the shore, and its stern secured to a tree by a rope so that the tide could not cause it to swing. Under these circumstances the landing of the shore ends was an easy matter and soon finished. The ship then resumed its way into the narrow furos described above, and night did not put a stop to its progress, as the outlines of the forest were clearly visible against the sky, and the water everywhere more than seven fathoms deep. While the speed of the ship was kept at about six knots, the pilot ordered the quarter-master to put the helm a-starboard, as he wished to increase the distance between ship and shore. The quarter-master was, however, confused, and put the helm hard a-port, with the result that the bows went into the forest until the branches of the trees touched the fore-yard. To appreciate the situation it should be mentioned that the foremast stands 74 feet abaft the bows, and that the foreyard is 69 feet above the water level. Luckily the soft ground, the elasticity of the forest trees, and the steepness of the banks, rendered this accident quite harmless, and on reversing the engines the ship at once came off, so that the laying could be resumed. Not far from this spot the Aturia furo branches off, through which the cable had to be laid, but which was impassable for the ss. "Faraday" on account of a two-fathom bar at the Tajipuru end of the furo.

As a splice had to be made with some cable on a barge, from which it was to be paid out through the Aturia furo, the "Faraday" had to be anchored, and the right-hand shore was approached so as to leave room for the ship to swing round when the tide turned. At the critical moment, when the anchor was to be lowered, somebody blundered, and turned out the electric light, leaving the anchor winch and its surroundings in darkness. By the time this mistake had been rectified the ship was dangerously near the shore, and even the anchor could not sufficiently check its advance, so that it again ran ashore, stopping within about five feet of a house, much to the alarm of the inhabitants. This manœuvre fixed the ship in a most convenient position, so that it was left there until the splice had been finished, and the tug "Cochrane," with the barge, had started laying the cable in the Aturia furo. Again there was no difficulty in backing the ship off the bank, but it had to proceed for twelve miles stern foremost before the furo was sufficiently wide to allow the ship to turn and go on to Breves, or rather a few miles beyond, to the mouth of the Boiassu, in order to enter the Furo Grande and the Tajipuru in a roundabout

way. As the ship was drawing over twenty-four feet, and the bar at the end of the Boiassu had only twenty-three feet of water at high tide, the result was easily foreseen, but the ship remained on the bar for nine days, by which time sufficient cable had been transferred to the barge and to the ss. "Malvern" to enable the ship to continue her journey. During this enforced sojourn in the midst of the most wonderful combination of islands and rivers, the two naturalists whom the British Museum authorities had kindly sent with the expedition, took full advantage of the opportunity to explore the locality in all directions.*

Unfortunately the time is too short to give many details of the intermediate stations, but their general aspect is very similar, and nothing noteworthy occurred at most of them. Commencing at the mouth of the river, the first station is Chaves and the second Macapá; to these two places a branch is laid from Gurupá. The ss. "Faraday" had the distinction of being the first European steamer which has navigated the Amazon river below the mouth of the Tajipuru; in fact neither the pilots nor the inhabitants knew of any foreign ship that had ever touched at these ports. In Gurupá, the second station of the main line, the inhabitants expressed their joy at being put in communication with the rest of the world, by actively helping in the landing of the first shore end. A young lady in white, niece of the mayor, borrowed a handkerchief from one of our engineers, daintily laid hold of the end of the cable and triumphantly carried it into the station. Here a ball was started, and the happy couples waltzed round the cable end to show their appreciation. Meanwhile the tug began pulling on the barge from which the cable was to be paid out, and just as these vessels began to feel the current, which runs rather strong there, something jambed, the cable would not run out, and the tug could not hold the barge against the current. Barge, tug and cable drifted down stream, the end gradually disappearing out of the station. This contretemps luckily did not disturb the dancers, who continued their rejoicings until the end had been brought back.

Monte Alegre lies on a furo which unfortunately has a shallow bar at its mouth, so that the cable had to be laid in and out by the barge and tug. This furo swarmed with "botos," a species of dolphin much coveted by the naturalists; but the natives do not try to catch them because they are neither good for food nor useful in other ways, besides they are remarkably shy and strong. From thence the cable is laid to Santarem at the mouth of the Tapajos, which presents a strong contrast to the Amazon on account of its clear waters and tranquil flow. This river is 1200 miles long, and is formed by the union of the Arinos and Juruena, rising in 14° 42′ S. lat., and 60° 43′ W. long., in the so-called "aguas vertentes" (the

* In the library were exhibited the specimens collected by the naturalists and other members of the expedition.

turning waters) close to the sources of some of the affluents of the Paraguay river. In the rainy season all these waters mix, and it is possible to pass in a boat from the mouth of the Rio de la Plata in 35° S. lat. to the mouth of the Orinoco in 10° N. lat., by way of the Paraguay, the Tapajos, the Amazon, the Rio Negro and the Cassequiare, which forms a connecting link between the Amazon system and the Orinoco.

From Santarem a branch cable is laid to Alemquer, and Obidos, the next station on the main line, is the last point touched in the State of Pará. It would not be right to leave unnoticed the rubber-gathering industry, which is at once the wealth and the bane of this part of the world. The implements in use are of the most primitive kind, as may be judged by the samples on the table, but the average earnings can easily be three pounds per day during the dry season, and the facility of earning so much money with little exertion makes the inhabitants unwilling to engage in more arduous labour. A narrow path leads from the hut on the water's edge into the forest, from one rubber tree to another, the path eventually returning to the hut. The trees are cut on the morning round and the rubber is gathered in the afternoon. As soon as it arrives at the hut, a fire of oily palm nuts (Attalea Excelsa) is lighted and the thin sap thickened in the smoke. For this purpose a paddle is used, on to which the sap is poured with a small earthenware or tin vessel. The smoke soon thickens it and a new layer is poured on, until the well-known flat cakes of india-rubber have been formed. Owing to the rise of the river during the rainy season, most of the huts have to be abandoned, and it can easily be imagined how comfortless they are. Nearly all of them are built on piles, and most of them are thatched with palm leaves. There is hardly any attempt made to cultivate the soil, such as it is, but everything is imported. The ss. "Cametense" in which the surveying party went out, was laden with cabbages, onions and potatoes, part of which went as far as Iquitos in Peru. Chiefly owing to this want of provisions, and to the generally careless mode of life, the mortality among india-rubber gatherers is very great. There are two stations in the State of Amazonas—Parintins, formerly called Villa Bella da Imperatriz, and Itacoatiara, formerly Serpa. Just before reaching the former station the Serra de Parintins is passed, which forms the boundary between the two States. At Parintins the river makes a sudden bend, and the resulting eddy current greatly impeded the work; at Itacoatiara, on the other hand, the bow of the ship was run ashore, and the end of the cable landed direct from the ship.

Before showing views of Manaos three pictures of the vegetation taken at a short range will be thrown on the screen to illustrate the luxuriance met with everywhere on the journey, but no attempt will be made to describe it, as that has been done to perfection in the classical works of Bates and Wallace. Everything they have said in this respect remains as true as it was forty years ago, and hardly

anything new can be added to their description of the general features of the Amazon valley; but the town of Manaos has completely changed its character since it was made the capital of that region in 1853. A town quite European in its features has arisen in the midst of the forest, and to the benefits of rapid transport—to which it has owed so much—there is now added the characteristic lever of modern progress, the annihilator of space and time—electrical communication.

[A. S.]

WEEKLY EVENING MEETING,

Friday, May 22, 1896.

George Matthey, Esq. F.R.S. Vice-President, in the Chair.

Professor J. A. Ewing, M.A. F.R.S. Professor of Mechanism and Applied Mechanics in the University of Cambridge.

Hysteresis.

(Abstract.)

The lecturer explained that the word hysteresis was not a term in neuro-pathology. It had nothing to do with hysterics. The name might be unfamiliar, but the thing it described was exceedingly common. It was scarcely too much to say that hysteresis was to be found everywhere, except, perhaps, in the dictionary.

The word was derived from the verb ὑστερέω, which signified to lag behind. It was introduced about fourteen years ago to name a characteristic which had been prominent in several researches into the physical qualities of certain materials, especially of iron. The name was invented at a time when the phenomenon of hysteresis had no more than a purely scientific interest; but in the rapid advance of industrial electricity hysteresis had become a matter of much commercial importance, and the word was now in common use by electrical engineers. Certain materials, when causes acted on them tending to change their physical state, had a tendency to persist in their previous state. This tendency to persist was what constituted hysteresis.

It was in connection with the magnetic properties of iron and steel that the most conspicuous and practically the most important manifestations of hysteresis were found. An experiment was shown to illustrate hysteresis in the changes of magnetic condition brought about by the application and removal of stress. An iron wire, magnetised by a constant current in a surrounding coil, was hung up and loaded with weights. The weights were alternately removed and reapplied, and the magnetic state of the wire was shown by means of a mirror magnetometer. It was seen that when the weights were repeatedly put on and off, the magnetism changed from one to another of two values; but when half the weight only was left on during unloading, the magnetism assumed a value much nearer to the loaded than to the unloaded state; whereas when half the weight was put on after unloading, the magnetism took a value nearer the unloaded than the loaded state. In other words, the magnetic effects of the loading lagged behind the changes in the loading itself. This

lagging was shown to be static in character, for it was in no way dependent on the rate at which the process of loading and unloading was performed. Other cases of static hysteresis in the thermoelectric and mechanical qualities of iron were mentioned.

Practically the most important instance was the hysteresis which was observed when a piece of iron had its magnetism changed by changing the magnetising force. When a piece of iron was first magnetised, the magnetism B was developed by gradual increase of the magnetising force H, in the way shown in Fig. 1. If at any stage in the process, such as *a*, the magnetising force was made to stop increasing, was reduced to zero, and was then reapplied in the opposite direction, the magnetism changed in the way shown by the curve *a c d* of Fig. 2. And finally, if the magnetising force were again reversed, so as to recover the direction and value it had at *a*, the process followed was represented by the curve *d e a* of Fig. 3.

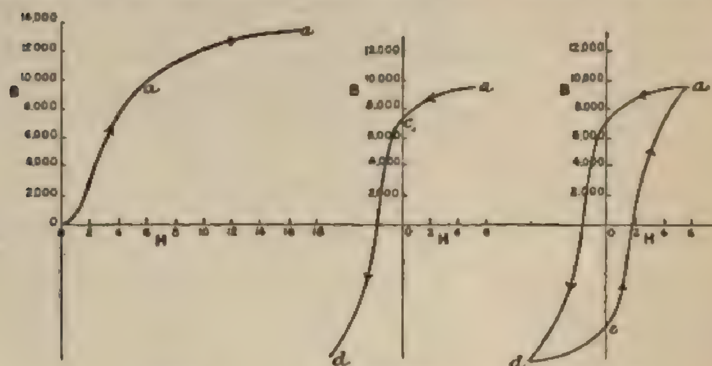


FIG. 1. FIG. 2. FIG. 3.

The closed curve of Fig. 3 showed how the changes of magnetism in this complete cycle of double reversal tended to lag behind the changes of magnetising force. In consequence of this hysteresis, energy was consumed in reversing the magnetism of iron, and it could be proved that the energy consumed in each double reversal was proportional to the area enclosed between the curves *a c d* and *d e a*.

This was the process that went on in the iron cores of transformers when used for electric lighting in the alternate-current system of supply. The magnetic cycle was gone through something like 100 times a second, and as a rule the transformer was in circuit continuously by day and night. Whether it was supplying lamps and doing useful service, or whether it was not, the waste of power due to hysteresis went on. It formed a very serious item in the cost of alternate-current supply, for the effect was that a large part of

the coal burnt at the central station, after having its energy passed through a series of costly conversions, was devoted in the end to nothing more than uselessly warming the transformers in the cellars of consumers or in boxes under the streets. So long as iron could not be found that was destitute of magnetic hysteresis, some loss on this account was inevitable; but it might be greatly lessened by choosing a suitable kind of iron. Experience showed that some kinds of iron had much less hysteresis than others. Thus in Fig. 4 the curve marked I related to a specimen of iron eminently suitable for use in transformers, while the curves marked II and III related to other brands of iron. They enclosed much larger areas, and showed

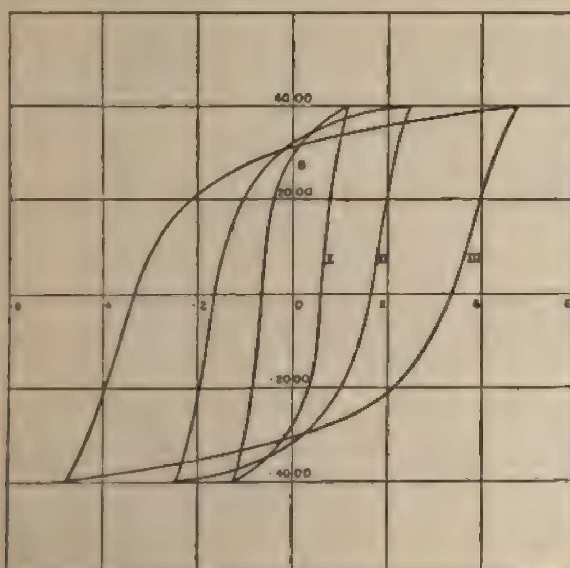


FIG. 4.

that the iron which gave them was to be avoided as having too much hysteresis. Of late years some of the makers of iron had striven with marked success to produce iron which should be comparatively free from hysteresis, and it was now possible to obtain material for transformers which reduced the loss to a fraction of what was formerly thought inevitable.

The lecturer's magnetic curve tracer was exhibited in action, showing magnetic curves, similar to Fig. 3, upon a screen by giving to a small mirror simultaneous horizontal and vertical movements, the former proportional to the magnetising force, and the latter to the magnetisation of the specimen of iron in the machine. As a

convenient means of practically testing the quality of iron in this respect the lecturer had lately introduced another instrument, which was also shown at work. In this hysteresis tester (Fig. 5) the sample of iron, in the form of a bundle of thin strips, was clamped in a carrier and caused to rotate between the poles of a magnet swinging on knife edges. As a consequence of hysteresis this magnet was deflected, and its deflection, which was noted by means of a pointer and scale, served to measure the hysteresis.

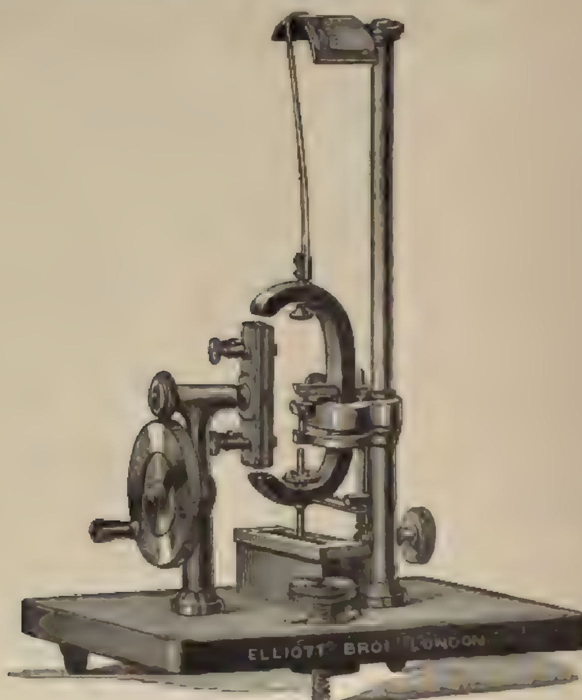


Fig. 5.

To show directly the heating effect of magnetic reversals in iron, a differential air thermometer was used, with long bulbs, one of which was partly filled by a bundle of iron wire. Both bulbs were surrounded by coils, through which an alternating current passed. The heating effect of the current itself was the same for both, but the bulb containing the iron was further heated in consequence of the hysteresis of the metal, and this additional heating was shown by movement of a liquid index in a tube connecting the two bulbs. It had even been proposed to apply the heating effect of hysteresis

to the boiling of water. A kettle invented for this purpose by Sir David Salomons and Mr. Pyke was exhibited.

In another experiment to illustrate the dissipation of energy through magnetic hysteresis a steel ball was caused to roll down an inclined railway formed by a slot cut in an iron tube. The tube was wound longitudinally with a magnetising coil which caused lines of magnetic induction to cross the slot. The ball was consequently magnetised, and as it rolled the changes of magnetism in it and in the neighbouring parts of the tube checked its motion, causing it to slow down or stop when the current in the magnetising wire was applied; but the resistance due to hysteresis ceased when the current was broken.

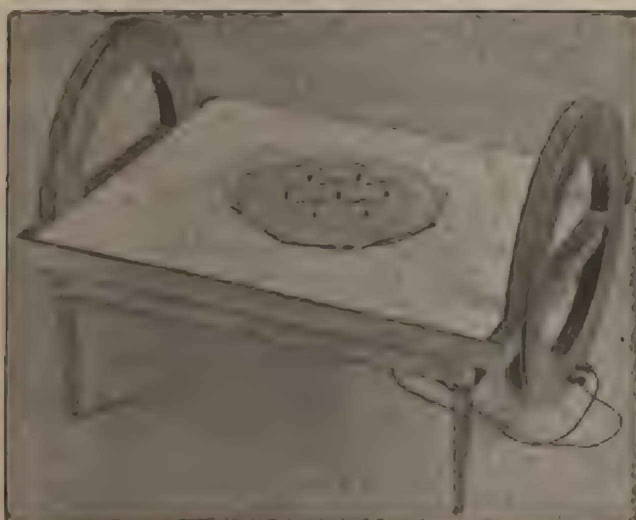


Fig. 6.

In conclusion the lecturer referred to the molecular theory of magnetisation, which he had explained in a former lecture,* and to the explanation it gave of magnetic hysteresis. Since then it had received a remarkable confirmation from the work of Mr. F. G. Baily, who had measured the hysteresis when iron discs were made to revolve in a strong magnetic field. He found that when the field was strengthened the hysteresis was at first increased, but a stage was reached when the strengthening of the field ceased to increase the hysteresis, and with a stronger field still the hysteresis was

* 'Proceedings,' Royal Institution, May 22, 1891.

actually reduced. Indeed, by a small further increase of the field the hysteresis could be made to practically vanish. This very curious result had been predicted originally by Mr. James Swinburne, as a consequence of the lecturer's theory, and had at that time seemed so unlikely that it was urged as an objection to the theory. It had now been proved to afford the theory the strongest possible confirmation.

A model was shown in illustration of this point, in which a glass plate carrying a number of small pivoted magnets (Fig. 6) was made to revolve slowly in a magnetic field produced by two neighbouring coils. So long as the field was weak the small magnets formed groups which were broken up during the revolution, thereby dissipating energy and exhibiting hysteresis; but when the field was sufficiently strengthened the small magnets continued to point one way without forming groups, for their mutual magnetic forces were then masked by the external or field force. There were consequently then no unstable phases in the motion and no hysteresis.

Hysteresis in the magnetic quality of iron was to be ascribed to the formation of stable groups of molecules, in consequence of the mutual forces which the molecules exerted on one another in virtue of their magnetic polarity. It might very well be that in other manifestations of hysteresis, such, for example, as the familiar phenomenon of friction between two solid surfaces when rubbing against one another, the resistance and consequent dissipation of energy were similarly due to the forming and breaking up of molecular groups, the molecules being mutually constrained by some other species of polar forces, possibly due to electrostatic charges upon them.

[J. A. E.]

WEEKLY EVENING MEETING,

Friday, May 29, 1896.

THE RIGHT HON. LORD HALSBURY, M.A. D.C.L. F.R.S. Manager, in the Chair.

AUGUSTINE BIRRELL, Esq. Q.C. M.P.

John Wesley: Some Aspects of the Eighteenth Century.

(Abstract.)

THE lecturer said that when he thought of the eighteenth century as it was lived in England in town and country, he found it difficult to reconcile all that he read about it with any sweeping description, condemnation or dominant note. It was a century of violent contrasts. It was a brutal age, for the press-gang, the whipping-post, gaol fever, all the horrors of the criminal code were among its characteristics. It was an ignorant age, for a great part of the population gave itself up to drunkenness and cock-fighting; a corrupt age, when offices were bought and sold and every man was supposed to have his price. Brutal, ignorant and corrupt, the eighteenth century was all these—was it not written in the storied page of Hogarth? And yet, too, there was plenty of evidence of enthusiasm, learning and probity. The life of John Wesley, who was born in 1703 and died in 1791, covered practically the whole of the eighteenth century, of which he was one of the most remarkable and strenuous figures, and his Journal was the most amazing record of human exertion ever penned by man. Those who had ever contested a Parliamentary election would know how exhausting was the experience; yet John Wesley contested the three kingdoms in the cause of Christ, and during that contest, which lasted forty-four years, he paid more turnpike toll than any man who ever lived. His usual record of travel was 8000 miles a year, and even when he was an old man it seldom fell below 5000 miles. The number of sermons he preached had been estimated at 40,500. Throughout it all he never knew what was meant by depression of spirits. Wesley was not popular with historians; he put the historian out of conceit with himself. It might be said that Wesley's personal character lacked charm, but it was not easy to define charm; nobody ever had defined it, and nobody who was wise ever would try to do so. But, charm or no charm, Wesley was a great bit of the eighteenth century, and was therefore a great revealing record of the century. He received a good classical education, and remained all his life very much of the scholar and the

gentleman. He was a man of very wide reading, and his judgments on books were not only "polite" but eminently sane and shrewd. His religious opinions, and his extraordinary credulity in some matters, in no way affected the perfect sanity of his behaviour or the soundness of his judgment. He was a cool, level-headed man, and had he devoted his talents to any other pursuit than that of spreading religion he must have acquired a large fortune. He knew that he would have succeeded in other walks of life, but from the first day of his life almost he learnt to regard religion as his business. In his Journal he never exaggerated, or never seemed to do so; the England he described was an England full of theology and all sorts of queer vague points, and strange subjects were discussed in all places—of some of them the very phraseology was now as extinct as the wolf, or at least as rare as the badger. Although not over well disposed, as his life went on, towards the clergy of the Establishment, he very seldom recorded any incidents of gross clerical misbehaviour. In spite of the rudeness of the manners of the people, Wesley's sufferings were really nothing to those with which Parliamentary candidates had had to put up for centuries. What would really shock the reader of his Journal was his description of what might be called the public side of the country—the state of its gaols or its criminal code, the callous indifference of the magistracy, the indifference of the clergy to what might be called missionary effort. Wesley's Journal was a book which ought to be kept in mind as a means of knowledge of the eighteenth century, just as much as 'Tom Jones' was a means of knowledge or as Hogarth was. As one read his Journal one was constrained to admire the magnificence of the vigour, the tremendous force of the devotion and the faith which kept John Wesley in perpetual motion for more than half a century, and one felt glad to be able to place that Journal beside Walpole's letters and Boswell's Johnson, and to know that in it there were some aspects of the eighteenth century that could not be found elsewhere.

[A. B.]

GENERAL MONTHLY MEETING,

Monday, June 1, 1896.

SIR JAMES CRICHTON-BROWNE, M.D. LL.D. F.R.S. Treasurer and Vice-President, in the Chair.

William Phipson Beale, Esq. Q.C. F.G.S.
Miss Esther Bright,
Edward Ball Knobel, Esq. Treas. R.A.S.

were elected Members of the Royal Institution.

The following Address to the Right. Hon. Lord Kelvin was read and adopted, and authorised to be signed by the President on behalf of the Members:—

"To the Right Hon. Lord Kelvin, D.C.L. LL.D. F.R.S. F.R.S.E. Grand Officer of the Legion of Honour, Professor of Natural Philosophy, University of Glasgow, Manager and Vice-President, Royal Institution of Great Britain.

"The Members of the Royal Institution of Great Britain beg leave to offer to your Lordship their cordial congratulations on the occasion of the Jubilee of your appointment to the Chair of Natural Philosophy in the University of Glasgow, and desire to express their high appreciation of the conspicuous services you have rendered during your incumbency of that chair in the Extension and Diffusion of Scientific Knowledge, which it is the main object of the Royal Institution to promote.

"Recognising as the Members of the Royal Institution do the incalculable and far-reaching value of your researches and labours in connection with electricity, magnetism, the atmosphere, heat and vortex motion, and the immediate practical utility of your ingenious inventions, in aiding further scientific investigation and in enlarging and quickening human intercourse, they wish more especially to acknowledge the benefits you have conferred on the Royal Institution by the admirable lectures which you have, from time to time, delivered within its walls. Your first lecture, "On the Origin and Transformations of Motive Power," was given on the 29th of February, 1856, when the late Sir Henry Holland occupied the Chair: and your last lecture, on "Isoperimetrical Problems," was given on May 12th, 1893, when the chair was filled by Sir Douglas Galton, K.C.B.

"In the thirty-seven years intervening between these dates—a period of intense and fruitful scientific activity—you have addressed the Members of the Royal Institution fifteen times, your lectures having been as Mirrors and Recorders in reflecting and measuring the advances achieved in mathematics and physics.

"The Members of the Royal Institution rejoice to think that besides contributing more than any man now living to the progress of Science, you have likewise secured it a higher place in public estimation than it has hitherto attained, and they earnestly hope that you will be long spared to wear the honours which have been so deservedly conferred upon you."

It was Resolved, That Sir Frederick Bramwell, Bart. and Professor Dewar be appointed delegates from the Royal Institution to present this Address.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

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Proceedings, Nos. 154, 155. 8vo. 1896.
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Analyst for May, 1896. 8vo.
Anthony's Photographic Bulletin for May, 1896. 8vo.
Astrophysical Journal for May, 1896. 8vo.
Athenæum for May, 1896. 4to.
Author for May, 1896.
Bimetallist for May, 1896.
Brewers' Journal for May, 1896. 8vo.
Chemical News for May, 1896. 4to.
Chemist and Druggist for May, 1896. 8vo.
Education for May, 1896. 8vo.
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...ineer for May, 1896. fol.
...eering for May, 1896. fol.
...ering Review and Metal Worker for May, 1896. 8vo.
...ustry Review for May, 1896.
...al Journal for May, 1896. 8vo.
...and Iron for May, 1896. fol.
... May, 1896. 8vo.
...for May, 1896. 8vo.
...cket for May, 1896. 8vo.
... 1896. 4to.

Editors—continued.
Nuovo Cimento for April, 1896. 8vo.
Physical Review for May–June, 1896. 8vo.
Science Siftings for May, 1896. 8vo.
Scientific African for May, 1896. 8vo.
Scots Magazine for May, 1896. 8vo.
Technical World for May, 1896. 8vo.
Transport for May, 1896. fol.
Tropical Agriculturist for April, 1896. 8vo.
Work for May, 1896. 8vo.
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WEEKLY EVENING MEETING,

Friday, June 5, 1896.

THE RIGHT HON. LORD KELVIN, D.C.L. LL.D. F.R.S. Vice-President, in the Chair.

PROFESSOR J. A. FLEMING, M.A. D.Sc. F.R.S. *M.R.I.*

Electric and Magnetic Research at Low Temperatures.

DURING the last four years much time has been spent by Professor Dewar and by me in the prosecution of a joint research on the principal electric and magnetic properties of metals at very low temperatures. Some reference has already been made in previous discourses by Professor Dewar to portions of this work,* but the special object of the present lecture is to extend these descriptions, and put you in possession of the latest results in this department of the low temperature investigations. It will be convenient to discuss the several divisions of it in the order in which they have engaged our attention.

One hundred and sixty-seven years ago Stephen Gray, a pensioner of the Charterhouse, in conjunction with his friend Granvile Wheler, stretched a packthread 300 feet long over silk supports, and demonstrated that an electrification of the thread at one end spread instantly over the whole mass, but that if metal wires replaced the silk no electrification of the thread was possible. This experiment undoubtedly formed the starting-point for the first definite recognition of the necessity for a classification of bodies into insulators and conductors, a distinction which Gray's brilliant contemporary, Dufay, extended and confirmed, and for which he and Desaguiliers coined these familiar terms.† Gray's contributions to knowledge as an epoch-making discoverer have received less notice from scientific historians than their real value deserves. It is less easy to state who first noticed that the powers of conduction and insulation were greatly affected by temperature. Cavendish, in 1776, however, was perfectly familiar with the fact that solutions of common salt conduct electricity better when warm than when cold,‡ and made measurements of the relative

* 'Scientific Uses of Liquid Air.' A Friday Evening Discourse, by Professor J. Dewar, LL.D. F.R.S. delivered at the Royal Institution, Jan 19, 1894.

† See the 'Intellectual Rise in Electricity,' by Park Benjamin. Stephen Gray's papers on this subject, communicated to the Royal Society, are as follows: Phil. Trans. 1720. vol. xxxi. p. 104; 1731, vol. xxxvii. p. 18; 1732, vol. xxxvii. p. 285; 1735, vol. xxxix. p. 16; 1736, vol. xxxix. p. 400. See also Dufay, Phil. Trans. 1733, No. 431, p. 258.

‡ See the 'Electrical Researches of Cavendish.' Edited by Clerk-Maxwell, p. 324.

resistances of an iron wire and a salt solution which were marvellously accurate, when we consider that his only means of measurement was the comparison of electric shocks taken through the bodies to be examined.

Not until after the invention of the battery and galvanometer was it clearly proved that differences exist between the conducting powers of metals; but by Davy, Becquerel, Ohm, Pouillet, Fechner and others all the fundamental facts were ascertained, and the classical researches of Wheatstone and later of Matthiessen gave us the accurate laws and constants of electrical conduction. By these investigations it was shown that in the case of electric conduction through metallic wires of uniform sectional area their total resistance was proportional to the length, inversely as the cross section, and also proportional to a specific constant for each material called its *resistivity*. Moreover, it was found that this resistivity was considerably affected by temperature, generally being increased in metals by rise of temperature, and decreased for carbon, electrolytic liquids and many badly conducting bodies.

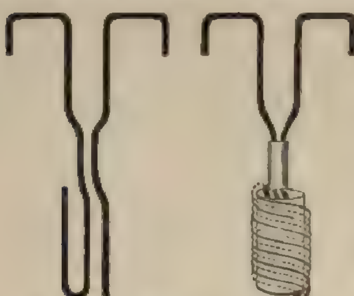

FIG. 1.

Low temperature resistance coil.

Although much knowledge of the behaviour of pure metals and alloys in regard to electric conduction has thus been accumulated, we considered that it would be of great scientific interest to examine with care the changes occurring in the conductivity of these bodies, or reciprocally in their resistivity, when cooled to temperatures of two hundred degrees or more below the Centigrade zero by the aid of liquid oxygen and liquid air.* Knowing the great influence of very small quantities of impurity on this quality, our first attention was directed to obtaining samples of alloys and metals in a state of great chemical purity, in giving to wires drawn from them a suitable form, and in devising a convenient support or holder by which the electrical resistance of the wire might be measured when immersed in liquid oxygen or liquid air, either in quiet ebullition in an open vessel, or under reduced pressure in a closed one. It will be unnecessary to dwell on the difficulties surrounding the preparation of these accurately drawn metallic wires of pure metal. Suffice it to say that our obligations to Mr. George Matthey, Mr. Edward Matthey, Mr. J. W. Swan and other friends were very great with respect to

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* Almost the only experimental work previously done in this subject seems to have been that of Cailletet and Bouty ('Journal de Physique,' July 1885), on the 'Resistance of Metals at − 100° C.,' using ethylene as a refrigerating agent; and a research by Wroblewski, on the 'Resistance of Copper at very Low Temperatures' ('Comptes Rendus,' 1885, vol. ci, p. 161).

this portion of the work. The final outcome of all failures was the production of a resistance coil of the following form:—Two thick wires of high conductivity copper about 3 or 4 mm. thick are bent as shown in Fig. 1, and wrapped round the lower part with a cylindrical sheath of thin vulcanised fibre laced to them by a silk thread. On this sheath, which generally had the form of an oval cylinder, a paraffined silk cord was spirally wound so as to leave a helical groove. In this groove was coiled the resistance wire, of known length and section, and its ends were attached by solder to the ends of the thick copper leads. The wire was wound a little loosely in the groove so as to allow for the great contraction which takes place in cooling, and yet the wire was exposed so as to take up instantly the temperature of the bath, whilst at the same time the mass of material to be cooled down was rendered as small as possible. The length of wire employed was generally about one or two metres, and the diameter from about one-twelfth to half a millimetre (·003 inch to ·02 inch). These mean diameters were measured by the microscope micrometer at about fifty to one hundred places for each metre length of the wire. Having thus prepared a great collection of resistance coils of pure metals and alloys, each in the form of a wire of known length and mean diameter, the next operation was the measurement of their resistance at definite temperatures. For the sake of those not fully familiar with the details of electrical measurement, a moment's digression may be made to explain two of the principal methods in use. Becquerel's work was chiefly conducted with the differential galvanometer. In this instrument two coils of wire of exactly equal length are coiled on one bobbin, in the centre of which hangs a small magnetic needle. The current from a battery (see Fig. 2) divides at one point, and flows along one path through the conductor or conductors under examination and through one coil (No. 2) of the galvanometer. The other portion of the current flows through a wire of variable length called a rheostat, and through the other coil of the galvanometer, equal in every respect to the first coil, but circulates round the needle, N.S., in an opposite direction to that of the current in the first coil. Hence, if the currents are of equal strength the needle is not disturbed at all from its zero position. We can make these currents equal by adjusting the length of the wire of the rheostat so that its resistance is equal to the resistance of the coil

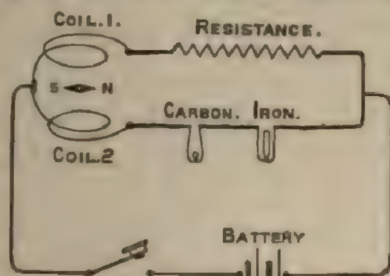

FIG. 2.

Diagram of arrangement of circuits for comparing resistances by means of the differential galvanometer.

being tested. By this means it is easy to verify all the ordinary laws of conduction. We can, for instance, show at once that by cooling an iron wire in iced water its resistance is decreased, whereas in cooling the carbon filament of a glow-lamp its resistance is increased.

This method is not generally so convenient as the arrangement first described by Mr. Hunter Christie to the Royal Society in 1833, re-devised ten years later by Wheatstone in 1843, and which has been always curiously misnamed the "Wheatstone's Bridge," even in spite of Wheatstone's own declaration that he did not invent it.* In this arrangement (see Fig. 3) the current from a battery B has two paths open to it by which to complete its circuit, and we employ a galvanometer with a single coil to discover two points on these two circuits which are at equal potentials. When these two points are connected the galvanometer needle is undisturbed, and it is a simple matter to show that under these circumstances the numerical values of the electrical resistances of the two segments A X, X D, of the circuit A D, denoted by P and Q, and the resistances R and S which form the

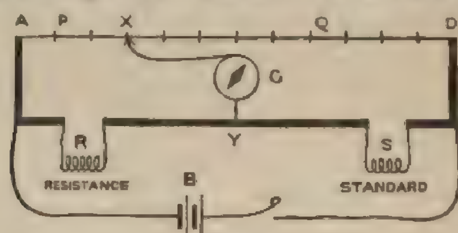

Fig. 3.

Wheatstone's Bridge arrangement for comparing resistances.

other branch, are to one another in simple proportion as R is to S—that is, P is to Q as R is to S. In actual work, one form, useful for lecture purposes, which this arrangement takes is that known as the slide wire bridge (see Fig. 4), and which is before you. In this construction the battery current flows partly through a uniform wire *a b*, stretched over a scale, and partly through a standard resistance R′, and the resistance R to be tested placed in series with it.

We employ a galvanometer G to connect the middle point between R and R′ with some point on the slide wire, and we can always find a point on the slide wire such that no current flows through the galvanometer. The ratio of the unknown resistance R is to that of the known standard resistance R′ in the ratio of the lengths of the two sections into which the contact piece divides the slide wire. Hence R is determined in terms of R′. Another form of this appliance in which all three arms of the bridge consist of coils of wire capable of

* See Phil. Trans. 1833, Mr. S. Hunter Christie, on the 'Experimental Determination of the Laws of Magneto-Electric Induction.' See also Wheatstone's Scientific Papers, p. 129, 'An Account of several new instruments for determining the Constant of a Voltaic Circuit,' Phil. Trans. vol. cxxxiii. p. 303, 1843.

being joined, as required, in series with each other by plugs, is most commonly used, and it was a most carefully adjusted Elliott bridge of this last pattern which we employed.

All our resistance measurements have been reduced to express them in terms of the International ohm, as defined by the Board of Trade Committee, and obtained by reference to standard coils carefully standardised for us at Cambridge. By this means the whole of our wires were measured at five definite temperatures, viz. at about 200° C.; at the temperature of boiling water, 100° C.; at the temperature of melting ice, 0° C.; at the temperature of solid carbonic acid melting in ether, which gives a temperature of about −78° C.; and at the temperature of liquid oxygen boiling under a pressure of 760 mm., which gives a temperature of −182° C.

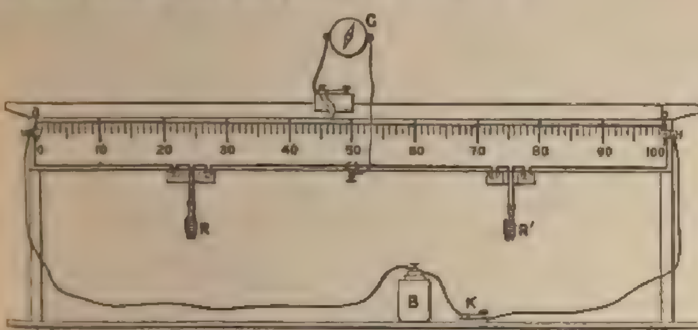

Fig. 4.

Slide wire bridge. Lecture form.

In this last case the coils were immersed in liquid oxygen contained in suitable vacuum-jacketed vessels. In this connection, I should like to express with due emphasis the opinion that none of this low temperature research would have been possible at all without the assistance of Professor Dewar's most valuable invention the glass vacuum-jacketed silvered vessel. For much of this work it has been necessary to employ many litres of liquid oxygen and air at a time, and to be able to keep it for hours in a state of perfect quiescence and absolutely constant temperature, and in no way could this have been done without this beautiful and scientific device.

Before describing the results of these experiments it may be interesting to exhibit a few of the principal facts. The most striking of them is the very great reduction in electrical resistance, or increase in conductivity, experienced by all the pure metals when cooled in liquid air. Here, for instance, are two coils of iron wire: balancing them on the bridge we find them to be of exactly equal resistance, but if one of the coils is cooled in liquid air its resistance is reduced to about one-tenth of its resistance at the ordinary tem-

perature of the air. We may also compare the resistances of these two similar iron coils, when one is placed in boiling liquid air and the other in boiling water. The resistances, instead of being in the ratio of one to one, are now in the ratio of one to twelve. Again, if we take two wires, one of pure iron and one of pure copper, of exactly equal length and equal section, we find that at ordinary temperatures (15° C.) the iron wire has about six times the resistance of the copper; but if we cool down the iron wire in liquid air to −186° C., still keeping the copper coil at the ordinary temperature (15° C.), we now find that the iron coil has actually become a much better conductor (about 30 per cent. better) than the copper.* On the other hand, if we examine the behaviour of this coil of German silver, which is a copper-zinc-nickel alloy, or of this platinum-silver coil, we find that the cooling down through 200° has a comparatively small effect upon its electrical resistance. We thus see that whilst pure metals have their electrical resistance immensely decreased by cooling to the temperature of liquid air, alloys generally do not experience anything like so great a change.

A word or two must next be said on the manner in which we have represented graphically all the results of our experiments. We desired to delineate lines on a chart so as to express the change in specific resistance of all our metals and alloys in terms of temperature; and the question then arises, how was the temperature measured? You already know that an ordinary thermometer, whether mercury, alcohol, or air, would be useless to measure temperatures at which even air liquefies under ordinary pressures.

The employment of the constant pressure hydrogen thermometer with reduced pressure would have given us temperature readings very approximately those of the absolute thermodynamic scale, but the experimental difficulties of its use would have been enormous. We preferred to use the platinum resistance thermometer, and to express our temperatures in platinum degrees as follows:—Our experience has shown us that a pure soft annealed platinum wire may be cooled as often as necessary to the lowest attainable temperatures, and yet will always have the same resistance when measured again at other constant temperatures. Availing ourselves of this fact, we have used in all this work a low temperature platinum thermometer made in the following way:—A well-annealed platinum wire is made into a resistance coil, as already described. Its resistance is carefully measured at the temperature of boiling water, 100° C., and melting ice, 0° C. From these measurements we construct a scale of temperature as follows:—A horizontal line A E (see Fig. 5) is taken on which to mark off temperature, and any two points A and B are taken on this line and the length A B divided into one hundred equal parts. At these points B and A perpendiculars are set up

* The exact resistances of the coils used for the experiment were as follows: Iron at 16° C. = 7·003 ohms, and reduces to 0·711 ohms; at − 186° C. copper at 16° = 1·169 ohms, reduces to 0·2033 at − 186° C.

proportional to the resistance of the platinum wire at 0° C. and at 100° respectively, and through the tops of these perpendiculars a sloping straight line is drawn until it cuts the axis of temperature at E. The graduation of the horizontal line is continued in both directions on the same scale as the subdivision of the line between the points marked 0 and 100. To measure and define any other temperature, say, for instance, the boiling-point of liquid oxygen under a pressure of 760 mm., we have simply to measure the resistance of the platinum wire in the liquid oxygen. We then look out on the chart the ordinate which has the same numerical value as the resistance of the wire in the oxygen, and at the foot of that ordinate

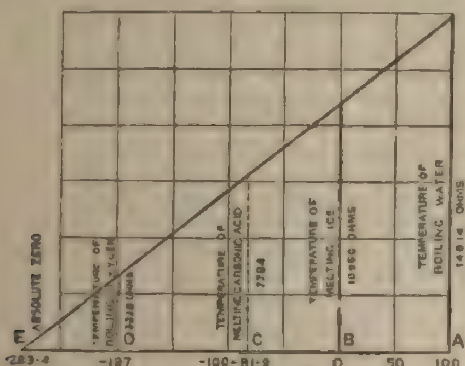

Fig. 5.

Method of constructing a scale of platinum temperature.

we find a number, viz. (−197), which is the temperature of the liquid oxygen on this platinum scale.

Two questions then arise—first, Do all annealed platinum wires give, when used in this way, the same numerical values for definite and identical temperatures? The answer to this is, Nearly, but not quite. In the case of two thermometers much used by us, the difference was about half a degree at −100° C., the boiling-point of liquid ethylene. Into this matter it is not possible here to enter more fully; suffice it to say that we have invariably referred our temperature measurements to one standard thermometer. The second question is equally important—it is, What is the relation of the scale of temperature so defined to the absolute thermodynamic scale; or, which is very nearly the same thing, to the scale of temperature defined by a constant pressure hydrogen thermometer? If the air thermometer and platinum thermometer readings are made to agree at 0° C. and 100° C., then a temperature which would be called 50° on the Centigrade scale would be denoted by 50·4 nearly on the platinum thermometer scale, and corresponding to −78° on the Centigrade scale, which is the temperature of carbonic acid melting in ether.

The platinum temperature by our standard is $-81°\cdot 9$; and corresponding to $-182°$ C., which is very nearly the Centigrade temperature of liquid oxygen boiling at the normal pressure of 760 mm.; the platinum temperature by the same standard is $-197°$. The conversion of these numbers representing low temperatures in platinum degrees into the numbers representing the corresponding absolute thermodynamic temperatures is a work we have reserved for a future research;* but meanwhile it may be said that there is no method of measuring low temperatures which is so easy of application and so accurate as that depending on the use of a platinum thermometer. All our work has been ultimately referred to one standard platinum thermometer, which we call P_1.

A suggestion may here be made. There is no reason why the Board of Trade electrical laboratory should not possess a standard platinum thermometer defining officially platinum or absolute temperatures for all time, and with which other platinum thermometers could be easily and very accurately compared.

Having thus defined our scale of temperature, we proceeded to embody the whole of our results in a chart which is now before you (see Fig. 6), and in which vertical distances represent resistivity, or specific resistance, or the resistance in absolute measure per cubic centimetre of the various metals, and horizontal distances represent platinum temperatures. The curves indicate the manner in which the resistivity varies with temperature for each substance.

The values of the resistivity of most ordinary metals and alloys are given in the table adjoining:—

Electrical Resistivity of Pure Annealed Metals.

Metal.	Resistivity in C.G.S. units at 0° C.	Percentage increment, 0° to 100° C.	Atomic volume.
Silver	1,468	40·0	10·04
Copper	1,561	42·8	7·10
Gold	2,197	37·7	10·04
Aluminium	2,665	43·5	10·56
Magnesium	4,355	38·1	13·76
Zinc	5,751	40·6	9·12
Iron	9,065	62·5	7·10
Cadmium	10,023	41·9	12·96
Palladium	10,219	35·4	9·12
Platinum	10,917	36·69	9·12
Nickel..	12,323	62·2	6·94
Tin	13,048	44·0	16·20
Thallium	17,633	39·8	17·20
Lead	20,380	41·1	18·27
Mercury	94,070	38·88	14·56
Bismuth	108,000	—	21·43

* Callendar has shown that over a wide range of temperature from 0° C. to 700° C. the difference between the platinum temperature and the air thermometer temperature is a parabolic function of the absolute temperature.

ELECTRICAL RESISTIVITY OF ALLOYS.

Alloy.	Composition.	Resistivity in C.G.S. units at 0° C.	Percentage increment, 0° C. to 100° C.
Aluminium-copper	94 : 6	2,904	38·1
Aluminium-titanium	—	3,887	29·0
Aluminium-silver	94 : 6	4,641	23·8
Gold-silver	90 : 10	6,280	12·4
Copper-aluminium	97 : 3	8,847	8·97
Copper-nickel-aluminium	87 : 6½ : 6½	14,912	6·45
Platinum-rhodium	90 : 10	21,142	14·3
Nickel-iron	95 : 5	29,452	20·1
German silver	$=Cu_3Zn_2Ni_3$	29,982	2·73
Platinum-iridium	$=Pt_4Ir$	30,896	8·22
Platinum-silver	1 : 2$=PtAg_4$	31,582	2·43
Platinoid	—	41,731	3·1
Manganin	—	46,678	0·
Iron-manganese	88 : 12	67,148	12·7

The first thing which strikes us on looking at the chart (Fig. 6) is that the lines for the pure metals all converge downwards in such a manner as to indicate that their electrical resistance would vanish at the absolute zero of temperature. but that no such convergence is indicated in the case of alloys. We have found that the slightest impurity in a metal changes the position of the resistance line. In the next place, note that the order of conductivity is different at low temperatures to that at ordinary temperatures. At 13° C. pure silver is the best conductor, but at −200° pure copper is better than silver, and the position of mercury is, of course, very different.

Again, the lines of some metals are very much curved. The principal magnetic metals, iron and nickel, have lines which are very concave upwards, and this is a characteristic apparently of many magnetic alloys. The mean temperature coefficient of these magnetic metals between 0° C. and 100° C. is much larger than that of other metals, and the percentage decrease in resistance in cooling them from + 200° C. to − 200° C. is greater than in the case of any other metal. It is worth noting in passing that these magnetic metals, iron and nickel, have smaller atomic volumes than any other metal, and that, generally speaking, the worst conductors amongst the metals are those that have the large atomic volumes and large valency.

Next turning to alloys, we may make mention of a few general facts with regard to their resistance. If to one pure metal we add a small quantity of any other metal the result is always to raise the resistance line almost parallel to that of the predominant constituent. Thus, in our own chart, the alloy consisting of 6 per cent. of copper with 94 per cent. of aluminium is parallel to the aluminium line, but higher up. Three per cent. of aluminium added to 97 per cent. of copper yields an alloy with a resistance line parallel to that of

copper, also higher up. When two pure metals are alloyed together in various proportions there is generally some proportion in which the resultant alloy has a maximum resistivity, and except in the case of alloys of zinc, tin, lead and cadmium with each other, the resistivity of the alloy is greater than that of either of its constituent metals. In the case of many well-known alloys the proportions which give high, if not the highest resistivity are those which correspond to definite and possible chemical combinations of the metals with each other, as, for instance, in the well-known platinum-silver alloy in proportion 33 to 66, which corresponds in proportion with the combination $PtAg_4$; the iron-nickel alloy in proportion of 80 to 20, which corresponds with the combination $NiFe_4$; the platinum-iridium alloy 80 to 20, which corresponds with the combination $IrPt_4$; and the copper-manganese alloy 70 to 30, which corresponds with the compound Cu_3Mn; all of which are, as far as valency is concerned, possible compounds. It is, however, found that very high resistivity generally involves in alloys a want of tenacity and ductility, and when we reach such limits as 100 microhms per cubic centimetre we begin to find the solid alloys becoming less useful on account of this deterioration of their useful mechanical quality.

We have especially studied the electrical resistance at low temperatures of a large series of steel alloys containing varying proportions of nickel, aluminium, chromium, tungsten and manganese in them.

We have found that the electrical effect of adding to the iron the other elements of the alloy is usually to shift up the resistance line nearly parallel to itself, so that the resistance lines of all the iron alloys are nearly parallel to that of the iron line, only the absolute value of all the ordinates is increased. This is equivalent to saying that the effect of the added material is to increase the specific resistance, but not to alter the slope or form of the resistance curve. Amongst these steel alloys there are two or three that are very interesting. A nickel-steel alloy containing 19 per cent. of nickel, sent to us by Mr. R. A. Hadfield, exhibits some very extraordinary properties. Nickel-steel alloys with large percentages of nickel can, as Dr. Hopkinson has shown,* exist over wide limits of temperature in two different physical states, in one of which they are strongly magnetic and in the other of which they are feebly magnetic, and they pass from the non-magnetic to the magnetic on cooling to low temperatures. Here, for instance, is a sample of the 19 per cent. nickel-steel in the non-magnetic condition. If it is cooled in liquid air we can make it pass instantly into a magnetic condition. In the first state it is fairly ductile and plastic, but in the second state it is very hard and brittle. Moreover, its electrical resistance and thermo-electric power are both permanently altered on undergoing this change. In the non-magnetic state it has a high resistivity of about

* See Proc. Roy. Soc. 1890, vol. xlvii. p. 138.

Fig. 6.

Chart showing the relation of Electrical Resistivity and Temperature.

81,500 C.G.S. units per cubic centimetre at 0° C., but on cooling in liquid air and becoming magnetic it is found to have decreased to about 47,200 C.G.S. units when taken at 0° C. A very pretty way of showing this difference in resistivity is to dip one half of a wire of the 19 per cent. nickel-steel in liquid air, and then take it out, and pass a strong electric current through the wire. The current raises the half which has not been dipped into liquid air to a red heat before the other half is visibly red hot.

It is, perhaps, more correct to say that this alloy can exist in an infinity of different physical states, because we have found that the lower it is cooled in temperature the lower its resistivity can be made

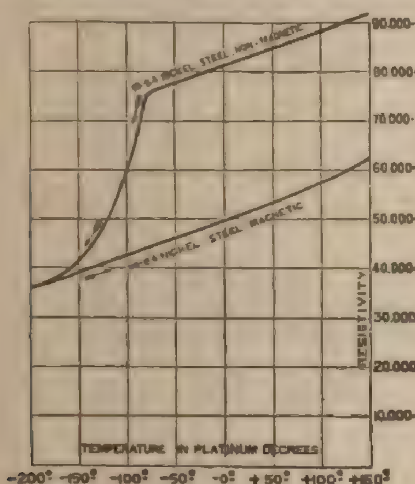

Fig. 7.

Curve showing the Variation of resistivity of nickel-steel (19·64 per cent. nickel) when taken through a cycle of temperature from + 150° to − 200° and back again.

to be when measured again at ordinary temperatures. On heating up the alloy again to a bright red heat it goes back into the non-magnetic ductile state.

The chart (Fig. 7) before you shows the manner in which the electrical resistance varies between the limits of − 200° C. and 150° C. when the alloy is taken through a cycle of temperature beginning at 150° C. in its non-magnetic state.

The 29 per cent. nickel-steel exhibits the same characteristics in a less marked degree. A close study of this interesting material shows that there is room for much valuable work upon it yet.

A manganese-steel, brought to notice by Mr. R. A. Hadfield, having about 12 per cent. of manganese in it, is also capable of existing in two states, a magnetic and a practically non-magnetic variety.

The magnetic variety, which is much more brittle, is, however, in this case formed by the prolonged slow heating of the non-magnetic variety out of contact with air. In the non-magnetic condition the material has a rather high specific resistance at 0° C., about 65,700 C.G.S. units per cubic centimetre; but the magnetic variety has a much lower specific resistance, viz. about 51,400 C.G.S. units at 0° C.

In all these cases it is interesting to note that the change of the alloy into the magnetic variety is accompanied by a decrease in resistivity or increase in conductivity, and an increase in brittleness.

We have tried cooling this non-magnetic variety of manganese-steel in liquid air, but have not been able in that way to make any change in its condition as regards magnetic susceptibility.

There is a particular alloy, of copper 84 per cent., manganese 12 per cent., and nickel 4 per cent., called manganin, which at ordinary temperature exhibits but little change of resistance with change of temperature. On taking the curve of its resistance over wide ranges of temperature we find that its curve is very concave downwards, and the vertex of the curve lies at about 16° C. Hence at ordinary temperatures small changes of temperature make no change in its resistance; but above that point its temperature coefficient is negative, and below it it is positive. All alloys in which a negative temperature coefficient has been observed are probably instances of the same mode of variation of resistance. It may be noted in passing that the element manganese when present in an alloy seems to have a great tendency to produce high resistivity and small temperature coefficient.

Returning then to the pure metals, we may ask, What is the meaning of the fact that in their case the resistance lines all converge so as to indicate that the electrical resistance would vanish at the absolute zero of temperature?

We know that the passage, as we call it, of an electric current through a conductor heats it, and that by Joule's law the rate of production of heat in the conductor is proportional to the square of the current strength and to the total resistance of the conductor.

Suppose we take two wires, say of iron and a certain copper-nickel-aluminium alloy having the same resistivity at 100° C. and of the same size and length. These wires will at + 100° C. have the same resistance. A given current flowing through them will therefore generate heat in them both at the same rate.

Cool them both down, however, to the temperature of liquid air. In the case of iron-wire the resistance is reduced to one-fifteenth of its value at − 200° C., in the other case it is reduced by only 10 per cent. Hence, at the low temperature the alloy dissipates energy for the same current 13½ times as rapidly as the pure metal.

It is a logical deduction from all we know to conclude that if we could reach the absolute zero of temperature the pure metal would not dissipate the energy of the current at all. Imagine two iron wires, then, stretched through space, say from the earth to the moon,

and kept everywhere at the absolute zero of temperature, we could transmit any amount of electrical energy along them without dissipating any of it as heat in the wires.

As a consequence of this, any pure metal cooled to the absolute zero of temperature would become a perfect screen for electro-magnetic radiation, and would be perfectly impenetrable to electro-magnetic induction.

We can show this increase in the power of electro-magnetic

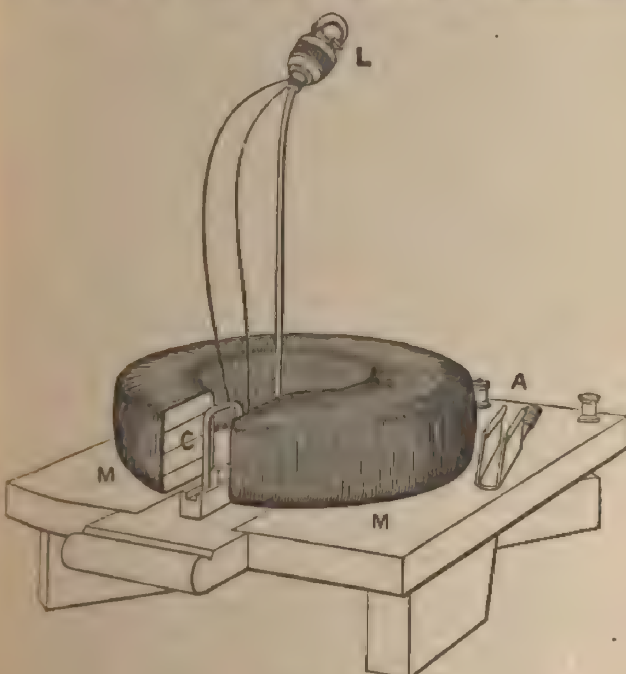

FIG. 8.

An alternating current magnet having a coil C between its poles over which a shield A of aluminium can be placed.

screening by metals when cooled in the following way. A suitable coil of wire C is placed (see Fig. 8) between the poles of an alternating current magnet M, M and a small incandescent lamp L connected with the coil. When the magnet is excited it induces currents in the coil and the lamp glows up. A cap of aluminium A is made of such a size as to drop easily over the coil. This aluminium is not of sufficient thickness or conductivity to screen off the induction when it is warm. If, however, we cool the aluminium cap in liquid air and

then drop it over the coil the lamp for one instant goes out, but it brightens up again as the metal cap instantly warms up. This shows us, however, that if the cap were at the absolute zero of temperature it would then be a complete screen for the induction. In fact, these experiments furnish us with a new definition of what we mean by the absolute zero of temperature. It is the temperature at which perfectly pure metals cease to have any electrical resistance.

In the conduction of currents at ordinary temperatures as we generally know it, two effects are inseparably connected with the conveyance of energy by this process. One is the dissipation of some of the energy as heat in the conductor, the other is a loss of potential or fall of electric pressure, the latter being one of the factors in the equivalent of the energy so dissipated. If, however, the conductor is at the absolute zero of temperature, there would be no heat produced in it, and no fall of potential along it, either for large or small currents. What then under these conditions is the function of the conductor? The answer is, that it becomes a mere boundary serving to limit the electro-magnetic field and determine the direction in which the energy transmission is taking place. These experiments therefore may be regarded as forging one more important link in that chain of experimental evidence which compels us to look for the processes concerned in the conveyance of energy by an electric current, not inside the conductor as we call it, but in the dielectric or medium outside. We may then ask, How is it that different bodies have such various dissipative powers when acting in this way as the boundary of an electro-magnetic field? The only suggestion on this point I venture to make here is as follows:—Materials of high specific resistance have all probably a very complex molecular structure. The alloys of high resistivity are probably not merely solidified mechanical mixtures of metals, but chemical compounds, and even in the case of elementary bodies like carbon and sulphur, which have high resistivity, these last-named bodies may have, owing to their high valency and tendency of their atoms to auto-combination, a complex molecular structure.

This structure may bestow upon them the power of taking up energy from the electro-magnetic medium, just as gases with a highly complex molecular structure are very absorbent of radiant heat, which, if the electro-magnetic theory of light is true, is only another form of electro-magnetic energy. All we know at present about the processes at work during the time a conductor is traversed by an electric current, is that there is a magnetic field outside the conductor and also within the mass of the conductor, and that some mechanism is at work absorbing energy through the surface of the conductor and dissipating it as heat in the interior. The resistance of a conductor is best defined as, and numerically measured by, the number expressing the rate at which it dissipates electro-magnetic energy per unit of current. For the same current, that is for the same external

magnetic field, conductors dissipate this energy at very different rates. Some, like silver and copper, which have the lowest rates, are elements of low valency and relatively small molecular volume, and have probably a simple molecular structure; others, like alloys of high resistivity, have in all probability a more complex molecular structure. Both this last, as well as the molecular mobility charac-

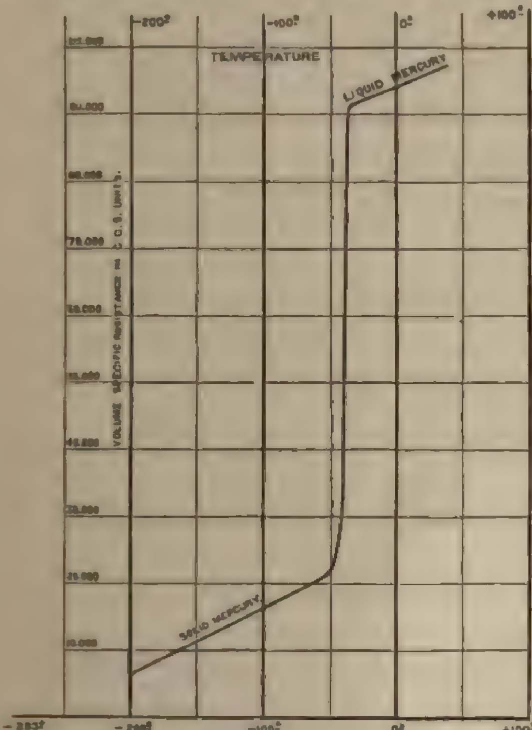

Fig. 9.

Resistivity curve of mercury in terms of platinum temperature.

teristic of the liquid state, are conditions which bestow the power of taking up rapidly and dissipating the energy of the electro-magnetic field, and this energy has to be kept supplied from external energy-transforming sources. We cannot, however, at present profitably construct further mechanical hypotheses to account for this difference between conductors, in the presence of our great ignorance about ether, molecules and energy.

In passing from the liquid to the solid state there is generally an immense increase in the conducting power of metals. This is well

shown in the case of mercury. A glass tube a metre in length was formed into a spiral coil and filled with pure mercury, suitable connections being provided at the ends. This coil was imbedded in a mass of paraffin wax, and a platinum wire thermometer placed in contact with it. The whole mass was then reduced to the temperature of liquid air, and observations taken of the resistance of the mercury as it heated slowly up after being removed from the liquid air. The curve in Fig. 9 shows the manner in which the resistance increases with great suddenness between $-41°$ and $-36°$ as the metal passes into the liquid condition. The resistance becomes four times greater between $-50°$ and $-36°$ in the course of $14°$ rise of temperature, and whilst in the act of passing through the melting

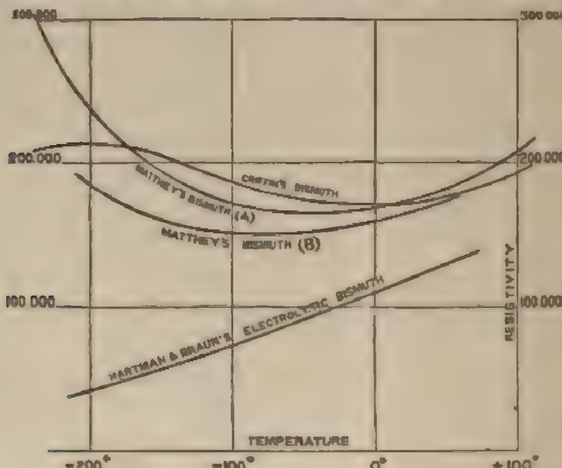

FIG. 10.

Resistivity curves of bismuth in terms of platinum temperature.

point of the mercury at $-38°{\cdot}8$ C. This chart shows that the resistance curve of the mercury in the solid state tends downwards, so as to indicate that its resistivity would completely vanish exactly at the absolute zero of temperature. It is interesting to note also that the portion of the resistance curve belonging to mercury in the solid state is sensibly parallel to that portion of it in the liquid state.

We carried on a long struggle with bismuth in the endeavour to unravel some of the electrical peculiarities of that metal at low temperatures. Chemists are aware of the extreme difficulty of preparing bismuth in a state of perfect chemical purity by purely chemical means. From several different sources we procured bismuth which had been carefully prepared by the reduction of the oxychloride or nitrate after careful re-precipitations. This bismuth was then pressed into wire, and its resistance curves taken down to the lowest attainable

temperatures. We found some very extraordinary results. Although sensibly agreeing in resistivity at ordinary temperatures, in two cases (see Fig. 10) the resistance curves had a minimum point, and after reaching this at about $-80°$ tended upwards again; thus showing that the resistance was increasing as the metal was further cooled. These curves could be repeated as often as necessary with these samples. Another specimen gave a curve with a double bend (see Fig. 10). These results convinced us that it would be necessary to prepare bismuth electrolytically, and with the assistance of Messrs. Hartmann and Braun, of Frankfort, who have made a special study of the preparation of electrolytic bismuth, we were provided with a quantity of the metal which examination showed to be chemically pure. On taking the resistance curve of a sample of this electrolytic bismuth when pressed into uniform wire under great pressure, we found that its behaviour was perfectly normal, and that the resistance line tended downwards, as in the case of all other pure metals, to the absolute zero. Also we found that the specific resistance of this last is very much less than that of the chemically prepared samples, and less even than that employed by Matthiessen. Hence *pure* bismuth is no exception to the law enunciated above. Bismuth is characterised especially by many peculiarities. It has been known for some time that the resistance of a bismuth wire is increased when it is placed in a magnetic field, so that the lines of the field are perpendicular to the direction of the current flow. This is easily shown by means of one of Hartmann and Braun's spirals, manufactured now purposely for measuring magnetic fields.

We have, however, discovered that if bismuth is cooled to the temperature of liquid air the effect of any given magnetic field in changing its resistance is increased many times. Thus, for example: A certain bismuth wire we used had a resistance of 1·690 ohms at 20° C. Placed in a magnetic field of strength 2750 C.G.S. units so that the wire was transverse to the direction of the field, its resistance was increased to 1·792 ohms, or by six per cent. The wire was then cooled in liquid air and its resistance lowered to 0·572 ohms. On putting it then into the magnetic field of strength 2750 C.G.S. units its resistance became 2·68 ohms. Hence it had increased 368 per cent. This magnetic field can thus actually reverse the effect of the cooling, and cause the bismuth, when cooled and magnetised, to have a greater resistance than when at ordinary temperatures and unmagnetised. We are at present engaged in further unravelling the problems presented by this new discovery with regard to bismuth.* It is certainly very startling to find that a magnetic field which increases the resistance only 5 per cent. at ordinary temperatures increases it five times at $-186°$ C. We have recently discovered a similar,

* Since the delivery of this discourse we have been able, by the employment of a powerful electro-magnet kindly lent to us by Sir David Salomons, to increase the resistance of bismuth, when cooled in liquid air, more than 150 times, by magnetising it transversely in a field of 22,000 C.G.S. units.

but much smaller effect in the case of nickel longitudinally magnetised. It will be seen that this process of taking the resistance of a conductor in liquid air is one which affords us a very critical means of discrimination as to the chemical purity of a metal. It ranks almost with the spectroscope as an analytical method. There is one other method by which we can exhibit the change in conductivity in a metal when cooled, and that is by the increased deflection which a disc of the metal experiences when suspended in an alternating current field in such a position that the plane of the disc is at an angle of 45° to the direction of the field.

Time will only permit one brief reference to the behaviour of carbon in regard to electrical conductivity when cooled to low temperatures. We have found that carbon in the form of carbon filaments taken from various incandescent lamps continued to increase in resistance as it was lowered in temperature. The resistivity at various temperatures of the carbon from an Edison-Swan lamp is as follows:—

C.G.S. Units.		Temp. C.
3835×10^3	at	99°
3911×10^3	at	18°·9
3953×10^3	at	1°
4054×10^3	at	− 78°
4079×10^3	at	− 100°
4180×10^3	at	− 182°

These values, when represented on a chart, give almost a straight line, and show that the resistivity of carbon continually increases as it is cooled, but at a very slow rate. Its temperature coefficient is therefore negative, and of about the same absolute magnitude as many alloys of high resistivity. The resistivity of this form of carbon is about three thousand times that of silver. Adamantine carbon taken from a Woodhouse and Rawson lamp had a resistivity 50 per cent. greater.

All the so-called insulators—e. g. glass, gutta-percha, ebonite, paraffin—have resistivities enormously greater than that of carbon, but like it, their resistance increases as the temperature is lowered. For the sake of comparison we have placed upon this chart of lines of metallic resistivity (referring to the large diagram used at the lecture) the resistance line of carbon with ordinates drawn to a scale of one-hundredth part of those of the metals. To properly represent to the full scale the line of carbon, this chart, which is 15 feet long, would have to be made one-third of a mile long. If we desired to represent on the same scale the resistivity of gutta-percha, the length of the chart would have to be billions of miles—in fact, so long that light would take 5000 years to traverse it from one end to the other; even then, to represent to the same scale the resistance lines of paraffin and ebonite, it would have to be thirty or forty times longer.*

We must next pass on to consider some problems in thermo-

* The resistivities of platinoid, carbon, and gutta-percha at 0° C. are nearly in the ratio of the numbers 4×10^4, 4×10^6 and 4×10^{24}.

electricity which have engaged our attention. If we construct a thermo-electric couple of two metals and connect this with a galvanometer, and if one junction is kept at a constant temperature, say 0° C., whilst the other junction is heated or cooled to various temperatures, we shall in general, but not always, find an electromotive force acting in this circuit when the junctions are at different temperatures. This electromotive force depends on three things—the nature of the metals, the temperatures of the junctions, and on a certain temperature called the *neutral temperature* of the metals. An important matter in the experimental study of thermo-electric action is to discover the position of these neutral temperatures, when different metals are tested with lead as the standard of comparison, and when one junction is kept at 0° C. Elaborate experiments made by Professor Tait many years ago furnished full information on this matter for temperatures lying above 0° C., and we especially desired to extend this knowledge to ranges of temperature between 0° C. and −200° C. Accordingly, a number of thermo-electric junctions were prepared of various pure metals and alloys, the comparison metal

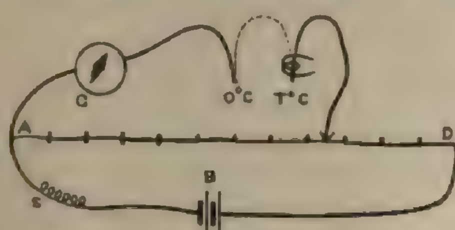

FIG. 11.

Potentiometer arrangement for measuring thermo-electromotive forces.

being always pure lead. These couples were grouped together, and one set of junctions always kept at 0° C. in melting ice. The other set of junctions was cooled to various low temperatures by means of liquid air. The experimental process then consisted in measuring the electromotive force set up in each couple respectively, and at the same instant measuring the temperature of the low temperature junction. After various failures a device was adopted for making this double measurement with great accuracy and expedition.

The arrangement consisted of a combined potentiometer and resistance balance (see Fig. 11). A long uniform wire stretched over a scale had a battery connected to its two ends so as to make a fall of potential down the wire which could be regulated by appropriate resistances. It will be easily seen that we can combine a galvanometer and resistance coil with this arrangement in such a manner as to form it into a Wheatstone's bridge or a potentiometer. In this latter form of instrument an unknown electromotive force is balanced

against the known fall of potential down a certain length of a graduated wire, and a galvanometer employed to ascertain the point on the slide wire at which this is the case. Omitting details, it may be stated that I succeeded in devising an arrangement of circuits in which this change from a potentiometer to a resistance bridge was effected by moving two brass plugs from one pair of holes to another. This instrument formed a most useful combined resistance and electromotive force measurer which enabled us to do two things—first, to measure the electromotive force in any thermo couple; secondly, to measure the temperature of the low temperature junction by measuring the resistance of a platinum wire wound round that junction and acting as a thermometer. In actual practice the platinum thermometer consisted of a small hollow copper cylinder, in the interior of this cylinder being inserted a number of the thermo junctions, and round the outside of which the platinum thermometer wire was wound. Aided

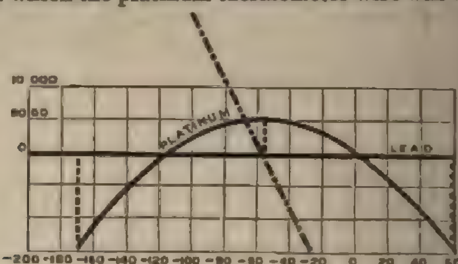

Fig. 12.

Curve of thermo-electromotive force of a platinum-lead couple at various temperatures; one junction kept at 0° C., the temperature of the other being varied. The sloping dotted line represents the variation of the thermo-electric power of platinum with respect to lead.

by this device we were able to measure temperatures with an accuracy of $\frac{1}{25}$ of a degree at a temperature of $-200°$ C., and to ascertain at the same instant the exact electromotive force acting in the couple. When these arrangements had been perfected the method adopted was to put one set of the junctions in melting ice. The other set, enclosed in the copper cylinder, were imbedded in a mass of paraffin wax, which was then cooled down to the temperature of liquid air. The mass was then removed and inserted in a vacuum vessel, and allowed to heat up very slowly. At frequent intervals during the heating the electromotive force of the couple was taken, and also the temperature of the junction.*

The events which under such conditions happen in the case of a platinum-lead junction can easily be shown and are very interesting (see Fig. 12). At the first immersion of one junction in liquid air, whilst the other is in melting ice, we get a current as shown by the

* For fuller information see Dewar and Fleming on the 'Thermo-Electric Powers of Metals and Alloys,' 'Philosophical Magazine,' July 1895.

Fig. 13.

Chart of Thermo-Electromotive Forces of Pure Metals compared with Lead.

ometer in one direction. On lifting one junction out of the air it begins to warm up. The first effect of this heating is ce the thermo current in the circuit. At about − 111° on our m scale, some distance therefore above that of liquid air, the in the circuit falls to zero. As the junction continues to heat current increases again in posite direction. At about the low temperature junc- reaches the temperature the neutral temperature, e current is a maximum direction. It then begins off once more, and finally s zero again when the two ns are both at the tem- re of melting ice, and it increases in the opposite on as this variable junction es to warm up from 0° C. er temperatures.

ving carried out the obser- described with all our couples, the results were nted on a chart (see Fig. follows:—A horizontal as taken on which were l off divisions representing m temperatures. Vertical es were then drawn at temperatures for each representing the electro- force in this couple when d junction was at the tem- re denoted by the abscissa ordinate. In this way a of curves were delineated all passed through the representing 0° C. These are the curves of thermo- motive force.

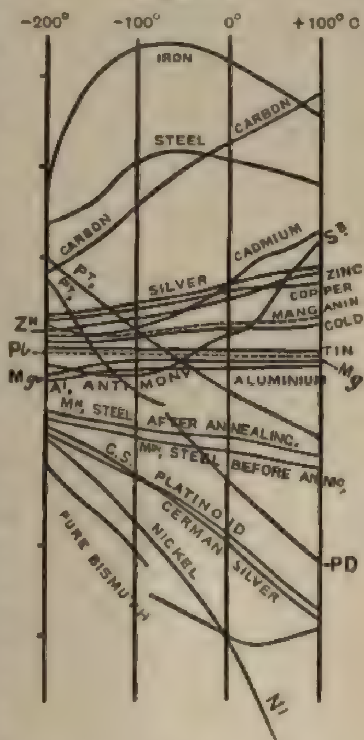

Fig. 14.

Curves showing the variation with temperature of the thermo-electric power of various metals. The thermo-electric line of lead being represented by the dotted line.

Professor Tait's researches s subject he adopted a of representing the facts has many advantages. Suppose the couple to have one at a constant temperature and the other to be vary- At any instant the electromotive force of the couple is g at a certain rate with the changing temperature of the nstant junction. This rate measures what is called the

thermo-electric power of the metals with respect to each other at that temperature. If we measure the slope of the electromotive force curve at any point, it can easily be shown that the numerical value of this slope gives us the rate of change of electromotive force with temperature. If we plot these slopes in terms of the corresponding temperature, we obtain another set of curves called curves of thermo-electric power. Lead is always taken as the standard metal for comparison, because the Thomson effect in lead is zero. From our chart of thermo-electromotive forces we have constructed another one of thermo-electric powers (see Fig. 14). The lines of thermo-electric power cut the lead line in various places, and the temperature at which they do this is called the neutral temperature of that metal with respect to lead. Professor Tait deduced from his experiments that these thermo-electric lines were straight lines for temperatures above zero Centigrade, and he made, in addition, the important discovery that for certain metals such as iron and nickel the thermo-electric lines have sudden changes of direction at high temperatures.

The general result of our investigations at low temperatures is to show that, whilst in some cases the thermo-electric lines, as may be seen from the diagram in Fig. 14, are approximately straight lines for temperatures down to the lowest reached, they are not all by any means straight lines. In some cases, such as iron and bismuth, we find sudden changes of direction of the thermo-electric lines similar to those found by Professor Tait at higher temperatures, and this indicates a change in sign in the Thomson effect at that point. Moreover, in many cases there is a decided tendency of the lines of many metals to bend round in a manner which indicates that their thermo-electric power probably would become zero at the absolute zero of temperature.

The temperature at which the thermo-electric line of any metal crosses the line of lead gives us the neutral temperature of that metal with respect to lead, and at that temperature the metal is thermo-electrically identical with lead. If one junction of a couple is at a temperature as far above the neutral temperature of the metals as the other is below it, the couple will give no electromotive force. This provides us with an experimental method of determining the position of certain neutral points. Thus, for instance, if one junction of a platinum-zinc couple is placed in liquid air and the other is raised to above 30° we get no electromotive force from that couple. This indicates that the neutral temperature of platinum and zinc is about − 85°, and this is shown to be the case from the chart. Two general conclusions are arrived at from a study of the thermo-electric lines as laid down in our chart. The first of these is that the thermo-electric lines of many metals are by no means straight lines over extreme ranges of temperature. Hence the thermo-electric power is not simply a linear function of the absolute temperature. The second important fact is, that in the thermo-electric lines of

certain metals at low temperatures there are sudden changes of direction which indicate a change in the sign of the Thomson effect in that metal at that temperature, and probably, therefore, some important molecular change at the corresponding temperature.

In the case of the 19 and 29 per cent. nickel-steel alloys there is an interesting thermo-electric phenomenon. If a loop of wire of this material is partly dipped in liquid air, the portion cooled becomes thermo-electrically different from the remainder, and gives a strong thermo current if connected to a galvanometer and warmed at one point, where the changed and unchanged portions meet.

Leaving the further elaboration of these points, we must next notice some of the facts with respect to the magnetisation of iron at low temperatures. Professor Dewar mentioned, in a discourse on the scientific uses of liquid air, some results obtained on cooling small steel magnets. These effects we have since again explored at greater length.

Let me show you, in the first place, the effect of cooling a small steel permanent magnet to the temperature of liquid air. We will first take a magnet made of a fragment of knitting needle or ordinary carbon steel and examine the effect of low temperature upon it. Placing the magnet behind the small suspended magnetic needle of a magnetometer we obtain a deflection of the magnetometer needle, which is a measure of the magnetisation of the magnet causing the deflection. On bringing up a small vessel of liquid air and immersing in it the magnet under test we notice at once a sudden decrease in the deflection of the magnetometer needle. This indicates that a notable percentage of the magnetisation of the magnet has been removed. On taking away the liquid air bath and allowing the magnet to heat up again we find that there is a still further decrease in magnetisation. On cooling it again with liquid air the magnetisation then increases, and from and after that time the effect of the cooling is always to increase the moment of the magnet, and the effect of heating it up again always to decrease the moment of the magnet. Hence we see that the effect of the first immersion in liquid air is to give a shock to the magnet which deprives it permanently of a considerable percentage of its magnetism; but when once it has survived this treatment, then cooling it strengthens the magnet, and warming it weakens it.

This is not by any means always the case. If we take a magnet made of the 19 per cent. nickel-steel, the peculiar characters of which were explained a few moments ago, we shall find a very different state of affairs. Here we see the first effect is, as before, to remove a very considerable percentage of the initial magnetisation; but after that stage is passed, then cooling this nickel-steel magnet always weakens it still more, and warming it up again strengthens it. The subsequent effect of cooling is therefore in the opposite direction in the carbon-steel and in this nickel-steel. These changes of moment can best be represented by a diagram of lines as in Fig. 15.

We have in this way examined the behaviour of magnets made of a very large number of steels—chromium-steels, aluminium-steels, tungsten-steels, silicon-steels and nickel-steels, in various states of temper, hard and soft. We find that in some cases there is no initial decrease of magnetism at all, and that the steady state begins at once. Broadly, however, the results amount to this :—A steel magnet when plunged into liquid air generally loses some fraction of its magnetisation, but that after a few such immersions it arrives at a fixed condition in which the effect of cooling it is in most cases to produce an increase of magnetic moment, but in a few exceptional cases to produce a decrease of magnetic moment. In the case of the nickel-steels we have found very curious changes of magnetic

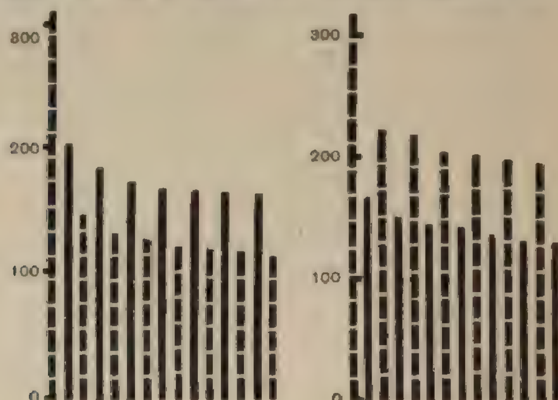

Fig. 15.

Diagram showing changes of magnetic moment of a magnet when alternately cooled in liquid air and warmed up again to $+5^\circ$ C. The length of the firm lines represents the value of the magnetic moment when cooled, and that of the dotted when warm.

moment as the magnet is heated up from -186° C. to $+300^\circ$. There is a maximum magnetic moment at about 40° C. (see Fig. 16) in the case of the 19 per cent. nickel-steel.

In the technical use of magnets for instrumental purposes they have to go through a process called *ageing* to get rid of the sub-permanent magnetism. One of the best ways of ageing a magnet is to plunge it several times into liquid air.

We have given a large amount of attention to a study of the changes taking place in the magnetic qualities of soft or annealed, and also in hard iron when cooled to very low temperatures.

In the first place, we have examined the change in the permeability of iron at the temperature of liquid air. If a ring of iron is wound over with a coil of wire and subjected to gradually in-

creasing magnetising forces, this force produces magnetisation in the iron, but the magnetisation does not increase proportionally with the force. It tends to a limit, and the curve which shows this variation is called a magnetisation curve. The number which expresses the ratio of the magnetisation to the magnetising force is called the susceptibility of the iron. Instead of considering the magnetisation of the iron as one of the variables, it is often convenient to consider the induction in the iron, and the induction is defined as a quantity, the rate of change of which with time measures the electromotive force set up in a secondary circuit wound round the iron ring.

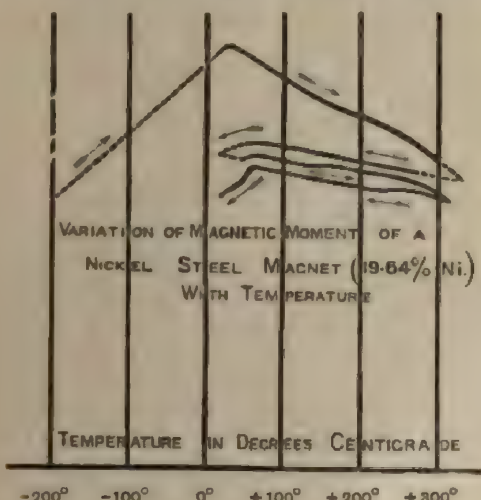

FIG. 16.

The ratio between the induction and the magnetising force at any instant is called the permeability of the iron. By tedious experiments with the ballistic galvanometer, it is possible to draw out a complete magnetisation curve of the iron, starting from the lowest induction up to the point at which the iron becomes practically saturated. Assisted by Mr. J. E. Petavel, who has given us most valuable help in these very tedious magnetic observations, as well as in the subsequent reductions of them, a large number of observations have been made on the permeability of a carefully annealed iron ring made of very fine Swedish iron of the highest quality.* The result is to show—

* It is only right to add that in other portions of this work, especially in the resistance and thermo-electric work, we have been much indebted for careful and persevering assistance to Messrs. J. and D. Morris and, in lesser degree, to Messrs. Jakeman and Tilney for help in other observations requiring several simultaneous observers.

as seen from the curve (see Fig. 17)—that cooling the iron to −186° C. slightly diminishes the permeability. In other words, it requires a greater magnetic force to produce a given amount of magnetisation when the iron is at −186° C. than when it is at the ordinary temperature.

When, however, we began to study the behaviour of hardened iron in this respect, we found ourselves in the presence of very

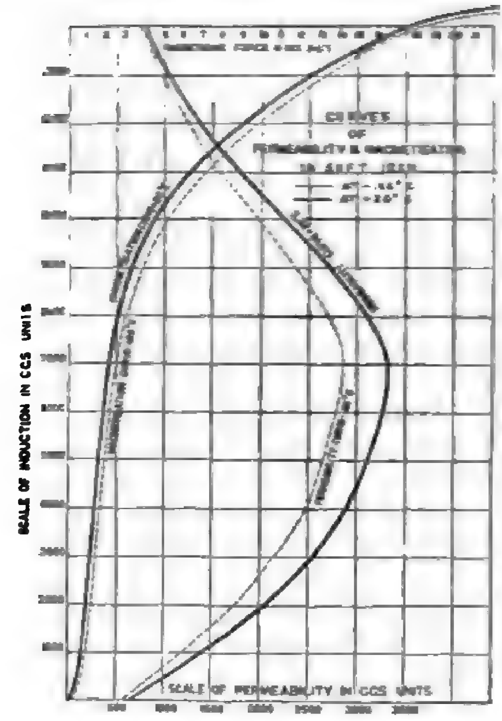

FIG. 17.

Magnetisation and permeability curves of soft iron at 20° C. and −186° C.

curious effects. If pure iron, which has been carefully annealed, is twisted, knocked, bent, stretched, or compressed, it passes into a state known as hard iron, and hard iron has very different magnetic qualities from soft iron. A very extended series of experiments with rings of hard iron have shown that hard iron, at least in certain cases, has its permeability greatly increased by cooling, and this change takes place with great suddenness. We can show you by a simple experiment that this is the case. If we take this hard iron

ring, which has two coils of wire wound round it, and connect one of these circuits to a battery, we shall send a current through this primary coil and magnetise the iron ring. If the other, or secondary circuit is connected to a galvanometer, then at the instant of starting the primary current there is a transitory induced current produced in the secondary circuit. As long as the induction in the iron remains constant no electric change will take place in this secondary circuit. If, however, we plunge the iron ring into liquid air, whilst still keeping the primary current constant, we find again a secondary current produced at the moment of cooling the iron. This indicates a sudden increase of permeability at the instant of cooling. If we bring the ring out of the liquid air we find it retains some of the increased permeability acquired on cooling, but loses a portion of it more slowly if it is heated up again to ordinary temperatures by plunging it into a bath of alcohol. Owing to these changes we found it impossible to repeat again exactly any required magnetisation curve in the case of the hard iron. The sudden cooling alters the magnetic qualities of the unannealed iron to such an extent that it is not possible to get it twice in exactly the same state.

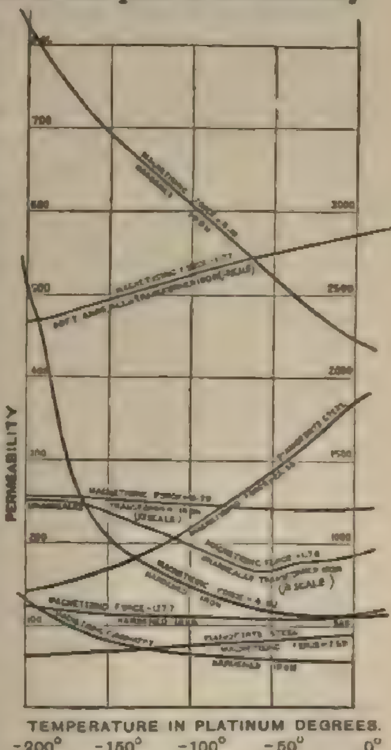

Fig. 18.

Curves showing the variation of permeability of iron with temperature between 0° C. and −200° C.

By subjecting a hard iron ring to frequent reversals of the same magnetising force, whilst it is warmed up slowly from the temperature of liquid air up to ordinary temperatures, we have been able to trace the gradual decrease of the permeability at any constant force throughout this range of temperature, and the results are embodied in the series of curves in Fig. 18.

We have found, on the other hand, that unhardened steel pianoforte wire behaves like soft annealed iron.

We have then examined the hysteresis of iron at low temperatures. As the meaning of that term was very fully explained by the inventor of it in a discourse given quite recently, no time need be spent in an

elaborate explanation of it. It is sufficient to say that when iron is magnetised and demagnetised, or carried round a cycle of magnetisation in which its direction of magnetisation is first in one direction and then in the other, this process involves the expenditure of energy, and such dissipation of energy is spoken of as the hysteresis loss in iron. It would occupy too much time to attempt to explain in full detail the manner in which this dissipated energy can be measured. As a matter of fact, the method we adopted was the laborious but exact one of delineating a complete magnetisation curve of the iron, by means of observations taken with the ballistic galvanometer for various maximum values of the magnetising force. In this way we were able finally to arrive at a curve which represented by its ordinates the value of the hysteresis loss in the iron in ergs per cubic

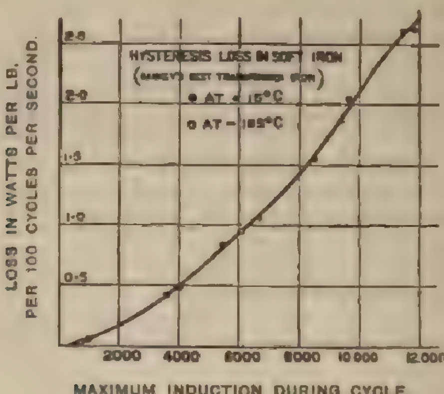

Fig. 19.

Variation of hysteresis loss in soft iron with temperature.

centimetre per cycle, and the abscissæ the maximum value of the corresponding magnetic induction. When curves had been drawn out (see Fig. 19) from all the many hundreds of observations for the case of the same soft iron ring at ordinary temperatures and at the temperature of liquid air, we found little or no sensible difference between them. The result is, then, that there is no appreciable change in the magnetic hysteresis loss of very carefully annealed soft Swedish iron when cooled to these low temperatures.* With regard to the hard iron, although the permeability is increased, it is most difficult to say yet whether the hysteresis is increased or not, as every fresh reduction in temperature of the iron alters its physical

* The iron used in all these experiments was a sample of Sankey's transformer iron, kindly sent to us by Mr. B. Jenkins.

state, and makes it almost impossible to obtain similar repeated measurements.

It is natural to inquire how far accepted theories of magnetic action are able to reconcile the above-mentioned results. Some of them undoubtedly are in accord with deductions from received hypotheses. It is generally considered that the facts connected with the magnetisation of iron indicate that each molecule, or perhaps small groups of molecules, of the iron are complete micro-magnets, and that in the unmagnetised condition of the iron these molecular magnets arrange themselves in groups or in closed circuits so that for each little group the external magnetic action or magnetic moment is approximately zero. Magnetisation consists in arranging the members of some or all of these groups so as to co-lineate the direction of more or less of the molecular magnets and produce an external resultant magnetic moment.

Let us then consider one such little group by the aid of a model made of small magnets, such as Ewing has suggested and used.

Suppose the members of this group to be at a certain distance from each other, and we apply a given magnetising force which is just sufficient to open out the group and co-lineate the magnetic axes of the several members of it.

Next, suppose we cool this iron, this would result in bringing the members of the group into closer contiguity. The result of this will be an increase of the interpolar magnetic forces of the different members of the group; and as we can see from the behaviour of the model, it would require a greater magnetic force to effect the same amount of co-lineation of the molecular magnets. This, therefore, corresponds with what we find to be the case on cooling soft iron to very low temperatures. Professor Dewar's experiments have shown that the tensile strength of iron and steel is increased to about double on cooling to $-182°$ C., and it is quite reasonable to suppose that this is the result, in part at least, due to an approximation of the molecules.

As regards the behaviour of magnetised steel and iron when cooled, it is highly likely, when the groups of molecular magnets have been opened out more or less, that some of these are in a condition of instability, in which bringing the members of the group nearer together will have the effect of making them close up again into magnetic circuits of no external action. Hence, if this is the case, the first effect of the sudden cooling will be to effect the observed change. These half-hearted groups of molecular magnets constitute the subpermanent magnetism which it is our desire to get rid of in ageing a magnet. Then, as regards the effect of temperature changes on the magnet when the stable condition of affairs is reached. In order to explain this, I think we must consider the action of the molecular groups upon each other. The approximation of molecular groups will in general, after the magnet is aged, have the effect of co-lineating more completely the different members of the groups, and hence increase

Fall, of $7\frac{1}{2}$ inches, and of the Horse Shoe, of 2·18 feet, would probably have been much greater had the water been less limpid.

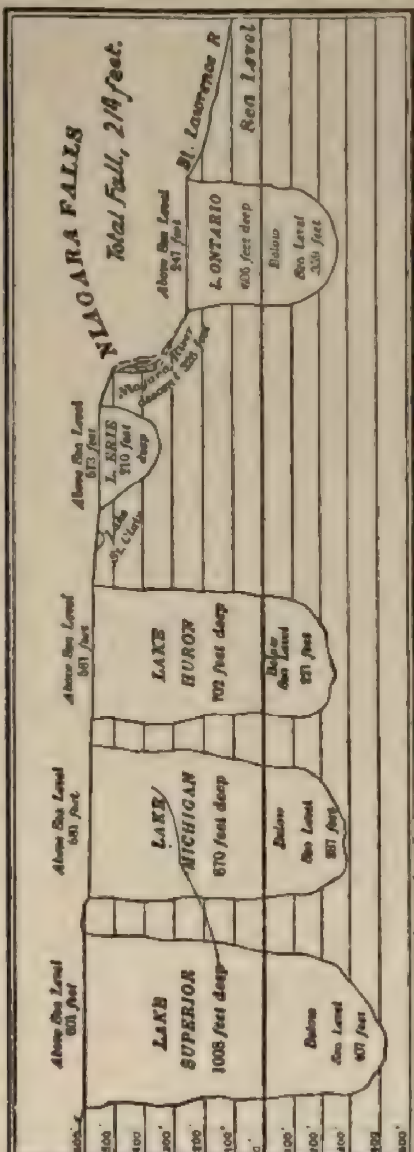

Fig. 1.—Profile showing Difference in Level of the Great Lakes.

The roar of the Falls, which can be heard for many miles, has a deep note, four octaves lower than the scale of the ordinary piano. The fall of such an immense body of water causes a very perceptible tremor of the ground throughout the vicinity. The existence of the Falls is also indicated by huge clouds of mist which, rising above the rainbows, tower sometimes a mile in air before breaking away.

It was Mr. Thomas Evershed, an American civil engineer, who unfolded the plan of diverting part of the stream at a considerable distance above the Falls, so that no natural beauty would be interfered with, while an enormous amount of power would be obtained with a very slight reduction in the volume of the stream at the crest of the Falls. Essentially scientific and correct as the plan now shows itself to be, it found prompt criticism and condemnation, but not less quickly did it rally the able and influential support of Messrs. W. B. Rankine, Francis Lynde Stetson, Edward A. Wickes, and

Edward D. Adams, who organised the corporate interests that, with an expenditure of 1,000,000*l.* in five years, have carried out the present work.

So many engineering problems arose early in the enterprise, that after the survey of the property in 1890, an International Niagara Commission was established in London, with power to investigate the best existing methods of power development and transmission, and to select from among them, as well as to award prizes of an aggregate of 4400*l.* This body included men like Lord Kelvin, Mascart, Coleman Sellers, Turrettini and Dr. Unwin, and its work was of the utmost value. Besides this the Niagara Co. and the allied Cataract Construction Co. enjoyed the direct aid of other experts, such as Prof. George Forbes, in a consultative capacity; while it was a necessary consequence that the manufacturers of the apparatus to be used threw upon their work the highest inventive and constructive talent at their command.

The time-honoured plan in water-power utilisation has been to string factories along a canal of considerable length, with but a short tail race. At Niagara the plan now brought under notice is that of a short canal with a very long tail race. The use of electricity for distributing the power allows the factories to be placed away from the canal, and in any location that may appear specially desirable or advantageous.

The perfected and concentrated Evershed scheme comprises a short surface canal 250 feet wide at its mouth, 1¼ mile above the Falls, far beyond the outlying Three Sisters Islands, with an intake inclined obliquely to the Niagara River. This canal extends inwardly 1700 feet, and has an average depth of some 12 feet, thus holding water adequate to the development of about 100,000 horse-power. The mouth of the canal is 600 feet from the shore line proper, and considerable work was necessary in its protection and excavation. The bed is now of clay, and the side walls are of solid masonry 17 feet high, 8 feet at the base, and 3 feet at the top. The north-eastern side of the canal is occupied by a power house and is pierced by ten inlets guarded by sentinel gates, each being the separate entrance to a wheel pit in the power house, where the water is used and the power is secured. The water as quickly as used is carried off by a tunnel to the Niagara River again.

The massive canal power house is a handsome building designed by Stanford White, and likely to stand until Niagara, spendthrift fashion, has consumed its way backward through its own crumbling strata of shale and limestone to the base of it. This building is outwardly of hard limestone, and inwardly of enamel brick and ordinary brick coated with white enamel paint. It is 200 feet in length at present, and has a 50-ton Sellers electric travelling crane for the placing of machinery and the handling of any parts that need repair. The wheel pit, over which the power house is situated, is a long deep cavernous slot at one side under the floor cut in the rock,

parallel with the canal outside. Here the water gets a fall of about 140 feet before it smites the turbines. The arrangement of the dynamos generating the current up in the power house, is such that each of them may be regarded as the screw at the end of a long shaft, just as we might see it if we stood an ocean steamer on its nose with its heel in the air. At the lower end of the dynamo shaft is the turbine (Fig. 2) in the wheel pit bottom, just as in the case of the steamer shaft we find attached to it the big triple or quadruple expansion marine steam engine. Perhaps we might compare the dynamo and the turbine to two reels, stuck one each end of a long lead pencil, so that when the lower reel is turned the upper reel must turn also. You might also compare the dynamos to bells up in the old church steeple, and the turbines to the ringers in the porch, playing the chimes and triple bob majors by their work on the long ropes that hang down. The wheel pit which contains the turbines is 178 feet in depth, and connects by a lateral tunnel with the main tunnel running at right angles. This main tunnel is no less than 7000 feet in length, with an average hydraulic slope of 6 feet in 1000. It has a maximum height of 21 feet, and a width of 18 feet 10 inches, its net section being 386 square feet. The water rushes through it and out of its mouth of stone and iron at a velocity of 26½ feet per second, or nearly 20 miles an hour.

More than 1000 men were employed continuously for more than three years in the construction of this tunnel. More than 300,000 tons of rock were removed, which have gone to form part of the new foreshore near the power house. More than 16,000,000 bricks were used for the lining, to say nothing of the cement, concrete and cut stone. The labour was chiefly Italian. The brick that fences in the headlong torrent consists of four rings of the best hand-burned brick of special shape, making a solid wall 16 inches thick. In some places it is thicker than that. Into this tunnel discharges also by a special subtunnel, the used-up water from the water wheels of the Niagara Falls Paper Co. The turbines (Fig. 3) have to generate 5000 horse-power each, at a distance of 140 feet underground, and to send it up to the surface. For this purpose the water is brought down to each by the supply penstock, made of steel tube and 7½ feet in diameter. This water impinges upon what is essentially a twin wheel, each receiving part of the stream as it rushes in at the centre, the arrangement being such that each wheel is three stories high, part of the water in the upper tier serving as a cushion to sustain the weight of the entire revolving mechanism. These wheels, which have thirty-two buckets and thirty-six guides, discharge 430 cubic feet per second, and they make 250 revolutions per minute. At 75 per cent. efficiency they give 5000 horse-power. The shaft that runs up from each one to the dynamo is of peculiar and interesting construction. It is composed of steel ⅜ inch thick, rolled into tubes which are 38 inches in diameter. At intervals this tube passes through journal bearings or guides that steady it, at which the shaft is narrowed to 11 inches in diameter and

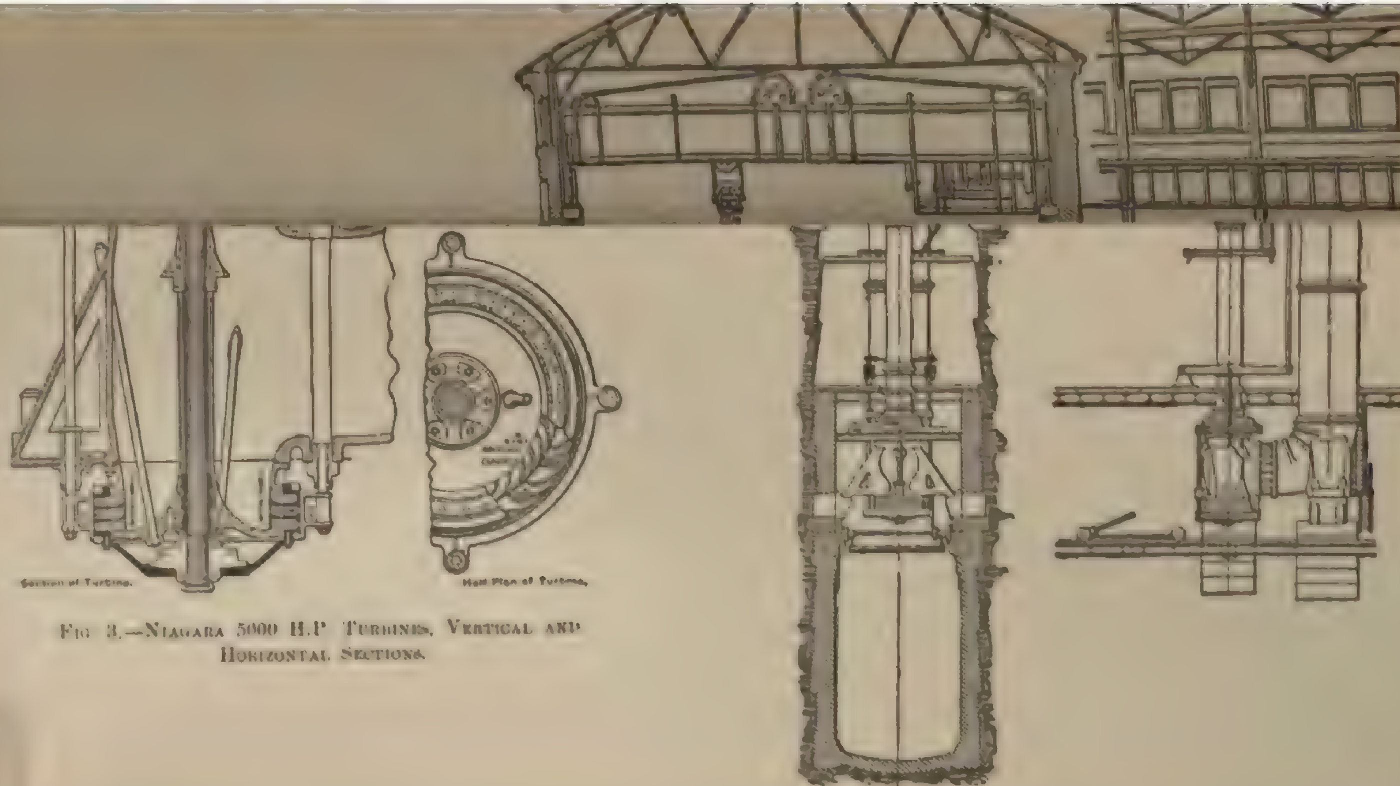

FIG. 3.—NIAGARA 5000 H.P. TURBINES, VERTICAL AND HORIZONTAL SECTIONS.

lid, flaring out again each side of the journal bearings. The speed tes of the turbine wheels are plain circular rims, which throttle the scharge on the outside of the wheels, and which, with the co-operan of the governors, keep the speed constant within 2 per cent. under dinary conditions of running. These wheels are of the Swiss design Faesch and Picard, and have been built by I. P. Morris & Co. of hiladelphia, for this work.

The dynamos thus directly connected to the turbines are of the esla two-phase type (Fig. 4). Each of these dynamos produces two

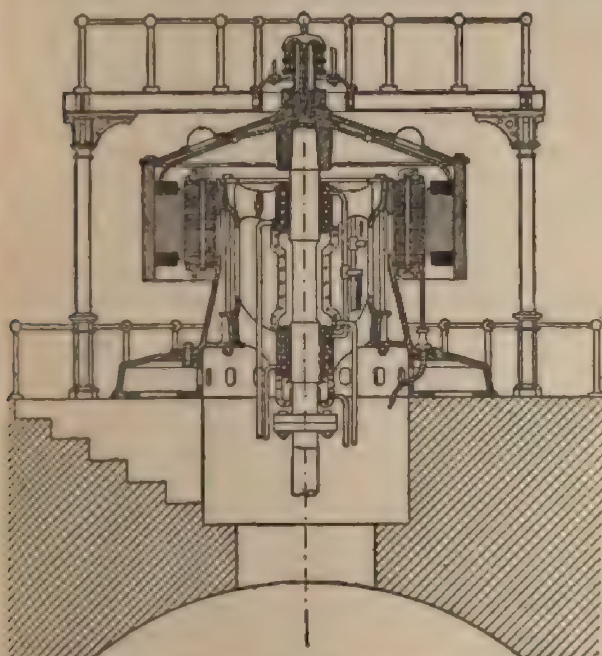

Fig. 4.—Niagara 5000 Horse-Power Two-Phase Alternator.

ternating currents, differing 90 degrees in phase from each other, ch current being of 775 ampères and 2250 volts, the two added tother making in round figures very nearly 5000 horse-power. This ount of energy in electrical current is delivered to the circuits for e when the dynamo is run by the turbine at the moderate speed of 0 revolutions per minute, or say 4 revolutions per second. Here en we have, broadly, a Tesla two-phase system embodying the novel ggestions and useful ideas of many able men, among whom should specially mentioned Mr. L. B. Stillwell, the engineer of the

Westinghouse Electric Co. upon whom the responsibility was thrown for its success.

Each generator, from the bottom of the bed plate to the floor of the bridge above it is 11 feet 6 inches high. Each generator weighs 170,000 lbs. and the revolving part alone weighs 79,000 lbs. In most dynamos the armature is the revolving part, but in this case it is the field that revolves while the armature stands still. It is noteworthy that if the armature inside the field were to revolve in the usual manner instead of the field, its magnetic pull would be added to the centrifugal force in acting to disrupt the revolving mass; but as it is, the magnetic attraction towards the armature now acts against the centrifugal force exerted on the field, and thus reduces the strains in the huge ring of spinning metal. The stationary armature inside the field is built up of thin sheets of mild steel. Along the edges of these sheets are 187 rectangular notches to receive the armature winding in which the current is generated. This winding is in reality not a winding, as it consists of solid copper bars $1\frac{11}{32}$ by $\frac{7}{16}$ inch, and there are two of these bars in every square hole, packed in with mica as a precaution against heating. These copper conductors are bolted and soldered to V-shaped copper connectors, and are then grouped so as to form two separate independent circuits. A pair of stout insulated cables connect each circuit with the power house switchboard.

The rotating field magnet outside the armature consists of a huge forged steel ring, made from a solid ingot of fluid compressed steel, 54 inches in diameter, which was brought to a forging heat and then expanded upon a mandril, under a 14,000-ton hydraulic press, to the ring, 11 feet $7\frac{1}{8}$ inches in diameter. On the inside of this ring are bolted twelve inwardly-projecting pole pieces of mild open hearth steel, and the winding around each consists of rectangular copper bars encased in two brass boxes. Each pole piece with its bobbin weighs about $1\frac{1}{4}$ tons, and the speed of this mass of steel, copper and brass is 9300 feet, or $1\frac{3}{4}$ miles per minute, when the apparatus is running at its normal 250 revolutions. Not until the ring was speeded up to 800 revolutions, or six miles per minute, would it fly asunder under the impulse of centrifugal force. As a matter of fact, 400 revolutions is the highest speed that can be attained. This revolving field magnet is connected with the shaft that has to turn it, and is supported from above, by a six-armed cast steel spider keyed to the shaft, this spider or driver forming a roof or penthouse over the whole machine. The shaft itself is held in two bearings inside the castings around which the armature is built up, and at the bearings is nearly 13 inches in diameter. At the lower end is a flange fitting with the flange at the top of the turbine shaft, and at the upper end is a taper over which the driver fits. The driver and shaft have a deep keyway, and into this a long and massive key fits, holding them solidly together. The driver is of mild cast steel, having a tensile strength of 74,700 lbs. per square inch. The bushings of the bearings are

of bronze, with zigzag grooves in which oil under pressure is in constant circulation. Grooves are also cut in the hub of each spider, to permit the circulation of water to cool the bearings, this water coming direct from the city mains at a pressure of 60 lbs. to the square inch. The oil returns to a reservoir and is used over and over again. Provision has been made against undue heating, and plenty of chance is given for air to circulate. This is necessary, as about 100 horse-power of current is going into heat, due to the lost magnetisation of the iron and the resistance in the conductors themselves. Ventilators or gills in the driver are so arranged as to draw up air from the base of the machine and eject it at considerable velocity, so that whatever heat is unavoidably engendered is rapidly dissipated.

In almost all electrical plants the switchboard is a tall wall or slab of marble or mahogany, not unlike a big front door with lots of knobs, knockers and keyholes on it; but at the Niagara power house it takes the form of an imposing platform, or having in mind its controlling functions, we may compare it to the bridge of an ocean steamer, while the man in charge or handling the wheels answers to the navigating officer. The ingenious feature is employed of using compressed air to aid in opening and closing the switches. The air comes from a compressor located at the wheel pit and driven by a small water motor. It supplies air to a large cylindrical reservoir, from which pipes lead to the various switches, the pressure being 125 lbs. to the square inch. Another interesting point is that the measuring instruments on the switchboard do not measure the whole current, but simply a derived portion of determined relation to that of the generators. All told, less than a thirtieth of a horse-power gives all the indications required. To the switchboard, current is taken from the dynamos by heavy insulated cable, and it is then taken off by huge copper bus bars which are carefully protected by layers of pure Para gum and vulcanised rubber, two layers of each being used; while outside of all is a special braided covering, treated chemically to render it non-combustible. The calculated losses from heating in a set of four bus bars carrying 25,000 horse-power, or the total output of the first five Niagara generators, is only 10 horse-power. About 1200 feet of insulated cable have been supplied to carry the current from the dynamos to the switchboard in the power house. It has not broken down until between 45,000 and 48,000 volts of alternating current were applied to it. There are 427 copper wires in that cable, consisting of 61 strands laid up in reverse layers, each strand consisting of seven wires. Next to the strand of copper is a wall of rubber one-quarter inch thick, double coated. Over this is wrapped absolutely pure rubber, imported from England and known as cut sheet. Then come two wrappings of vulcanisable Para rubber, next there is a wrapping of cut sheet, and on top of that are two more rubber coats. This is then taped, covered with a substantial braid, and vulcanised. The object in using the cut sheet is to vulcanise it

by contact, in order to make it absolutely water-tight. This cable weighs just over 4 lbs. to the foot, of which 3 lbs. are copper and 1 lb. insulation.

We have thus advanced far enough to get our current on to the bus bars, and the next step is to get it from them out of the power house. This final work is done by extending our bars, so to speak, and carrying them across the bridge over the canal, into what is known as the transformer house. It is here that the current received from the other side of the canal is to be raised in potential, so that it can be sent great distances over small wires without material loss. Meantime we may note that the Niagara Falls Power Co. itself owns more than a square mile around the power house, upon which a large amount of power will be consumed in the near future by manufacturing establishments of all kinds, and that it is already delivering power in large blocks electrically for a great variety of purposes. Special apparatus for this work has been built by the General Electric Co. The current for the production of aluminium is made "direct" by passing through static and rotary transformers, while the Acheson Carborundum process uses the pure alternating current. Besides this, the trolley road from Niagara to Buffalo is already taking part of its power from the Niagara power house by means of rotary transformers. For these and other local uses the company has constructed subways in which to carry the wire across its own territory. These subways are 5 feet 6 inches high, and 3 feet 10 inches wide inside. They are built up with 12 inches of Portland cement and gravel, backed up with about 1 foot of masonry at the bottom and extending about 3 feet up each side. The electric conductors are carried on insulated brackets or insulators arranged upon the pins along the walls. These brackets are 30 feet apart. At the bottom of the conduit manholes are holes for tapping off into side conduits, and along it all runs a track, upon which an inspector can propel himself on a private trolley car if necessary. Thus is distributed locally, the electric power for which the consumer pays the very modest sum of 3*l*. 17*s*. 6*d*. per electrical horse-power per annum delivered on the wire, or about two guineas for a turbine horse-power, a rate which is not to be equalled anywhere, in view of the absolute certainty of the power, free from all annoyance, extra expense, or bother of any kind on the part of the consumer.

It is a curious fact that the proposal to transmit the energy of Niagara long distances over wire should have been regarded with so much doubt and scepticism, and that the courageous backers of the enterprise should have needed time to demonstrate that they were neither knaves nor fools, but simply brave, far-seeing men. We have to-day parallel instances to Niagara in the transmission of oil and natural gas. Oil is delivered in New York City over a line of pipe which is at least 400 miles long, and which has some thirty-five pumping stations en route, the capacity of the line being 30,000

barrels a day. All that oil has first to be gathered from individual wells in the oil region, and delivered to storage tanks with a capacity of 9,000,000 barrels of oil. Chicago, Philadelphia and Baltimore are centres for similar systems of oil pipe running hundreds of miles over hill and dale. As for natural gas, that is to-day sent in similar manner over distances of 120 miles, Chicago being thus supplied from the Indiana gas fields; and the gas has its pressure raised and lowered several times on its way from the gas well to the consumer's tap, just as though it were current from Niagara.

We must not overlook some of the fantastic schemes proposed for transmitting the power of Niagara before electricity was adopted. One of them was to hitch the turbines to a big steel shaft running through New York State from east to west, so that where the shaft passed a town or factory, all you had to do was to hitch on a belt or some gear wheels and thus take off all the power wanted. Not much less expensive was the plan to have a big tube from New York to Chicago with Niagara falls at the centre, and with the Niagara turbines hitched to a monster air compressor which should compress air under 250 lbs. pressure to the square inch in the tube.

So far as actual electrical long-distance transmission from Niagara is concerned, it can only be said to be in the embryonic stage, for the sole reason that for nearly a year past the Power Company has been unable to get into Buffalo, and that not until last year was it able to arrive at acceptable conditions, satisfactory alike to itself and to the city. Work is now being pushed, and by June 1897 power from the Falls will, by contract with the city, be in regular delivery to the local consumption circuits at Buffalo, twenty-two miles away. But the question arises, and has been fiercely discussed, whether it will pay to send the current beyond Buffalo. Recent official investigations have shown that steam power in large bulk under the most favourable conditions, costs to-day in Buffalo 10*l.* per year per horse-power and upwards. Evidently Niagara power starting at 2*l.* on the turbine shaft, or say less than 4*l.* on the line, has a good margin for effective competition with steam in Buffalo.

As to the far-away places, the well known engineers, Prof. E. J. Houston and Mr. A. E. Kennelly, have made a most careful estimate of the distance to which the energy of Niagara could be economically transmitted by electricity. Taking established conditions, and prices that are asked to-day for apparatus, they have shown, to their own satisfaction at least, that even in Albany or anywhere else in the same radius, 330 miles from the Falls, the converted energy of the great cataract could be delivered cheaper than good steam engines on the spot could make steam power with coal at the normal price there of 12*s.* per ton.

What this enterprise at Niagara aims to do is not to monopolise the power but to distribute it; and it makes Niagara, more than it ever was before, common property. After all is said and done, very few people ever see the Falls, and then only for a chance holiday once in a

lifetime; but now the useful energy of the cataract is made cheaply and immediately available, every day in the year, to hundreds and thousands, even millions of people, in an endless variety of ways.

We must not omit from our survey the Erie Canal, in the revival and greater utilisation of which as an important highway of commerce Niagara power is expected to play no mean part. In competition with the steam railway, canals have suffered greatly the last fifty years. In the United States, out of 4468 miles of canal built at a cost of 40,000,000*l.*, about one-half has been abandoned and not much of the rest pays expenses. Yet canals have enormous carrying capacity, and a single boat will hold as much as twenty freight cars. The New York State authorities have agreed to conditions by which Niagara energy can be used to propel the canal boats at the rate of 4*l.* per horse-power per year. Where steam-boat haulage for 242 tons of freight now costs about 6½*d.* a boat mile, it is estimated that electric haulage will cost not to exceed 5¼*d.*; while, with the energy from Niagara at only 4*l.* per horse-power per year, it will cost much less. Some two years ago the first attempt was made in the United States on the Erie Canal, with the canal boat "F. W. Hawley," when the trolley system was used with the motor on the boat, as it is on an electric car, driving the propeller as if it were the car wheels. Another plan is that of hauling the boat from the tow-path, and that is what is now being done with the electric system of Mr. Richard Lamb on the Erie Canal at Tonawanda, near Niagara. Imagine an elevator shaft working lengthwise instead of vertically. There is placed on poles, a heavy fixed cable on which the motor truck rests, and a lighter traction cable is also strung that is taken up and paid out by a sheave, as the motor propels itself along and pulls the canal boat to which it is attached. If the boats come from opposite directions they simply exchange motors, just as they might mules or locomotives, and go on without delay.

On its property at Niagara the Power Company has already begun the development of the new village called Echota, a pretty Indian name which signifies "Place of Refuge." I believe it is Mr. W. D. Howells, our American novelist, who in kindred spirit speaks of the "Repose" of Niagara. It was laid out by Mr. John Bogart, formerly State Engineer, and is intended to embody all that is best in sanitation, lighting and urban comfort. It does not need the eye of faith to see here the beginning of one of the busiest, cleanest, prettiest and healthiest localities in the Union. The working man whose factory is not poisoned by smoke and dust, whose home was designed by distinguished architects, whose streets and parks were laid out by celebrated engineers, and whose leisure is spent within sight and sound of lovely Niagara, has little cause for grumbling at his lot.

The American company has also preempted the great utilisation of the Canadian share of Niagara's energy. The plan for this work proposes the erection of two power houses of a total ultimate capacity of 125,000 horse-power. Each power house is fed by its own canal

and is therefore an independent unit. Owing to the better lay of the land, the tunnels carrying off the water discharged from the turbines on the Canadian side will have lengths respectively of only 300 and 800 feet, thus avoiding the extreme length and cost unavoidable on the American side. With both the Canadian and American plants fully developed, no less than 350,000 horse-power will be available. The stationary engines now in use in New York State represent only 500,000 horse-power. Yet the 350,000 horse-power are but one-twentieth of the 7,000,000 horse-power which Prof. Unwin has estimated the Falls to represent theoretically. If the 350,000 horse-power were estimated at 4*l.* per year per horse-power, and should replace the same amount of steam power at 10*l.*, the annual saving for power in New York State alone would be more than 2,000,000*l.* per year.

Let me by way of conclusion emphasise the truth that this splendid engineering work leaves all the genuine beauty of Niagara untouched. It may even help to conserve the scene as it exists to-day, for the terrific weight and rush of waters over the Horse Shoe Fall is eating it away and breaking its cliff into a series of receding slopes and rapids; so that even a slight diminution of the whelming mass of water will to that extent lessen disruption and decay. Be that so or not so, those of us who are lovers of engineering can now at Niagara gratify that taste in the unpretentious place where some of this vast energy is reclaimed for human use, and then as ever join with those who, not more than ourselves, love natural beauty, and find with them renewed pleasure and delight in the majestic, organ-toned and eternal cataract.

[T. C. M.]

GENERAL MONTHLY MEETING,

Monday, July 6, 1896.

SIR JAMES CRICHTON-BROWNE, M.D. LL.D. F.R.S. Treasurer and Vice-President, in the Chair.

The Right Hon. Lord Windsor,
Herbert Page, Esq. F.R.C.S.
Alfred Suart, Esq.

were elected Members of the Royal Institution.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Lords of the Admiralty—Report of the Astronomer Royal to the Board of Visitors, 1896. 4to.
Appendix to the Nautical Almanac for 1900. 8vo. 1896.
The Governor-General of India—Memoirs, Vol. XXVII. Part 1. 8vo. 1895.
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Report on Transportation Business in the United States at the Eleventh Census, 1890, Part I. Transportation by Land. 4to. 1895.
Report on Vital and Social Statistics in the United States, Part III. Statistics of Deaths. 4to. 1894.
United States Patent Office—Official Gazette, Vol. LXXIV. Nos. 10–13; Vol. LXXV. Nos. 1–4. 8vo. 1896.
Verein zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, 1896, Heft 5. 4to. 1896.
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GENERAL MONTHLY MEETING,

Monday, November 2, 1896.

Sir James Crichton-Browne, M.D. LL.D. F.R.S. Treasurer and Vice-President, in the Chair.

George Cawston, Esq.
J. Broughton Dugdale, Esq. J.P. D.L.
Henry Harben, Esq. J.P.
John H. Usmar, Esq.

were elected Members of the Royal Institution.

The Special Thanks of the Members were returned for the following Donations to the Fund for the Promotion of Experimental Research at Low Temperatures:—

The Proprietors of *The Times*	£100
Dr. Ludwig Mond	50
Professor Dewar	50
Sir Andrew Noble	100

The following reply from the Right Hon. Lord Kelvin to the Address from the Members of the Royal Institution on the occasion of the Jubilee of his appointment to the Chair of Natural Philosophy in the University of Glasgow, was read and ordered to be entered on the Minutes.

"The University, Glasgow.

"For the Address which I have had the honour to receive from the Royal Institution on the occasion of the Jubilee of my Professorship of Natural Philosophy in the University of Glasgow, I desire to express my warmest thanks. I value very highly the great honour which it has conferred on me. The friendly appreciation of my scientific work contained in the address is most gratifying. I feel deeply touched by the great kindness to myself, and the good wishes for my welfare of which it gives expression.

KELVIN.

July 6, 1896."

The Managers reported that at their Meeting held this day, they had elected Professor Augustus D. Waller, M.D. F.R.S. Fullerian Professor of Physiology for three years (the appointment dating from January 18, 1897).

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Secretary of State for India—South Indian Inscriptions. By E. Hultzsch. Vol. II. Part 3. 4to. 1896.
The Lords of the Admiralty—Greenwich Observations for 1893. 4to. 1896.
Greenwich Spectroscopic and Photographic Results for 1893. 4to. 1896.
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Report on Real Estate Mortgages. 4to. 1895.
Abstract of the Eleventh Census, 1890. 8vo. 1896.
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Alphabetical List, Vol. LXXIII. 8vo.
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Fifteenth Annual Report, 1893–94. 4to. 1895.
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Yale University Observatory—Report for 1895–96. 8vo.
Zoological Society of London—Proceedings, 1896, Part 2. 8vo. 1896.

GENERAL MONTHLY MEETING,

Monday, December 7, 1896.

AMES CRICHTON-BROWNE, M.D. LL.D. F.R.S. Treasurer and Vice-President, in the Chair.

The Hon. and Rev. William Byron,
Sir Gervas Powell Glyn, Bart.
Alexander Scott, Esq. M.A. D.Sc.
Mrs. T. B. Sowerby,
Rev. Samuel A. Thompson-Yates, M.A.

ected Members of the Royal Institution.

Special Thanks of the Members were returned for the ng Donation to the Fund for the Promotion of Experimental h at Low Temperatures :—

The Duke of Northumberland, K.G. .. £200

Special Thanks of the Members were returned to Colonel ge Grove for a Bust of his father, the late Sir William Grove, and also to Professor Dewar for a Marble Pedestal for the

following Lecture arrangements were announced :—

CHRISTMAS LECTURES.

SSOR SILVANUS P. THOMPSON, D.Sc. F.R.S. *M.R.I.* Six Lectures to a Juvenile Auditory) on LIGHT, VISIBLE AND INVISIBLE. On (*Tuesday*), Dec. 31, 1896; Jan. 2, 5, 7, 9, 1897.

SOR AUGUSTUS D. WALLER, M.D. F.R.S. Fullerian Professor of Phy- R.I. Twelve Lectures on ANIMAL ELECTRICITY. On *Tuesdays*, Jan. 19, 2, 9, 16, 23, March 2, 9, 16, 23, 30, April 6.

SOR HENRY A. MIERS, M.A. F.R.S. Three Lectures on SOME SECRETS ALS. On *Thursdays*, Jan. 21, 28, Feb. 4.

GREGORY, Esq. D.Sc. F.G.S. of the British Museum (Natural History). ctures on THE PROBLEMS OF ARCTIC GEOLOGY. On *Thursdays*, Feb. 11,

SOR PERCY GARDNER, Litt.D. F.S.A. Professor of Classical Archæology n the University of Oxford. Three Lectures on GREEK HISTORY AND MONUMENTS. On *Thursdays*, March 4, 11, 18.

SOR W. BOYD DAWKINS, M.A. F.R.S. F.S.A. F.G.S. Three Lectures RELATION OF GEOLOGY TO HISTORY. 1. The Incoming of Man. 2. The of History in Britain. 3. Roman Britain. On *Thursdays*, March 25,

CARL ARMBRUSTER, Esq. Three Lectures on NEGLECTED ITALIAN AND FRENCH COMPOSERS (with Musical Illustrations). On *Saturdays*, Jan. 23, 30, Feb. 6.

WALTER FREWEN LORD, Esq. Three Lectures on THE GROWTH OF THE MEDITERRANEAN ROUTE TO THE EAST. On *Saturdays*, Feb. 13, 20, 27.

THE RIGHT HON. LORD RAYLEIGH, M.A. D.C.L. LL.D. F.R.S. *M.R.I.* Professor of Natural Philosophy, R.I. Six Lectures on ELECTRICITY AND ELECTRICAL VIBRATIONS. On *Saturdays*, March 6, 13, 20, 27, April 3, 10.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

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Royal Institution of Great Britain.

WEEKLY EVENING MEETING,

Friday, January 29, 1897.

[Sir] James Crichton-Browne, M.D. LL.D. F.R.S. Treasurer and Vice-President, in the Chair.

Professor Jagadis Chunder Bose, M.A. D.Sc.
Professor of Physics in the Presidency College, Calcutta.

[Elec]tro-Magnetic Radiation and the Polarisation of the Electric Ray.

[The] great work of Hertz in verifying the anticipations of Maxwell [has] been followed in this country by many important investigations [on E]lectric Waves. The Royal Institution witnessed the repetition of [some] of the brilliant experiments of Professors Fitzgerald and Lodge. [My i]nterest in the subject, and inspiration for work, are to a great [extent] derived from the memorable addresses delivered in this hall, [and I] am glad to have an opportunity to lay before you, at this very place, an account of some work I have been able to carry out.

[A]s the subject of ether waves produced by periodic electric disturb[ances] is to be dealt with in this lecture, a few models exhibiting the [produc]tion of material waves by periodic mechanical disturbances may [be of] interest. A pendulum swings backwards and forwards at regular [interva]ls of time; so does an elastic spring when bent and suddenly [releas]ed. These periodic strokes produce waves in the surrounding [mediu]m; the aerial waves striking the ear may, under certain condi[tions,] produce the sensation of sound. The necessary condition for [audibi]lity is, that the frequency of vibration should lie within certain [limits].

[A]s the air is invisible, we cannot see the waves that are produced. [Here] is a model in which the medium is thrown into visible waves by [the ac]tion of periodic disturbances. The beaded string representing [the me]dium is connected at its lower end with a revolving electric [motor]. The rotation of the motor is periodic; observe how the [period]ic rotation throws the string into wave forms; how these waves [carry] energy from the source to a distant place; how a suitable re[ceiver], a bell for example, is made to respond. I now produce quicker [vibratio]ns by sending a stronger current through the motor; the [freque]ncy or pitch is raised, and the waves formed are seen to become [shorte]r. By means of the attached counter, the different frequencies [are d]etermined.

[H]ere is a second model, a spiral spring, attached to which is a [s]tring. As the string is pulled, the spring is strained more and [more,] till the thread suddenly breaks. The spring, suddenly released, [begins] to oscillate up and down. Electric vibration is produced in

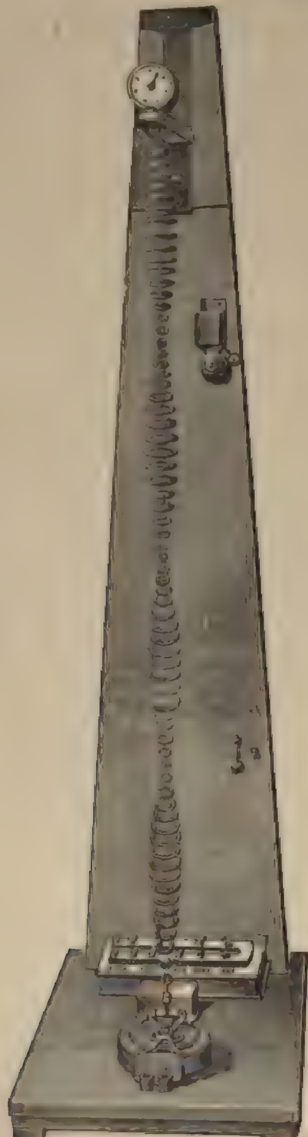

FIG. 1.—Mechanical Wave Apparatus.

(The current regulating the speed of rotation is varied by an interposed rheostat. The counter is at the top.)

a somewhat similar way. If two metallic spheres be strongly charged with opposite electrifications, the medium is electrically strained, and when this strain is suddenly removed by a discharge, waves are produced in the medium. The discharge is oscillatory, consisting of backward and forward rushes of electricity; positive electricity flowing now in one direction, and immediately afterwards in an opposite direction. These rapid alternate flows, giving rise to ether vibration, may be illustrated by a modification of the well-known Cartesian diver experiment. By means of a bulb and connecting tube, alternate compression and rarefaction may be produced in the cylinder, attended with alternate rushes of air-currents through the connecting tube. These give rise to oscillation of the immersed ball.

By oscillatory electric discharge, waves are produced in the ether. To produce oscillatory discharge, Hertz used plates or rods with sparking balls at the ends. He found that the sparks ceased to be oscillatory as soon as the surface of the sparking balls got roughened; there was then a leak of electricity, and no sudden discharge. The balls had to be taken out every now and then for repolishing, and the process was tedious in the extreme. Prof. Lodge made the important discovery that if two side balls were made to spark into an interposed third ball, the oscillatory nature of the discharge was not affected

to so great an extent by a change in the nature of the surface. But even here the disintegration of the sparking surface produced by a torrent of sparks soon puts an end to oscillation. I found this difficulty removed to a great extent by making the balls of platinum, which resists the disintegrating action. I also found that it was not at all necessary to have a series of useless sparks, which ultimately spoils the efficiency of the radiator and makes its action uncertain. A flash of radiation for an experiment is obtained from a single spark, and for a series of experiments one does not require more than fifty or a hundred sparks, which do not in any way affect the radiator. As an electric generator I use a small and modified form of Ruhmkorff's coil, actuated by a single storage cell. A spark is produced by a short contact and subsequent break of a tapping key. With these modifications one of the most troublesome sources of uncertainty is removed. The coil and the cell are inclosed in a small double-walled metallic box, with a tube for the passage of the electric beam. The magnetic variation due to the make and break of the primary of the Ruhmkorff's coil, disturbs the receiver. This difficulty is removed by making the inner box of soft iron, which acts as a magnetic screen.

A few words may here be said about the necessary conditions to be kept in view in making an electric wave apparatus an instrument of precision. If one merely wishes to produce response in a receiver at a distance, the more energetic the vibration is, the more likely it is to overcome obstacles. The waves may with advantage be of large size, as they possess very great penetrative power. The surface or the depth of the sensitive layer in the receiver may be extended, for if one part of it does not respond another part will. But for experimental investigations the conditions to be fulfilled are quite different. Too great an intensity of radiation makes it almost impossible to prevent the disturbance due to stray radiation. As the waves are invisible, it is difficult to know through what unguarded points they are escaping. They may be reflected from the walls of the room or the person of the experimenter, and falling on the receiver disturb it.

The radiation falling on any portion of the receiving circuit—the leading wires or the galvanometer—disturbs the delicate receiver. It is extremely difficult to shield the receiving circuit from the disturbing action of stray radiation. These difficulties were, however, successfully removed by the use of short electric waves. With these, it is not at all necessary to take special precautions to shield either the galvanometer or the leading wires, the sensitive layer in the receiver alone being affected by the radiation. The bare leading wires may be exposed in close proximity to the source of radiation, and yet no disturbance is produced.

For experimental investigations it is also necessary to have a narrow pencil of electric radiation, and this is very difficult to obtain, unless waves of very short length are used. With large waves diverging in all directions and curling round corners, all attempt at accurate work is futile. For angular measurements it is necessary to direct

the electric beam in the given direction along narrow tubes, and receive it in another tube in which is placed the receiver. The waves experience great difficulty in passing through narrow apertures, and there are other troubles from the interference of direct and reflected waves. These difficulties were ultimately overcome by making suitable radiators emitting very short waves; the three radiators here exhibited, give rise to waves which are approximately ¼ inch, ½ inch and 1 inch in length. The intensity of emitted radiation is moderately strong, and this is an advantage in many cases. It sometimes becomes necessary to have a greater intensity without the attendant trouble inseparable from too long waves. I have been able to secure this by making a new radiator, where the oscillatory discharge takes place between two circular plates and an interposed platinum ball. The sparking takes place at right angles to the circular plates. The intensity of radiation is by this expedient very greatly increased. The parallel pencil of electric radiation, used in many of the experiments to be described below, is only about half an inch in diameter. The production of such a narrow pencil became absolutely necessary for a certain class of investigations. Merely qualitative results for reflection or refraction may no doubt be obtained with gigantic mirrors or prisms, but when we come to study the phenomena of polarisation as exhibited by crystals, Nature imposes a limit, and this limitation of the size of the crystals has to be accepted in conducting any investigation on their polarising properties.

The greatest drawback, however, in conducting experimental investigations with electric radiation arises from the difficulty of constructing a satisfactory receiver for detecting these waves. For this purpose I at first used the original form of coherer made of metallic filings as devised by Professor Lodge. It is a very delicate detector for electric radiation, but unfortunately I found its indications often to be extremely capricious.

The conditions for a satisfactory receiver are the following:—

(1) Its indications should always be reliable.

(2) Its sensitiveness should remain fairly uniform during the experiment.

(3) The sensibility should be capable of variation, to suit different experiments.

(4) The receiver should be of small size, and preferably linear, to enable angular measurements to be taken with accuracy.

These conditions seemed at first almost impossible to be attained. The coherer sometimes would be so abnormally sensitive that it would react without any apparent cause. At other times, when acting in an admirable manner, the sensitiveness would suddenly disappear at the most tantalising moment. It was a most dreary experience when the radiator and the receiver failed by turns, and it was impossible to find out which was really at fault.

From a series of experiments carried out to find the causes which may affect prejudicially the action of the receiver, I was led to sup-

pose that the uncertainty in the response of the receiver is probably due to the following:—

(1) Some of the particles of the coherer might be too loosely applied against each other, whereas others, on the contrary, might be jammed together, preventing proper response.

(2) The loss of sensibility might also be due to the fatigue produced on the contact surfaces by the prolonged action of radiation.

(3) As the radiation was almost entirely absorbed by the outermost layer, the inner mass, which acted as a short circuit, was not necessary.

For these reasons I modified the receiver into a spiral-spring form. Fine metallic wires (generally steel, occasionally others, or a combination of different metals) were wound in narrow spirals and laid in a single layer on a groove cut in ebonite, so that the spirals could roll on a smooth surface. The ridges of the contiguous spirals made numerous and well-defined contacts, about one thousand in number. The useless conducting mass was thus abolished, and the resistance of the receiving circuit almost entirely concentrated at the sensitive contact surface exposed to radiation. If any change of resistance, however slight, took place at the sensitive layers, the galvanometer in circuit would show strong indications. The pressure throughout the mass was made uniform as each spring transmitted the pressure to the next. When the contact surfaces had too long been acted on, fresh surfaces could easily be brought into contact by the simultaneous rolling of all the spirals.

The sensibility of the receiver to a given radiation, I found, depends (1) on the pressure to which the spirals are subjected, and (2) on the E.M.F. acting on the circuit. The pressure on the spirals may be adjusted, as will be described later on, by means of a fine screw. The E.M.F. is varied by a potentiometer-slide arrangement. This is a matter of great importance, as I often found a receiver, otherwise in good condition, failing to respond when the E.M.F. varied slightly from the proper value. The receiver, when subjected to radiation, undergoes exhaustion. The sensibility can, however, be maintained fairly uniform by slightly varying the E.M.F. to keep pace with the fatigue produced.

The receiving circuit thus consists of a spiral-spring coherer, in series with a voltaic cell and a dead-beat galvanometer. The receiver is made by cutting a narrow groove in a rectangular piece of ebonite, and filling the groove with bits of coiled spirals arranged side by side in a single layer. The spirals are prevented from falling by a glass slide in front. They are placed between two pieces of brass, of which the upper one is sliding and the lower one fixed. These two pieces are in connection with two projecting metallic rods, which serve as electrodes. An electric current enters along the breadth of the top spiral and leaves by the lowest spiral, having to traverse the intermediate spirals along the numerous points of contact. When electric radiation is absorbed by the sensitive sur-

face, there is a sudden diminution of the resistance, and the galvanometer spot is violently deflected.

By means of a very fine screw the upper sliding piece can be gently pushed in or out. In this way the spirals may be very gradually compressed, and the resistance of the receiver diminished. The galvanometer spot can thus easily be brought to any convenient position on the scale. When electric radiation falls on the sensitive surface the spot is deflected. By a slight unscrewing the resistance is increased, and the spot made to return to its old position. The receiver is thus re-sensitised for the next experiment.

The receiver thus constructed is perfectly reliable ; the sensibility can be widely varied to suit different experiments, and this sensibility maintained fairly uniform. When necessary, the sensitiveness can be exalted to almost any extent, and it is thus possible to carry out some of the most delicate experiments (specially on polarisation) with certainty.

The main difficulties being thus removed, I attempted to construct a complete electric wave apparatus, which would be portable, with which all the experiments on electric radiation could be carried out with almost as great an ease and certainty as corresponding experiments on light, and which would enable one to obtain even quantitative results with fair accuracy.

The complete apparatus is here exhibited ; all its different parts, including the galvanometer, and all the accessories for reflection, refraction, polarisation, and other experiments, are contained in a small case only 2 feet in length, 1 foot in height and 1 foot in breadth. The apparatus can be set up in a few minutes, the various adjustments requiring only a short time.

The radiating apparatus is 6 by 5 by 3 inches, the size of a small lantern. It contains the coil and a small storage cell ; the radiator tube is closed with a thin plate of ebonite to prevent deposit of dust on the radiator. One charge of the cell stores enough energy for experiments to be carried out for nearly a month. It is always ready for use and requires very little attention. A flash of radiation for an experiment is produced by a single tap and break of the interrupting key.

The radiating apparatus and the receiver are mounted on stands sliding in an optical bench. Experiments are carried out with divergent or parallel beams of electric radiation. To obtain a parallel beam, a lens of sulphur or glass is mounted in a tube. Suitable lenses can be constructed from the accurate determination, which I have been able to make, of the indices of refraction of various substances for the electric ray, by a method which will be described later on. This lens-tube fits on the radiator-tube, and is stopped by a guide when the oscillatory spark is at the principal focus of the lens. The radiator-tube is further provided with a series of diaphragms by which the amount of radiation may be varied.

For experiments requiring angular measurement, a spectrometer-

circle is mounted on one of the sliding stands. The spectrometer carries a circular platform, on which the various reflectors, refractors, &c., are placed. The platform carries an index, and can rotate independently of the circle on which it is mounted. The receiver is carried on a radial arm (provided with an index), and points to the centre of the circle. An observing telescope may also be used with a glass objective, and a linear receiver at the focus.

I shall now exhibit some of the principal experiments on electric radiation.

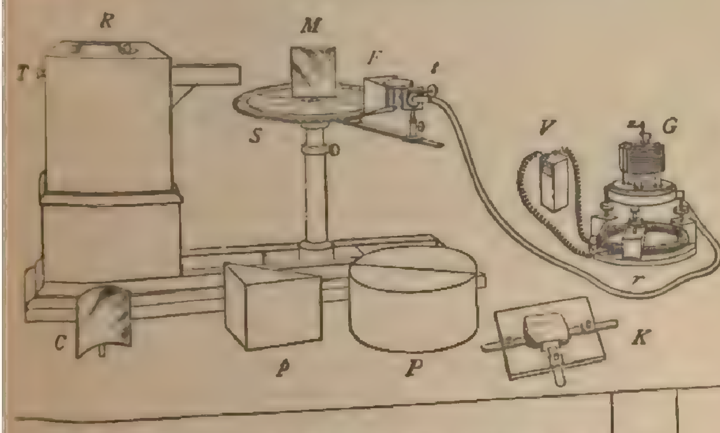

FIG. 2.—Arrangement of the Apparatus. One-sixth nat. size.

R, radiator; T, tapping key; S, spectrometer-circle; M, plane mirror; C, cylindrical mirror; *p*, totally reflecting prism; P, semi-cylinders; K, crystal-holder; F, collecting funnel attached to the spiral spring receiver; *t*, tangent screw, by which the receiver is rotated; V, voltaic cell; *r*, circular rheostat; G, galvanometer.

Selective Absorption.

I arrange the radiation apparatus so that a parallel beam of electric radiation proceeding from the lantern falls on the receiver placed opposite; the receiver responds energetically, the light-spot from the galvanometer being swept violently across the screen. I now interpose various substances to find out which of them allow the radiation to pass through and which do not. A piece of brick, or a block of pitch, is thus seen to be quite transparent, whereas a thick stratum of water is almost opaque. A substance is said to be coloured when it allows light of one kind to pass through, but absorbs light of a different kind. A block of pitch is opaque to visible light, but transparent to electric radiation; whereas water, which is transparent to light, is opaque to electric radiation. These substances exhibit selective absorption, and are therefore coloured.

There is an interesting speculation in reference to the possibility of the sun emitting electric radiation. No such radiation has yet been detected in sunlight. It may be that the electric rays are absorbed by the solar or the terrestrial atmosphere. As regards the latter supposition, the experiment which I am able to exhibit on the transparency of liquid air may be of interest. Professor Dewar has kindly lent me this large bulb full of liquid air, which is equivalent to a great thickness of ordinary air. This thick stratum allows the radiation to pass through with the greatest facility, proving the high transparency of the liquid air.

Verification of the Laws of Reflection.

A small plane metallic mirror is mounted on the platform of the spectrometer-circle. The receiver is mounted on a radial arm. The law of reflection is easily verified in the usual way. The second mirror, which is curved, forms an invisible image of the source of radiation. As I slowly rotate the cylindrical mirror, the invisible image moves through space; now it falls on the receiver, and there is a strong response produced in the receiver.

Refraction.

Deviation of the electric ray by a prism may be shown by a prism made of sulphur or ebonite. More interesting is the phenomenon of total reflection. A pair of totally-reflecting prisms may be obtained by cutting a cube of glass, which may be an ordinary paper-weight, across a diagonal. The critical angle of a specimen of glass I found to be 29°, and a right-angled isosceles prism of this material produces total reflection in a very efficient manner. When the receiver is placed opposite the radiator, and the prism interposed with one of its faces perpendicular to the electric beam, there is not the slightest action on the receiver. On turning the receiver through 90°, the receiver responds to the totally-reflected ray.

Opacity due to multiple refraction and reflection, analogous to the opacity of powdered glass to light, is shown by filling a long trough with irregularly-shaped pieces of pitch, and interposing it between the radiator and the receiver. The electric ray is unable to pass through the heterogeneous media, owing to the multiplicity of refractions and reflections, and the receiver remains unaffected. But on restoring partial homogeneity by pouring in kerosene, which has about the same refractive index as pitch, the radiation is easily transmitted.

Determination of the Index of Refraction.

Accurate determination of the indices of refraction becomes important when lenses have to be constructed for rendering the electric beam parallel. The index for electric radiation is often very different

from the optical index, and the focal distance of a glass lens for light gives no clue to its focal distance for electric radiation. I found, for example, the index of refraction of a specimen of glass to be 2·04, whereas the index of the same specimen for sodium light is only 1·53.

There are again many substances, like the various rocks, wood, coal-tar, and others, whose indices cannot be determined owing to their opacity to light. These substances are, however, transparent to electric radiation, and it is therefore possible to determine their electric indices. For the determination of the index, the prism-method is not very suitable. I found the following method, of which I shall exhibit the optical counterpart, to yield good results. When light passes from a dense to a light medium, then, at a certain critical angle, the light is totally reflected, and from the critical angle the index can be determined. I have here a cylindrical trough filled with water. Two glass plates inclosing a parallel air-film are suspended vertically across the diameter of the cylinder, dividing the cylinder into two halves. The cylinder, mounted on a graduated

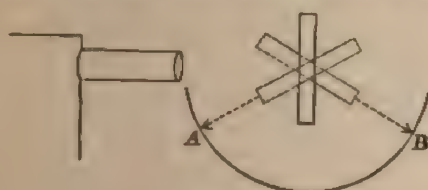

FIG. 3.

(The dotted lines show the two positions of the air-film for total reflection.)

circle, is adjusted in front of an illuminated slit, an image of the slit being cast by the water-cylinder on the screen. The divergent beam from the slit, rendered nearly parallel by the first half of the cylinder, is incident on the air-film, and is then focussed by the second half of the cylinder. As the cylinder is slowly rotated, the angle of incidence at the air-film is gradually increased, but the image on the screen remains fixed. On continuing the rotation you observe the almost sudden extinction of the image. I say almost, because the light is not monochromatic, and the different components of white light undergo total reflection in succession. Just before total extinction the image you observe is reddish in colour, the violet and the blue lights being already reflected. On continuing the rotation the image is completely extinguished. Rotation of the cylinder in an opposite direction gives another reading for total reflection, and the difference of the two readings is evidently equal to twice the critical angle.

In a similar way I have been able to determine the indices of refraction of various substances, both solid and liquid, for electric radiation. In the case of solids, two semi-cylinders, separated by a

suitable parallel air-space, are placed on the spectrometer-circle, the receiver being placed opposite the radiator. The trouble of following the deviated ray is thus obviated. The index of refraction of glass I found to be 2·04; that of commercial sulphur is 1·73.

Double Refraction and Polarisation.

I now proceed to demonstrate some of the principal phenomena of polarisation, especially in reference to the polarisation produced by crystals and other substances, and by dielectrics when subjected to molecular stress due to pressure or unequal heating.

As the wave-length of electric radiation is many thousand times the wave-length of light, there is a misgiving as to whether it would

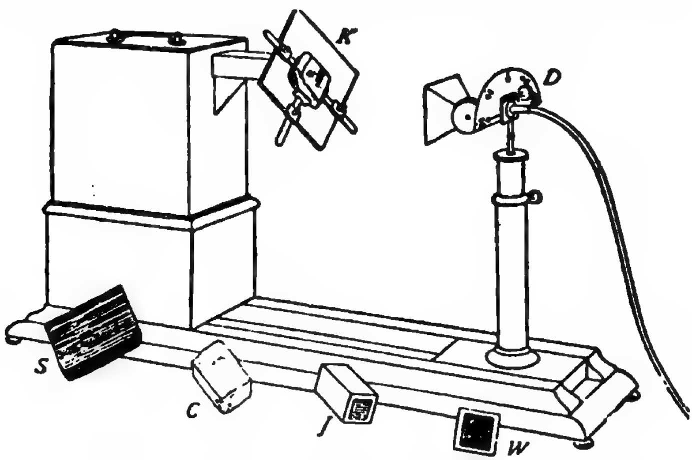

Fig. 4.—Polarisation Apparatus.

K, crystal-holder; S, a piece of stratified rock; C, a crystal; J, jute polariser; W, wire-grating polariser; D, vertical graduated disc, by which the rotation is measured.

be possible to exhibit polarisation effects with crystals of ordinary size. I hope to be able to demonstrate that such a misgiving is groundless.

A beam of ordinary light incident on a crystal of Iceland spar is generally bifurcated after transmission, and the two emergent beams are found polarised in planes at right angles to each other. The usual optical method of detecting the bi-refringent action of crystal, is to interpose it between the crossed polariser and analyser. The interposition of the crystal generally brightens the dark field. This is the so-called depolarisation effect, and is a delicate test for double-refracting substances. There is, however, no depolarisation when the

principal plane of the crystal coincides with the polarisation planes of either the polariser or the analyser. The field also remains dark when the optic axis of the crystal is parallel to the incident ray.

A similar method is adopted for experimenting with polarised electric radiation.

The spectrometer-circle is removed from the optical bench, and an ordinary stand for mounting the receiver substituted. By fitting the lens-tube, the electric beam is made parallel. At the end of the tube may be fixed either the grating polariser or the jute or serpentine polarisers, to be subsequently described.

The receiver fitted with the analyser is adjusted by a tangent screw, the rotation of the analyser being measured by means of an index and a graduated vertical disc.

The polarising gratings may be made, according to Hertz, by winding copper wires, parallel, round square frames. The polarisation apparatus is, however, so extremely delicate, that unless all the wires are strictly parallel, and the gratings *exactly* crossed, there is always a resolved component of radiation which acts on the sensitive receiver. It is a very difficult and tedious operation to cross the gratings. I have found it to be a better plan to take two thick square plates of copper of the same size, and, placing one over the other, cut a series of slits (which stop short of the edges) parallel to one of the edges. One of these square pieces serves as a polariser, and the other as an analyser. When the two square pieces are adjusted, face to face, with coincident edges, the gratings must either be parallel or exactly crossed. Such accurate adjustments make it possible to carry out some of the most delicate experiments.

The radiator-tube, with the lens and the attached polariser, is capable of rotation. The emergent beam may thus be polarised in a vertical or a horizontal plane. The analyser fitted on to the receiver may also be rotated. The gratings may thus be adjusted in two positions.

(1) Parallel position.
(2) Crossed position.

In the first position the radiation is transmitted through both the gratings, falls on the sensitive surface, and the galvanometer responds. The field is then said to be bright. In the second position the radiation is extinguished by the crossed gratings, the galvanometer remains unaffected, and the field is said to be dark. But in interposing a double-refracting substance in certain positions between the crossed gratings, the field is partially restored, and the galvanometer-spot sweeps across the scale.

I have now the analyser and the polariser exactly crossed, and there is not the slightest action on the receiver. Observe the great sensitiveness of the arrangement; I turn the polariser very slightly from the crossed position, and the galvanometer-spot is violently deflected.

I now readjust the gratings in a crossed position. I have in my hand a large block of the crystal beryl; it is perfectly opaque to light. I now hold the crystal with its principal plane inclined at 45° between the crossed gratings, and the galvanometer-spot, hitherto quiescent, sweeps across the scale. It is very curious to observe the restoration of the extinguished field of electric radiation, itself invisible, by the interposition of what appears to the eye to be a perfectly opaque block of crystal. If the crystal is slowly rotated, there is no action on the receiver when the principal plane of the crystal is parallel to either the polariser or the analyser. Thus, during one complete rotation there are four positions of the crystal when no depolarisation effect is produced.

Rotation of the crystal, when held with its optic axis parallel to the incident ray, produces no action. The field remains dark.

Here is another large crystal, idocrase, belonging to the orthorhombic system, which shows the same action. It is not at all necessary to have large crystals; a piece of calc-spar, taken out of an optical instrument, will polarise the electric ray. But the effect produced by the crystal epidote seems extraordinary. I have here a piece with a thickness of only ·7 cm.—a fraction of the wave-length of the electric radiation—and yet observe how strong is its depolarising effect.

I subjoin a representative list of crystals belonging to the different systems, which would be found to produce double refraction of the electric ray.

Tetragonal System.—Idocrase, scapolite.

Orthorhombic System.—Barytes, celestine, cryolite, andalusite, hypersthene.

Hexagonal System.—Calcite, apatite, quartz, beryl, tourmaline.

Monoclinic System.—Selenite, orthoclase, epidote.

Triclinic System.—Labradorite, microcline, amblygonite.

Double Refraction produced by a Strained Dielectric.

Effect due to Pressure.—A piece of glass, when strongly compressed, becomes double refracting for light. An analogous experiment may be shown with electric radiation. Instead of producing pressure artificially, it seemed to me that stratified rocks, which, from the nature of their formation, were subjected to great pressure, would serve well for my experiment. Here is a piece of slate about an inch in thickness. I interpose this piece with the plane of stratification inclined at 45°, and the spot of light flies off the scale. I now carefully rotate the piece of slate; there is no depolarisation effect when the plane of stratification is parallel to either the polariser or the analyser. Thus the existence of strain inside an opaque mass can easily be detected, and what is more, the directions of maximum and minimum pressures can be determined with great exactitude.

Effect due to Strains in Cooling.—An effect similar to that pro-

luced by unannealed glass may be shown by this piece of solid paraffin, which was cast in a mould, and chilled unequally by a freezing mixture. One of these blocks was cast two years ago, and it has still retained its unannealed property. This effect may even be shown without any special preparation. Pieces of glass or ebonite, too, are often found sufficiently strained to exhibit double refraction.

Phenomena of Double Absorption.

Being desirous of making a crystal polariser, I naturally turned to tourmaline, but was disappointed to find it utterly unsuitable as a polariser. There is a difference in transparency in directions parallel and perpendicular to the length, but even a considerable thickness of the crystal does not completely absorb one of the two rays. Because visible light is polarised by absorption by tourmaline, it does not follow that all kinds of radiation would be so polarised. The failure of tourmaline to polarise the Röntgen rays is therefore not unexpected, supposing such rays to be capable of polarisation.

It was a long time before I could discover crystals which acted as electric tourmalines. In the meanwhile I found many natural substances which produced polarisation by selective unilateral absorption. For example, I found locks of human hair to polarise the electric ray. I have here two bundles of hair; I interpose one at 45°, and you observe the depolarisation effect. The darker specimen seems to be the more efficient. Turning to other substances more easily accessible, I found vegetable fibres to be good polarisers. Among these may be mentioned the fibres of aloes (*Agave*), rhea (*Boehmeria nivea*), pine-apple (*Ananas sativus*), plantain (*Musa paradisiaca*). Common jute (*Corchorus capsularis*) exhibits the property of polarisation in a very marked degree. I cut fibres of this material about 3 cm. in length, and built with them a cell with all the fibres parallel. I subjected this cell to a strong pressure under a press. I thus obtained a compact cell 3 cm. by 3 cm. in area, and 5 cm. in thickness. This was mounted in a metallic case, with two openings 2 cm. by 2 cm. on opposite sides for the passage of radiation. This cell absorbs vibrations parallel to the length of the fibres, and transmits those perpendicular to the length. Two such cells could thus be used, one as a polariser and the other as an analyser.

Turning to crystals, I found a large number of them exhibiting selective absorption in one direction. Of these nemalite and crysotile exhibit this property to a remarkable extent. Nemalite is a fibrous variety of brucite; crysotile being a variety of serpentine. The direction of absorption in these cases is parallel to the length, the direction of transmission being perpendicular to the length. I have here a piece of crysotile, only one inch in thickness. I adjust the polariser and the analyser parallel, and interpose the crysotile with its length parallel to the electric vibration. You observe that

the radiation is completely absorbed, none being transmitted. I now hold the piece with its length perpendicular to the electric vibration; the radiation is now copiously transmitted. Crysotile is thus seen to act as a perfect electric tourmaline.

Anisotropic Conductivity exhibited by certain Polarising Substances.

In a polarising grating, the electric vibrations perpendicular to the bars of the grating are alone transmitted, the vibrations parallel to the grating being absorbed or reflected. In a grating we have a structure which is not isotropic, for the electric conductivity parallel to the bars is very great, whereas the conductivity across the bars (owing to the interruptions due to spaces) is almost nothing. We may, therefore, expect electric vibrations parallel to the bars to produce local induction currents, which would ultimately be dissipated as heat. There would thus be no transmission of vibrations parallel to the grating, all such vibrations being absorbed. But owing to the break of metallic continuity, no induction current can take place across the grating; the vibrations in this direction are, therefore, transmitted. From these considerations we see how non-polarised vibrations falling on a grating would have the vibration components parallel to the direction of maximum conductivity absorbed, and those in the direction of least conductivity transmitted in a polarised condition.

I have shown that nemalite and crysotile polarise by selective absorption, the vibration perpendicular to their length being transmitted, and those parallel to their length being absorbed. Bearing in mind the relation between the double conductivity and double absorption, as exhibited by gratings, I was led to investigate whether the directions of the greatest and least absorptions in nemalite and crysotile were also the directions of maximum and minimum conductivities respectively. I found the conductivity of a specimen of nemalite in the direction of absorption to be about fourteen times the conductivity in the direction of transmission. In crysotile, too, the directions of the greatest and least absorption were also the directions of maximum and minimum conductivities.

It must, however, be noted that the substances mentioned above are bad conductors, and the difference of conductivity in the two directions is not anything like what we get in polarising gratings. A thin layer of nemalite or crysotile will, therefore, be unable to produce complete polarisation. But by the cumulative effect of many such layers in a thick piece, the vibrations which are perpendicular to the direction of maximum conductivity are alone transmitted, the emergent beam being thus completely polarised.

A double-conducting structure will thus be seen to act as a polariser. I have here an artificial electric tourmaline, made of a bundle of parallel capillary glass fibres. The capillaries have been filled with dilute copper sulphate solution. A simple, and certainly the most handy,

polariser is one's outstretched fingers. I interpose my fingers at 45° between the crossed polariser and the analyser, and you observe the immediate restoration of the extinguished field of radiation. The double-conducting nature of the structure is here quite evident.

While repeating these experiments I happened to have by me this old copy of 'Bradshaw,' and it struck me that here was an excellent double-conducting structure which ought to polarise the electric ray. For looking at the edge of the book we see the paper continuous in one direction along the pages, whereas this continuity is broken across the pages by the interposed air-films. I shall now demonstrate the extraordinary efficiency of this book as an electric polariser. I hold it at 45° between the crossed gratings, and you observe the strong depolarisation effect produced. I now arrange the polariser and the analyser in a parallel position, and interpose the 'Bradshaw' with its edge parallel to the electric vibration; there is not the slightest action in the receiver, the book held in this particular direction being perfectly opaque to electric radiation. But on turning it round through 90°, the 'Bradshaw,' usually so opaque, becomes quite transparent, as is indicated by the violent deflection of the galvanometer-spot of light. An ordinary book is thus seen to act as a perfect polariser of the electric ray; the vibrations parallel to the pages are completely absorbed, and those at right angles transmitted in a perfectly polarised condition.

The electric radiation is thus seen to be reflected, refracted and polarised just in the same way as light is reflected, refracted and polarised. The two phenomena are identical. The anticipations of Maxwell have thus been verified by the great work of Hertz and his successors.

By pressing the key of this radiation apparatus I am able to produce ether vibrations, 30,000 millions in one second. A second stop in connection with another apparatus will give rise to a different vibration. Imagine a large electric organ provided with a very large number of stops, each key giving rise to a particular ether note. Imagine the lowest key producing one vibration in a second. We should then get a gigantic ether wave 186,000 miles long Let the next key give rise to two vibrations in a second, and let each succeeding key produce higher and higher notes. Imagine an unseen hand pressing the different keys in rapid succession. The ether notes will thus rise in frequency from one vibration in a second, to tens, to hundreds, to thousands, to hundreds of thousands, to millions, to millions of millions. While the ethereal sea in which we are all immersed is being thus agitated by these multitudinous waves, we shall remain entirely unaffected, for we possess no organs of perception to respond to these waves. As the ether note rises still higher in pitch, we shall for a brief moment perceive a sensation of warmth. As the note still rises higher, our eye will begin to be affected, a red glimmer of light will be the first to make its appearance. From this point the few colours we see are comprised within a single octave

of vibration—from about 400 to 800 billions in one second. As the frequency of vibration rises still higher, our organs of perception fail us completely; a great gap in our consciousness obliterates the rest. The brief flash of light is succeeded by unbroken darkness.

These great regions of invisible lights are now being slowly and patiently explored. In time the great gaps which now exist will be filled up, and light-gleams, visible and invisible, will be found merging one into the other in unbroken sequence.

Before I conclude I may be permitted to express my sincere thanks to the managers of the Royal Institution for according me the privilege of addressing you this evening. I cannot sufficiently express my gratefulness for all the kindness I have received in this country. When the managers of this Institution, which has done so much to advance the cause of Science and Arts, invited me here, I felt that the scope of this great Institution was not merely confined to these shores, but embraced other countries, even the most distant. The land from which I come did at one time strive to extend human knowledge, but that was many centuries ago; a dark age has since supervened. It is now the privilege of the West to lead in this work. I would fain hope, and I am sure I am echoing your sentiments, that a time may come when the East, too, will take her part in this glorious undertaking; and that at no distant time it shall neither be the West nor the East, but both the East and the West, that will work together, each taking her share in extending the boundaries of knowledge, and bringing out the manifold blessings that follow in its train.

[J. C. B.]

GENERAL MONTHLY MEETING.

Monday, February 1, 1897.

SIR JAMES CRICHTON-BROWNE, M.D. LL.D. F.R.S. Treasurer and Vice-President, in the Chair.

Alfred Louis Cohen, Esq.
Mrs. Delaforce.
Sir Charles A. Elliott, K.C.S.I. LL.D.
John Lawson Johnston, Esq.
Dr. A. Liebmann,
T. George Longstaff, Esq.
Howard Marsh, Esq. F.R.C.S.
Rev. Edward G. C. Parr, M.A.
Charles Rose, Esq.
Edward P. Thompson, Esq.

were elected Members of the Royal Institution.

The Special Thanks of the Members were returned for the following Donation to the Fund for the Promotion of Experimental Research at Low Temperatures:—

J. Wolfe Barry, Esq. C.B. ..	£25
Sir Frederick Abel, Bart. K.C.B.	£50

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

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WEEKLY EVENING MEETING,

Friday, February, 5, 1897.

The Right Hon. Lord Halsbury, M.A. D.C.L. F.R.S. Lord Chancellor, in the Chair.

The Right Rev. The Lord Bishop of London.

The Picturesque in History.

It is an old controversy whether history is a branch of literature or a branch of science; but there is no reason why the controversy should ever be decided. A book is written; it must take its chance. It is cast upon the world to exercise such influence as it can, to teach or to attract, to mould thought or to create interest, to solve questions or to suggest them. There is always one consoling reflection for authors, which ought to save them from disappointment. The deeper the impression which a book produces, the smaller is the circle of its readers likely to be. The general public likes to take its journeys by easy stages, and will not be carried too far all at once. Only a select few will be ready to undertake a serious expedition; but they are the explorers, and through their efforts knowledge will ultimately grow. When pioneers have entered upon a new field, it takes some time before the communications are made which make travelling easy. Meanwhile, ideas and notions float disjointedly into the general stock of knowledge, and affect public opinion insensibly in various ways. Knowledge of the past is of value as it affords a background against which men view the present. It is of some value, as likely to affect men's judgment of what is going on around them, that they should feel that there has been a past at all. Every additional item of knowledge about the process by which human society has slowly reached its present form is of increasing value. From whatever source it comes to them, it is so much to the good. History is to be welcomed, whatever form it assumes.

There can be no doubt that in late years there has been a very decided increase of general interest in history amongst us. The nature of political questions, and the tendency of thought about social questions, have given a decided impulse in this direction. In small towns and villages historical subjects are amongst the most popular for lectures; and historical allusions are acceptable to all audiences. It was not so fifteen years ago. At that time I remember an eminent statesman speaking to me sadly of his experience. He had been speaking to a vast audience in the open air, under the shadow of one

of our oldest cathedrals. The crowd was so great that it had to be addressed from various platforms, of which he occupied one. He told me that he was led by his architectural surroundings to indulge in a peroration in which he exhorted his hearers to act worthily of their mighty past, and pointed to the splendid building as a perpetual memorial of the great deeds and noble aspirations of their forefathers. The allusion fell upon dull ears; no cheer was raised; the point was entirely missed. My friend then strolled to the next platform, where a longer-winded orator was indulging in a lengthier speech. He, too, selected the cathedral to give local colour to his peroration. He denounced the wrongs of the people, and shook his fist at the great church as the symbol of oppression, the home of purse-proud prelates who adorned themselves and their belongings at the expense of the poor. But in this case also no cheer followed; again a rhetorical sally which owed its point to any feeling for the past was unheeded. The working men cared neither for the good nor the evil of the past; their minds were set upon the present, and that was enough for them. I think this indifference would not be shown nowadays. One view or the other would raise a hearty cheer. There is nowadays a conception that things have grown, and that the way to mend them is to get them to grow in the right direction. This attitude of mind is the abiding contribution which a knowledge of history will make to social progress. Perhaps every branch of knowledge is more valuable for the temper which it creates, which can be shared by every one, than by its direct contributions, which can be judged by only a few. Again, I say, let us welcome the results of knowledge in any and every form.

It is not, however, my intention to-night to criticise the various ways in which history has been written. It is enough to say that it is not absolutely necessary to be dull in order to prove that you are wise, or to repress all human emotion in order to show that you are strictly impartial. On the other hand, the perpetual appeal to sentiment grows tedious, and the steadfast desire to construct a consistent character by disregarding uncomfortable facts, or explaining them away, does not carry conviction. It is even more impossible to write history with a purpose than it is to write fiction with a purpose. Fiction can at least select its own limitations, and professedly excludes all the events of the lives of its characters except what suits its immediate purpose. We know that the state of the world's affairs could not be set to suit a particular past, and that men cannot be read into the expression of abstract principles. History is very impatient of direct morals. Its teaching is to be found in large tendencies, which, it may be, are very imperfectly traceable within particular limits. History cannot be made picturesque by the skill of the writer. It must be picturesque in itself if it is to be so at all. All that the writer can claim is the artistic insight which discerns the elements of a forcible composition in unexpected places, and reveals unknown beauties by compelling attention to what might otherwise be overlooked.

We may agree that history should be made as picturesque as possible; but picturesqueness cannot be applied in patches. Characters must be made life-like by remembering that after all they were human beings, neither wholly good nor wholly bad, but animated by motives analogous to those which animate ourselves, and are common to man in all ages. An historian ought to live with his characters as much as possible, and form a conception of their temperament and appearance, so as to feel that he is dealing, not with dummies, but with real persons. This is not always the method pursued. I remember being told by a friend that he was in a great library, and saw a popular writer anxiously searching the catalogue, with a bundle of proofs under his arm. He proffered his assistance, as he was merely reading at large for a few days, and would be glad to have an object. "Oh," said the author with a sigh, "I want to know the colour of So-and-so's hair, and I don't know where to find out." My friend spent three days in discovering this fact, and observed, when the book appeared, that the information was used in a description of the hero at a great crisis of his fortunes: "He stood with his shock of red hair and flashing eyes," &c. Now in this case it is obvious that the judgment on which the book was written was formed first, and then picturesque details were sought to deck it out. I have sometimes meditated whether or no the judgment would have been the same if the writer had known at first that his hero had red hair. As we are affected in daily life by personal appearance as an index of character, so we might well be affected by some corresponding conception of temperament in great men of the past. Historical portraits are very valuable; the knowledge how a man's appearance impressed those who saw him is equally valuable. No outburst of description makes a man real. This is only possible by a sympathy between the writer and his character, which penetrates all that he says of him. A large, yet consistent, representation is the best form of picturesqueness in this important field.

The danger of an excessive desire for picturesqueness is that it leads to a purely external view of the course of affairs. The writer passes hastily from one strongly marked personality to another, from one striking event to another, and neglects all that lies between them. Yet personalities are only really interesting as they exhibit tendencies which are widely spread; and it is the strength of these tendencies which finds expression in the dominating character. In fact, the character itself is of no value for the purposes of history unless it be brought into relation with the general conditions of life and thought which produced it. This is the difference between history and fiction. For the purposes of fiction you have to grant the possibility of the character which is analysed or displayed in action. For the purposes of history you have to understand the correspondency of the character with the conditions and circumstances of national life. It requires a skilful delineation of those conditions to give a character historical reality. He cannot be detached from his background. His whole

interest lies in the fact that he really existed, and he must above all things be made possible. The reader must not be left bewildered and amazed, asking himself what sort of men lived on the earth in those days, and what were the interests and pursuits of the ordinary man.

It is obvious, therefore, that all history cannot be made equally picturesque, and that it is useless to attempt to make it so by deliberate omissions of all that is not picturesque. We must take human affairs as they come. After all, men did not live in the past for our amusement, but for our instruction. There were probably as many dull people in the past as there are in the present, and we may console ourselves with that reflection. I can see no reason why any one should read history except that he wishes to learn how things really went on. I do not know that any method of writing can make them always exciting. I hear people sometimes complain, "The newspapers are very dull to-day." I find they mean that there is no record of a great accident, or a horrible murder, or a political catastrophe. I think, however, they would change their remark and become very serious if, let us suppose, the newspapers chronicled a great railway accident on every day in one week. They would crave for a period of uneventfulness, and think that it was more permanently satisfying. We need a stable basis to rest upon before we can find comfortable pleasure in contemplating instability. Picturesqueness must have an element of restfulness. It is not to be found in constant excitement, but in clear-cut and attractive presentation of events.

The possibility of such presentation, strange to say, becomes greater as the events are more remote. This is due to two causes: first, that we have made up our minds more clearly about what is important in the past; secondly, because the amount of materials which are available is limited. There is an immense difference between writing history previous to the sixteenth century and writing history after that date, owing to the nature of the material. The change which separates modern from mediæval times was made by the conscious growth of nations, and the consequent complexity of international relations. The difficulty of dealing with modern history is the impossibility of isolating events and their results. This truth is expressed in the amazing development of diplomacy and the vast multiplication of documents, which is to the historical craftsman the dividing line between two periods. The contemporary chronicler, who was previously the chief authority, sinks into the background. The historian has to wander patiently through endless byways, which lead apparently nowhere. It is comparatively easy to form a clear conception of a man's character when you have only the general outlines of his life and the record of his permanent achievements. It is much more difficult when you can follow his projects from day to day. The great mass of those projects came to nothing. Yet it is true, if we look to private life, that a man's

character is more revealed by what he tries to do than by what he succeeds in doing. Indeed, it is not paradoxical to say that his abiding influence is expressed by his aspirations rather than by his achievements. His most fruitful heritage is, generally speaking, his temper, his attitude towards life, his method of facing its problems. The great question is, Did he heighten or did he lower the sense of duty of those amongst whom he lived and worked? The same mode of judgment seems to me to hold true in the large affairs with which history is concerned. Before we can judge a statesman rightly we must follow his aims and methods in detail. He could only command certain forces, the power of which was best known to himself. It is easy to prescribe an heroic policy at great crises, to lament apparent pusillanimity, and to arrange quietly in one's study, after a lapse of centuries, an ideal termination to political difficulties. But we are all of us conscious of the difference between what we would do and what we can do. Everybody who sits on a committee comes away feeling that he could have managed its business better by himself. But the use even of a committee is to show you what available resources a particular line of action can command; and you generally depart with a conviction that it is only the second-best policy which has any chance of immediate success. Statesmen in the past suffered under the same limitations. The possession of supreme power by rulers is only apparent. Somehow or other they had to discover what the nation was likely to do, and more than that they could not venture to undertake. Improvements in the mechanism of government are of use as they enable statesmen to gauge more accurately the forces on which they can rely. There is one lesson that comes from reading diplomatic records: it is that rulers were always trying to make the best of a bad business. Parliamentary obstruction is only a condensed form of what had always to be reckoned with. The outward expression of tendencies has changed, rather than the tendencies themselves.

It is very difficult to clothe with any appearance of interest abortive attempts which came to nothing, which were put forward in ambiguous language, and were often cloaks to some further purpose behind. Yet, as a matter of fact, these constituted the main activity of many statesmen, and if we leave them untraced or unmentioned, we are missing the point of their laborious lives. There is no more widespread delusion than that a man in a great position gets his own way. He is envied by the ignorant and thoughtless for his supposed power, for his freedom from those petty inconveniences of which they themselves are keenly conscious. The opportunity to do what one wills—this is assumed to be the privilege of those who direct affairs. One of the great lessons of history is to show the bondage, as well as the responsibility, of power. The trials and disappointments of the great deserve recognition—not only their failures in great undertakings, the dramatic

downfall of over-lofty schemes, but the small difficulties of their daily business, the imperious limitations by which they were constantly hampered. This has a meaning of direct importance to us all; but it is hard to make the troubles of daily life picturesque. The writer of fiction moves us by the stirring adventures of his hero and heroine in overcoming difficulties which stood in the way of their marriage. Then he leaves them to settle down to humdrum life as best they can. They are no longer interesting, but become as ignoble and commonplace as their parents were at the beginning of the book. The historian cannot treat his personages in the same way. He has to face the difficulty of extracting some interest from their average occupations. He is tempted to shirk it, and to hurry on to something in which he can find fuller scope for his power of description.

It is, therefore, this diplomatic record which goes far to injure the picturesqueness of history. It constantly reveals limitations which could not be overcome. It shows us the hero in his shirt-sleeves, labouring mostly in vain, and it enables us to see only too clearly his inevitable defects. But if we look a little longer we see that it enlarges his personality, and exhibits him as the representative of his nation. This really sets him on a higher level, and gives him a greater dignity. He is bearing the burden of his country, and is fettered by her deficiencies. There are many things which might be done if he had the means to do them. He can only reckon on so much, and must make it go as far as he can. His projects are tentative, and he is often obliged to withdraw from much for want of a little. He is not really his own master, but serves a public which imperfectly understands its own position and grudges everything it gives. Whatever else picturesqueness may attempt to do, it must not seek to abolish the pathos of humble industry.

I have been speaking generally about picturesque ways of writing history, in the ordinary acceptation of the term. Let me attempt to go a little farther, and try to discover in what the picturesqueness of history consists. It is obvious that, if it lies in a series of vivid pictures of events and striking presentations of character, the historian cannot rival the writer of fiction, and historical novels are the proper mode of expressing picturesque presentation. Some historians have felt the need of a more imaginative treatment than their subject properly allowed, and have supplemented their serious histories by historical novels. But the point which I wish to consider is the sense in which history can be made picturesque, and the reason why some periods of history are more capable of picturesque treatment than others.

Now the term *picturesque* itself suggests artistic handling; and it is obvious that in art as much depends on the selection of the subject as on the mode of treating it. An historian is bound by his subject, and cannot make it picturesque if it is not so in reality. The great periods of picturesqueness are those in which personality is

most powerful. This constitutes to many minds the charm of the history of Italy, especially in the fifteenth century. There was then a copious supply of determined and adventurous characters, whose main object was to express themselves fully. Outward circumstances gave them a favourable opportunity. They rose by their own dexterity, and aimed at artistic completeness in all their achievements. They are attractive by their freedom from conventional restraints, by their unhesitating self-confidence, and by the magnificence of their aims. The same spirit which animated Italy passed on in a somewhat modified form to the rest of Europe in the sixteenth century, and became domesticated in France. From that time onward we may say that French history is the most picturesque.

Yet it is worth observing that a mere expression of character, unfettered by ordinary restraints, does not of itself satisfy our craving for picturesqueness. In fact, the most purely personal history is that of the later Roman Empire, of the Byzantine Empire, and of its successor, the Russian Empire. For striking scenes and dramatic events, these histories surpass any others. Caligula and Nero, Leo the Isaurian and Irene, Ivan the Terrible and Peter the Great, outstrip in wilfulness and daring anything that Italy or France ever produced. Yet they seem to us remote and monstrous; they do not touch us with any sympathy; they belong to a range of ideas which is not our own; they represent characteristics of power with which we are not familiar. It is not enough that scenes should be striking, or characters strongly marked. Scenes and characters alike must stand in some definite relation to ourselves and our actual surroundings. I doubt if our interest in Italian history would be so strong were it not for the fact that its records still remain and have their message for us. Italian princes would be forgotten had they not been patrons of artists and architects, whose works speak to us by their beauty and their grandeur. We wish to know what was the view of life which gave these creations such dignity and grace, who were the men for whom such stately palaces were built, what was the conception of human character and its possibilities which prevailed in the community from which they sprang? The men themselves are only interesting because they were conspicuous and intelligible instances of tendencies which we wish to see expressed in action, that we may more clearly understand their meaning as expressed in the abstract forms of architecture and art. Our interest is not primarily in the men themselves, or their doings, but in the significance of the ideas which lay behind them. The same thing is true of the picturesqueness of French history. We are attracted by the process which produced that mental alertness and precision which characterise the French mind, that power of organising life so as to get the most out of it, which is still the peculiar merit of the French people.

This leads me to another point. A bald record of events or a faint description of a character by a contemporary does not suffice for historical picturesqueness. Things may loom large, and we may

see their importance, but we cannot hope to reproduce them by mere exercise of imagination. Picturesqueness must come from adequate materials, and every touch must be real. Imagination, after all, is only an arrangement of experience. You cannot really create; you are only borrowing and adjusting odds and ends according to some dominant conception. It is useless in history to read a man about whom little is known into the likeness of another about whom you may know much. It is useless to reproduce an obscure period in the terms of a period with which you are more familiar. Where we do not know we cannot safely invent. Now picturesqueness in history must depend on the material available for intimate knowledge. It is only at times when men were keenly interested in life and character that such records were produced. We cannot make the life of Byzantium live again, for the records are formal and official. Outside accounts of magnificence suggest little; we need the touch of intimacy to give life. In short, picturesqueness is only possible in dealing with periods when literature was vigorous and contemporary memoirs were plentiful.

I should not like to say whether the demand created the supply, or the supply created the demand. It is enough that men were interested in themselves and in one another, and have left us the result of their interest. That interest arose from a belief in the importance of what was happening, and a power of tracing it to individual action. Hence prominent individuals were closely scanned, their motives were analysed, and the influences which weighed with them were carefully observed. In some cases the men themselves were worthy of study: in other cases their importance was entirely due to their position. But anyhow they were representatives of their times, of the habits, manners and ideas which were current. The picture which we wish to have in our own minds is not merely that of the man, or of the events in which he took part, but of the life and the society which lay behind him.

The picturesqueness of history, therefore, is largely due to memoirs; and the countries and epochs which have produced them are especially picturesque. Now it is great crises, periods of disruption, great emergencies, which as a rule impress contemporaries and furnish matter for close observation. The production of crises is, of course, not the highest sign of human intelligence. In fact, a crisis is due to blundering and incapacity. But when a crisis occurs it is a revelation of character. This is obvious in the drama. It is impossible to represent an ordinary man engaged in his ordinary pursuits. To show what sort of man he is, it is necessary to place him in an extraordinary and unexpected position; then all his hidden strength or weakness comes to light. A man can only be defined by his limitations; and these are only obvious when he has to act on his own initiative, robbed of his ordinary props, and forced to draw upon his own intellectual and moral resources. Hence it comes that we feel the attraction of troublous times in history, and

regard them as the most picturesque. The Great Rebellion and the French Revolution have furnished endless motives to dramatists, novelists and painters, because they suggest possibilities of striking contrasts, and afford available situations. The human interest is then most intense, and our sympathies are most easily awakened.

But though such times are the best for displaying individual character, it may be doubted if they are the best for displaying national life and national character. Indeed, they exaggerate differing tendencies which, in an ordinary way, work harmoniously together, and force them into violent opposition. It is true that the tendencies were there, that they rested upon certain ideas and made for certain ends. But in the exigencies of a struggle they assumed undue proportions and became one-sided through the apparent necessity of denying any right of existence to the ideas opposed to them. In short, national life depends on the blending of various elements, and the co-operation on a large scale of efforts which, regarded on a small scale, seem to be diametrically opposed. Periods of revolution destroy this process, and make the apparent opposition an absolute one for a time, so that the parallel between the individual and the nation fails in this point. A crisis in the life of the individual reveals his true character, because it compels him to gather together the various elements of which that character is composed and condense them into a decisive act. In the case of a nation the contrary occurs. The crisis dissolves the bands which bind national character together, and sets some of its elements against others. All are equally necessary; they must ultimately be recombined and reabsorbed; they do not really exist in the form in which they show themselves under the exigencies of conflict. Revolutionary epochs may be the most interesting, but they are not the most instructive. They may show us forcible characters, but these characters are rarely attractive. They may emphasise national characteristics, but they do not show them in the form in which they really work. It is true that a decisive choice will be made which elements are to be dominant in the new combination. So far as those elements were unknown and unsuspected before, the interest lies in discovering their origin and the source whence they drew their power. The picturesqueness of revolutionary periods is really dramatic and psychological, not strictly historical.

We come back, therefore, to the position that history is picturesque at those epochs when national tendencies are expressed in individual characters, and when the consciousness of this fact creates a literary study of those characters which is given in considerable detail. It is worth while to go a step further, and consider what may be learned from this fact. Perhaps this may best be done by reference to the history of our own country, with which we are most familiar.

English history is not very picturesque. It has not produced a large number of striking situations or of strongly marked characters.

It is by no means rich in memoirs, and the most stirring times have not called forth the most vivid description of their incidents. There is no brilliant biography of Oliver Cromwell, for instance, by a contemporary. We have to piece together materials for the characters of Henry VIII., Elizabeth, Mary Queen of Scots, and Charles I. No one at the time attempted to grasp them. The dramatic moments of their careers were only dimly and imperfectly felt. Let me illustrate what I meant when I said that it was impossible for later writers to create deeper impressions than were present in the minds of contemporaries. Two situations occur to me as surpassing all others in English history in vividness and dramatic effect; they are the murder of St. Thomas of Canterbury and the death of Wolsey. This is entirely due to the fact that they profoundly moved men's minds at the time, and are recorded in language which is full of the emotion so engendered. Both were regarded as great and significant catastrophes, important in themselves and in their results. The death of Wolsey is a remarkable instance. In outward circumstance it is inferior to the execution of More or the burning of Cranmer. Yet it remains more picturesque. We feel that More and Cranmer fell in a way like soldiers on the field of battle. They shared the fortunes of their cause, and our interest lies in discovering the exact point on which they took their intellectual stand, and laid down their lives rather than take a step further. But Wolsey is a type of human fortunes, of the inherent limitations of man's endeavours, of the sudden reversal of high hopes, of the restless chafing of an imprisoned spirit, and its final despair. This position arises from the literary skill of his biographer, Cavendish, reflecting doubtless the permanent impression of his time, and expressing with deepening melancholy the profound pathos of the wreckage of a life. This intensity of feeling could not have gathered round an ordinary career, but was engendered by the profound conviction that with the fall of Wolsey England had entered upon a new course in its national life—a course the end and goal of which no man could foresee. Wolsey had striven to make England powerful in a changing world. He had created forces which he could not restrain within the limits which his prudence had prescribed. There was deeper emotion at the downfall of him who strove to keep the peace than over the sad fate of combatants on either side when once war had been proclaimed. It is only the pen of one who is conscious of living through such a crisis that can be instinct with real feeling and can convey that feeling to after-times.

It is curious to observe that these two instances of Thomas of Canterbury and Wolsey, are both cases of men who pursued clear and decided objects, and whose characters consequently detached themselves from the general background of contemporary life. The objects which they pursued were not in either case popular, and they had to trust mainly to their own resoluteness and skill for ultimate success. Hence came the attraction of their characters for their biographers. They were men who could be studied and de-

scribed in themselves, apart from the results of their actions. In fact, any estimate of or sympathy with their line of action was entirely secondary to the interest of the men themselves. In this sense they resemble the subjects of Italian or French history. They rose to power by their own capacity, and they used their position consciously for the furtherance of objects which they deliberately selected for themselves. It is this which gives a picturesque interest to characters in history. We are most easily attracted by a sense of completeness and self-determination. This, indeed, is the artistic quality in character, and alone admits of clear and forcible delineation. Opportunism, however successful, cannot well be depicted clearly; it must be considered by reference to a number of possibilities, and challenges our judgment at every step. A man who is doing his best under untold difficulties may be heroic, but he rarely enjoys any great moments which set forth his heroism in a striking way. Our judgment may after a long survey recognise his worth, but that does not make him picturesque. William the Silent can never fill a large canvas, great as was his contribution to the best interests of the world.

The picturesqueness, then, of the history of any nation, or period, depends upon the possibility of an individual detaching himself from ordinary life in such a way as to express in himself its unconscious tendencies. The possibility of such individual detachment depends on the ideas on which the ordinary life of the nation is founded. If these ideas are to be represented by a person, they must be comparatively simple. For this reason great crises in a nation's history are the most picturesque, for they simplify national ideas by forcing one or two great principles into temporary supremacy over all else. Yet even in great crises England has not brought forth clearly representative characters. Oliver Cromwell, for instance, was the executor, rather than the representative, of the principles of the Great Rebellion. They were never definite enough to be summed up by any individual. However highly we may rate Cromwell's capacity, we cannot make him out as eminently picturesque, or place him by the side of Napoleon.

We may, I think, go a step further. The ideas on which national life are founded may be ultimately reduced to the national conception of liberty. Ultimately each man values the society of which he forms part for the opportunities which it affords him of doing or being what he wishes to do or be.

Now there is a difference, which is not always recognised, in the meaning of liberty to different peoples. It would be a long matter to attempt to explain this difference in detail and account for it. But we may say generally that it depends on the way in which the rights of the individual are regarded in relation to the rights of the community. Let me apply this to the instances of picturesqueness which I have taken. In Italy, in the sixteenth century, the communities were so small, and their position was so precarious, that men longed

for the growth of a national spirit, as the limits in which their actual life was lived were too narrow to express that life in its fulness. A nation could only be formed by the power and influence of a dominant and resolute personality. Hence men were so interested in the development of such a personality that they were ready to watch various experiments and to endure much tyranny in the hopes of final success. This created a curious accentuation of the value of individual character, and an absence of any sense of its limitations, which was undoubtedly fitted to produce picturesqueness, but had serious drawbacks in practice.

In the same way, the historical circumstances of the consolidation of the provinces of France under the Monarchy developed a high appreciation of individual character; and the keenly logical intelligence of the French mind gave it a permanent place in literature.

England, on the other hand, became in early times an organised community, and there was no violent break in the pursuit of this organisation. I cannot now trace in detail the results of the different course of English and French history as reflected in the characters of the people. But this at least is obvious: the average Frenchman conceives of himself as having a right to gratify his individual desires, without thought of others, to a degree unknown to the average Englishman. French civilisation is concerned with the arrangement of the externals of life in the most comfortable way. English civilisation is concerned primarily with political institutions and with the organisation of the activities of life. The Frenchman conceives himself as an individual, the Englishman conceives himself as part of a community. The Frenchman, though wedded to his own country, and having no desire to leave it, still considers himself as a citizen of the world. The Englishman, though a rambler and an adventurer, ready to make his home anywhere, still considers himself an Englishman wherever he goes. France took for the motto of its aspirations "Liberty, Fraternity, Equality." I believe that if England had had occasion to formulate its aspirations in the same way, its motto would have run "Liberty, Justice, Duty."

Now picturesqueness is obtained by isolating men from their surroundings, by getting clear-cut situations. To this a Frenchman lends himself; he is accustomed to think and act by and for himself. An Englishman objects to isolation; however much he may be alone, and however decidedly he may act, it is as a representative of England, with a mass of national tradition behind him, which he would not rid himself of if he could. He will take enormous responsibility upon himself, but while taking it repudiates it. He minimises his own individual part in what he does, and is persistently apologetic.

I think I can illustrate my meaning from our literature. Shakespeare has shown with curious insight the difference between northern and southern peoples. Othello and Romeo, when touched with passion, are pure individuals, and act entirely with reference to their own feelings. The difficulties of Hamlet lay in the fact that he could not

forget that he was heir to the throne of Denmark, and could not act in such a way that righteous vengeance should seem to be private ambition. He could not escape from his attachment to society, and therefore he will always fail to have the picturesqueness which belongs to individual detachment.

I have been speaking of picturesqueness in its ordinary sense. The upshot of my remarks is that in proportion as history is picturesque in this sense it is not really history. For history is concerned with the life of the community, and picturesqueness with the character of individuals. But there is, I think, a larger and truer picturesqueness, which may be found not in details but in principles. The great object of history is to trace the continuity of national life, and to discover and estimate the ideas on which that life is founded. Individuals are only valuable as they express those ideas and embody that life. Such expressions are often to be found in lowly places, and are manifested in inconspicuous lives. It is the true function of history to discover and exhibit them wherever they may be. In our own history, at all events, I am convinced that we need a heightened sense of the causes which produced those qualities which have created the British Empire. The most picturesque hero is the English people itself, growing through manifold training into the full manhood which it still enjoys. What made it? What principles does it embody? How may these principles be enlarged in view of its great and growing responsibilities? These are questions which have an undying interest, and men's minds are being more and more turned towards them. For us, at all events, the highest imaginative charm gathers, not round individuals, but round the growth of our conceptions of public duty. To trace the growth of that body of ideas which make up England's contribution to the world's progress, to estimate their defects, and to consider how they may be increased by broader sympathies and greater teachableness—this is a task which requires the qualities at once of a scientific explorer and of a consummate artist.

WEEKLY EVENING MEETING,

Friday, February 12, 1897.

George Matthey, Esq. F.R.S. F.C.S. Vice-President, in the Chair.

Professor John Milne, F.R.S. F.G.S.

Recent Advances in Seismology.

As an introduction to the discourse for this evening, I feel it my duty to call attention to the broad meaning which it now seems necessary to apply to the word Seismology. Only a few years ago the occupation of the seismologist was strictly confined to the study of sudden movements which from time to time take place in the crust of our earth. These movements, although sometimes violent, were to him transient phenomena which seldom continued longer than a few seconds, or at the most one or two minutes. Recent investigations have shown that the same disturbances are preceded by minute tremors which, under certain conditions, may last many minutes, whilst after all movement to which we are sensible has ceased, the ground may palpitate for many hours. Another set of phenomena to which attention is now directed, are the earthquakes which are repeated many times per year in every country in the world, which by our unaided senses are passed by unnoticed. In short, the unfelt evidences of seismicity are much more general than those which are accompanied by destruction and alarm, and a new seismology has been discovered which is at least as important as the old.

Now that we are assured that the greater number of earthquakes are but intermittent accelerations in the more general movements of rock folding and rock crushing, to separate the announcements that these mighty changes are in operation from the changes themselves, is to separate an infant from its parent, an effect from its cause. Besides these legitimate relations of earthquakes, the practical seismologist finds that he often records movements of a quasi-seismic origin, together with others like diurnal waves, and tremors which find an explanation in causes external to the surface of our earth. These latter are at present without a home, and although they are non-seismic, in many instances at least, they represent actual movement in the ground, and seismology finds itself in the position of foster-mother to strange children. These various movements which take place within and on the surface of the earth, the study of which may, until we find a more suitable word, be embraced under the term seismology, are indicated in the following table:—

1. Bradyseismic or slow secular changes, resulting in the elevation or depression of countries and mountain ranges.
2. Secular flow or crush. Of this we have only indirect evidence.
3. Annual or longer period changes in level.
4. Earthquakes or accelerations in bradyseismic action or secular flow. Volcanic earthquakes. Sea waves. Air waves.
5. Unfelt earthquakes, common to all countries.
6. Irregular changes in level completed in a few minutes, or in many days.
7. Diurnal waves.
8. Tremors, or microseisms and pulsations. Possibly in part atmospheric movements.

The advances that have been made during recent years by recording movements which may possibly have a bradyseismical character are, as compared with the information derived from the study of the other movements with which we have to deal, but few in number. Both in Germany and in Japan, horizontal pendulums have been carefully installed underground, and it has been found that in both instances, as with the levels of Plantamour, although there is an annual change in inclination which cannot be accounted for by seasonal changes in temperature, there is for periods of several years' duration a continuous tilting in one direction.

A very curious observation made in Tokio, was, that very often for several days before a local earthquake, a horizontal pendulum would gradually wander towards the west. Although such a sequence in phenomena may have been accidental, because it has been shown by observation with seismographs founded on the solid rock that the greatest and most frequent motion is in the direction of the dip rather than parallel to the strike, indicating that the direction of folding is a direction of pronounced yielding, whilst slow change in level is apparently most pronounced in districts where mountain growth is possibly yet in progress, we see in the Japan observations an indication of the possibility that crises in bradyseismical motion may be foretold.

I learn from Col. J. Farquharson, R.E., Director of the Ordnance Survey, that some years ago the question whether during recent years there had been any changes in level in Britain was carefully tested in Lancashire and Yorkshire, under the direction of Sir Charles Wilson. The first levelling in these counties was carried out between 1843 and 1850, and the second between 1888 and 1894. Excepting in the coal and salt districts, no material changes were found to have taken place. It is, however, to be remembered that this re-levelling was confined to lines of level along roads, and whether there have or have not been any changes in the height of hills or mountains since the first measurements were made we do not at present know.

One method of measuring bradyseismical effects within a period of three or four years, and to determine how far such movements may be connected with the occurrence of earthquakes, would be to estab-

lish in a suitable district a triangular arrangement of three sets of levels, the distance between each set being several miles. All the instruments should be on the rock, and displacements parallel and at right angles to the dip should be recorded.

A summary of all the advances which have of late years been made in the study of earthquakes would, in great measure, be found in an epitome of the twenty volumes which since 1880 have been published by the Seismological Society of Japan, a work which is being actively continued by a committee supported by the Japanese Government.

Previous to 1878 our knowledge of the character of earthquake motion was largely dependent upon the effects such motion produced upon various bodies and upon our senses. To correct and extend this knowledge, students of earthquakes in Japan at about this time devoted nearly their whole attention to seismometry, first testing pre-existing forms of apparatus, and then experimenting with forms which were new. Those instruments which were intended to record the rapid and violent movements of the ground, whether these were in a vertical or horizontal direction, did this relatively to a mass so suspended that, although its supports were moved, a point in this mass remained practically at rest. An account of these seismographs was in 1888 given to this Institution by Prof. J. A. Ewing, F.R.S.

For earthquakes in which there was a vertical component of motion, however, it was soon noticed that these "steady points" were swung from side to side by tilting, and instruments had then to be devised to measure angular displacements. Following these came a class of instruments intended to record the slow undulatory and often unfelt earthquake motions. These, together with a group of tromometers or tremor measurers—apparatus to record the time at which shocks had occurred—resulted in the development of a group of instruments which would require for their description a volume on Seismometry, and it is fair to say that the seismometry of Japan revolutionised the seismometry of the world.

After the new inventions, the story of which forms one of the most important in Japanese seismology, records were obtained which showed that the impressions we had with regard to earthquake movements had been widely incorrect, whilst they also indicated that our estimates in mechanical units of seismic destructivity had been founded on a wrong hypothesis. Having given the dimensions of a body that has been overturned, or the dimensions and tensile strength of a wall or column-like structure that has been shattered, we are now in a position to calculate the acceleration to which the same has been subjected, and the result arrived at is not far removed from calculations of the same quantity derived from the diagrams obtained at the same time and at the same place from a seismograph. Investigations of this description have been applied with marked success to construction, and as new engineering works and new buildings spring up in Japan, we see that rules and formulæ are followed which are

unknown and not required in countries free from earthquakes. That these rules, which take into consideration that structures have to withstand stresses due to more or less horizontal displacements at their foundations, have been followed, is in itself a testimony that engineers regard them as being worthy of consideration, and we now feel assured that when an earthquake like that of 1891, which cost Japan 10,000 lives and an expenditure on repairs of at least 80,000,000 dollars, is repeated, the losses will be comparatively trifling. That the new departures in engineering and building practice have proved beneficial has been repeatedly demonstrated. Because experiments showed that earthquake motion at a comparatively shallow depth was somewhat less than what it was upon the surface, a number of modern and important buildings in Tokio have had given to them deep foundations and are surrounded by open areas. On several occasions these buildings have stood unimpaired whilst neighbouring structures have been badly shattered.

The tall chimneys of factories, as well as those of ordinary dwellings, have been so far modified that the new forms stand whilst the old forms fall. The greatest material benefits which seismology has conferred upon Japan will, however, probably be found in the radical changes which are taking place in the construction of ordinary dwellings.

One application of seismometry to the working of railways in Japan has resulted in a saving of fuel of from 1 lb. to 5 lbs. of coal per mile per locomotive. In these and other ways, by following up initiatives created during the last twenty years, Japan has reached a high position, if not foremost, amongst nations who have given attention to seismology. The Government of that empire, recognising the value of what has been already accomplished, and that much more is yet open to investigation, have at their university established a Chair of Seismology, a committee which is liberally supported, to make investigations relating to earthquakes and their effects, and a seismic survey of their empire.

When we remember that a single earthquake has often cost Japan a far greater loss of life and an expenditure of public funds at least comparable with that accompanying her recent war, it is not remarkable that her chief interest in earthquakes has been directed towards means to mitigate their effects; by doing which, whilst conferring benefit on herself, she has also conferred benefits upon the earthquake-shaken countries of the world. Notwithstanding this, questions of interest to science have not been overlooked. The object of one series of experiments, which were carried out at intervals extending over several years, was to measure the velocity with which disturbances produced by explosions of dynamite and other substances were propagated, and to study the character of the vibrations as they radiated from their source. Near to an origin a clear separation between normal and transverse movements was observable, which at distances exceeding 50 or 100 feet was lost. Single waves as they spread outwards were

seen to gradually change into double waves. The velocity of propagation evidently increased with the intensity of the initial impulse; it was greater for vertical and normal than for transverse waves, and vibrations generally were propagated more rapidly to stations near an origin than between stations at some distance from the same. These and many other results were confirmed and extended by records obtained from a series of nine seismometric stations situated on a plot of ground the area of which was only a few acres. In these investigations the records, which were drawn upon the surfaces of smoked plates, were those of real earthquakes. The motion on one side of this ground was invariably so much greater than it was 900 feet distant upon the other side, that it offered an explanation for the peculiar distribution of ruin so often observed in a city after it has been shaken by an earthquake. The houses in one street may stand, whilst others possibly not more than 100 feet distant, also standing on alluvium, but somewhat softer in character, may be shattered. From the survey of a field, seismic investigations were extended to the survey of Tokio, and then to the survey of the northern half of Japan. At this point the Government came to the assistance of private observers, and took under its control the survey of the whole empire, embracing an area of 140,000 square miles, within which there are now close on 1000 stations at which earthquakes are recorded.

The results of this undertaking are not at present fully known. What we have learned is that during the last six years the average number of shocks have been about three per day, a frequency which is greater than that which is usually given for the whole world.

If we take the well-marked earthquake districts of the world and give to them frequencies one-third of that in Japan, it would not be an over-estimate to say that 10,000 movements sufficiently strong to be felt and shake considerable areas of our planet occur every year. Five thousand of these come from the home of our deep-sea cables.

The Japan earthquakes, like those of South America, mostly originate on the side of the country which slopes steeply down beneath the Pacific Ocean. In fact, it may be taken as a rule that whenever ground over a considerable distance, which I will take at 120 geographical miles, has an average slope greater than 1 in 50, in such districts under the influence of bradyseismical bending or of secular crush round the base of the continental domes, earthquakes are frequent. From Japan to beneath the Pacific, slopes of 1 in 25 occur, whilst on the coast of Peru slopes as great as 1 in 16 may be found. The volcanic districts of Japan which, like those of South America, are found along the upper part of a bradyseismic fold, are singularly free from earthquakes, and the times of seismic and volcanic activity show no marked connection.

The analyses of the Japan records, as a whole, as with the analysis of the records of most other countries, show a marked annual and semi-annual periodicity. The former of these, which shows a winter

maximum for both hemispheres, is attributed by Dr. C. G. Knott * to the fact that in winter we have large accumulations of snow and steeper barometric gradients than in summer, and it is these inequalities of stress of long continuance which cause yieldings to be more frequent at one season rather than at another.

The most important feature in the Japanese records, which gives to them a value greater than those of any other country, is the fact that the various shocks may be classified according to the district from which they originated, and at the same time a value or weight can be given to each, according to the area it disturbed, whilst primary and secondary shocks can be readily separated from each other.

The advantage of such tables, when, for example, we seek for a possible connection between certain lunar influences or the rising of the tide upon a coast, because such influences are at a maximum in different districts at different hours, is at once apparent, whilst all surprise that investigators who have only had at their disposal tables of earthquakes the origins of which have been in widely separated districts have failed in establishing laws, which we might anticipate, at once disappears.

Thanks to the liberality and foresight of the Japanese Government, we are now in a position to make investigations hitherto impossible, and to confirm or disprove very many of the results of previous investigators. Dr. Knott, who is engaged upon these voluminous statistics, finds a confirmation of the law of Perry that there is a maximum in earthquake frequency near the time of perigee; that there are maxima associated with the moon's declination; its conjunction with the sun; the time of the moon's meridian passage; and the ebb and flow of tides. Until these investigations have been completed and published, their importance cannot be fairly estimated. Dr. F. Omori has pointed out the existence of diurnal and semi-diurnal periodicities, and that the frequency of after-shocks follows fairly definite laws; the former of which investigations has by rigid treatment been emphasised and extended by Dr. C. Davison.

Many investigations have been made to discover a relationship between seismic phenomena and those of an electric or magnetic character, but the only certain result is to show that the artificial or actual shaking of the ground near to an earth plate may be accompanied by temporary currents, whilst the displacement of large bodies of strata, as for example those which accompanied or caused the earthquake of 1891, may result, as pointed out by Prof. Tanakadate, in a permanent readjustment in the relative position of the isomagnetics in a district.

After this earthquake, the cause of which was attributable to the sudden fracturing of rocks, the line of which is traceable on the

* Trans. Seis. Soc. vol. iv. pt. 1, "Earthquake Frequency," C. G. Knott, F.R.S.E.

surface over a distance of 40 miles, many opportunities presented themselves for the observation of sound waves. Often a subterranean boom was heard, unaccompanied by any sensible shaking, but more frequently it was a warning that within a very few seconds there would be a more or less violent shaking.

If we assume that the sounds originated at the same foci as the after-shocks, the velocity with which the former were transmitted was therefore higher than that at which the latter were transmitted. But inasmuch as observation showed that the earth waves had a velocity seven times as great as an air wave, the conclusion is that whatever may be the mechanical action producing the earthquake sound, it is a vibratory motion transmitted through the rocks; and because it is never audible at many miles distant from its source, the vibrations producing it either rapidly die out or change in character.

Another interesting investigation, which is by no means completed, has been to note the effects produced by earthquakes upon the lower animals, several of which are apparently more alive to the existence of minute tremors than human beings. The effect produced by earthquakes on human beings, which partakes largely of an emotional and moral character, is a subject about which many interesting facts have been collected.

Perhaps the greatest triumph in seismological investigations is the fact that we are now assured that if a large earthquake occurs in any one portion of our globe, it can with suitable instruments be recorded in any other portion of the same. Because the rate at which these movements are propagated is so very high, in some instances approaching 12 km. per second, or double the rate at which a wave of compression would pass through steel or glass; because at a given station we have never recorded two disturbances which we should expect had the movement like a barometrical wave been transmitted in all directions round the earth; and finally, because it appears that the velocity to points at a great distance from an origin is higher than that to points relatively near to the same, the conclusion for the present is that the motion, rather than being propagated round the world, is propagated through the same.

Inasmuch as these velocities throw light upon the effective rigidity of the materials constituting the paths along which they were transmitted, the importance of establishing, say at twenty stations, observatories with light horizontal instruments to record these earth movements is at once apparent. The cost of such a set of instruments required to carry out a seismic survey of the world would be about [illegible].

At the observatories where these instruments were established, in addition to the speedy announcements of great catastrophes in distant places, the records of these, and of disturbances of a more local origin, would throw light upon some of the otherwise unaccountable deflections sometimes shown in photograms from magnetographs, barographs and other instruments sensitive to slight displacement.

whilst, as will be shown later, changes in level, affecting astronomical observations, would be continuously recorded.

From the times at which movements were recorded at different stations, it would seem possible to localise the origins of disturbances which in many instances are submarine. This would throw new light upon changes taking place in ocean beds, lead to the identification of districts which those who lay cables are desirous of avoiding, and sometimes enable us to attribute cable ruptures to natural rather than to artificial causes.

Another function of instruments which record these unfelt movements is that their records may often be used to anticipate, confirm or to correct telegraphic information, which are matters of great importance to all communities. Good examples of work having this character are seen if we compare the records obtained in the Isle of Wight and the telegraphic information respecting the three disasters which last year were sooner or later after their occurrence reported as having taken place in Japan.

For some weeks our newspapers told us that on June 17th the eastern coast of Japan had been inundated by sea waves, and that something like 30,000 people had lost their lives. Those who had reason to believe that either on the 16th or 17th, vessels, whether men-of-war or merchantmen, or even friends travelling on land, were to reach the stricken districts on these dates, probably felt some anxiety respecting their safety. The Isle of Wight seismograms showed that in this instance there had been an error connected with telegraphic transmission, of two days, the disaster having taken place on the 15th, whilst on the 16th and 17th all was quiet.

On August 31st similar diagrams indicated that at a distance of about 6000 miles, and therefore probably in Japan, there had been a very violent disturbance commencing at 5.7 P.M. For detailed information about this catastrophe we had to wait until mails arrived some four weeks later. These earth messages reached England from Japan in 16 minutes.

The last disaster, which was reported as having taken place in Kobe, created considerable anxiety with many who had friends and property in that prosperous little city. An absence of records in the Isle of Wight indicated that there had at least been gross exaggeration in the telegraphic news, whilst some weeks later it was discovered that the widely published message, which had been sent regardless of the alarm it might create, was devoid of all foundation.

These, then, are a few of the advantages we should expect from a seismic survey of the world, and all that is required to carry the same into effect is a sum which is very much less than that which is required for the purchase of a modern telescope.

From these disturbances, the origins of which are to be found in gradual or sudden yieldings within the crust of our globe, I will now pass to those movements the origin of which is apparently traceable to external influences, the most interesting of which is the diurnal

wave. At Shide, in the Isle of Wight, where instruments like conical pendulums are installed with their booms in the meridian on the eastern side of a valley which runs north and south, the movements are such that on fine days these booms point towards the sun, indicating that in the morning there is a downward tilting towards the east, and in the afternoon towards the west; at night the motion is eastwards. The direction of this movement, which may have a range of 2″ or 3″, is, however, at the same time different at different places; for example, in Japan, on parallel ridges bounding a swampy valley, the simultaneous movements on these ridges were found to be in contrary directions: that is to say, they were such that we may imagine the trees on the opposite sides of the valley every day to have performed a slow bow to each other.

Because these movements are practically confined to fine weather, whilst in dull wet weather they are hardly discernible, we should imagine them to be the result of expansions and contractions in the surface soil, or warping of the piers carrying the instrument following changes in temperature; but when we find that they are practically as marked in an underground chamber, where the changes in temperature are exceedingly small, the suggested explanation apparently fails.

Another cause to which we may turn, as possibly throwing light upon these movements, lies in the fact that, by the action of the sun, there is on two sides of most observing stations a difference in the load which, by evaporation, is carried up into the atmosphere and there dissipated. As an illustration of this, if on one side of an observatory we had a field of clover and on the other side a surface of earth, the *difference* in the loads removed during a day in summer would often exceed 12 lbs. per square yard. Because the clover side would be the one which would be the most relieved, this would tend to rise, and the pendulum would swing towards the uncovered surface. At night-time the causes leading to a slow return of the pendulum *towards* its normal position would be varied. For example, the area which during the day had lost the most by evaporation would be the one presenting the greater number of points for the condensation of moisture as it rose from the ground, which, on the bare side, would be free to escape to the atmosphere; hence the clover-covered surface would, relatively to the ground on the opposite side of the pendulum, grow heavy, be depressed, and the pendulum take up a retrograde motion, which usually appears to be somewhat less than the daylight displacement.

Another phenomenon bearing upon the movement during the night is the almost unstudied sub-surface precipitation of moisture. Experiment has shown that in certain cases after sunset, when the surface of bare earth is chilled or, in winter, frozen, aqueous vapour rising upwards beneath such an area, instead of escaping to the atmosphere is condensed underground, and the superficial soil grows heavier. Soil which is filled with stones probably shows this in a marked manner; each stone, being a good radiator, is at night

quickly chilled to form a condenser, beneath which moisture collects which otherwise would have escaped to the atmosphere. For this reason fields containing a certain number of stones are more fertile than others where stones are absent.

Another important question, bearing upon differential loading of differently covered areas, depends upon the existence or non-existence of a covering of vegetation. We know how much many plants pump upwards to transpire during the day, but their action during the night is to the writer quite unknown. During the night this transpiration may be small, but are they yet pumping to replace their daylight loss?

An action of this sort, if it exists, only implies a transfer of load from beneath to a higher level on the surface, but if on one part of an area with a common water supply this goes on, whilst it does not take place on another portion of the same, it would follow that the former might be superficially altered in form. What is here stated respecting the cause of the diurnal wave is only a suggestion waiting disapproval or confirmation.

Changes of level are closely connected with rainfall, which, when it saturates a valley has, at one station at least, been accompanied by movements indicating an increased steepness of the bounding hills. During fine weather the motion is reversed, or, in other words, the surface movements on the two sides of a valley, with alternations of fine or wet weather, have corresponded to a concertina-like opening or shutting of the same. Certain seasonal changes in level may in part be due to the removal and replacement of loads represented by leaves and plants.

The last group of movements on which I shall touch are pulsations and tremors, the existence of which are supposed to be indicated by the regular or irregular swingings which are from time to time established in pendulums and other forms of apparatus which are delicately suspended. The occurrence of the latter movements, which have been so carefully studied for many years in Italy, and automatically recorded in Japan, show remarkable relationships to the localities where they are observed, the instruments by which they are recorded, to the seasons, the hours of the day and night, and to a number of meteorological phenomena.

In Japan, tremors were never observed underground upon rock foundations, which, however, has not been the case in Italy. At one station they may be marked, whilst at another station, only a few hundred yards distant, they may be only shown feebly or be entirely absent. A light horizontal pendulum is usually more disturbed than one that is relatively heavy. Tremor frequency and tremor intensity are more frequent during the night than during the day. A favourite hour for tremors to appear, or to attain a maximum, is about 6 A.M., and at one station they were always to be seen between midnight and this hour. They are much more frequent during winter than during summer, when barometric changes are rapid, and when the observing

station is crossed by a steep barometric gradient, whether the local barometer is high or low. Tremors may be marked during a calm, whilst during a gale, when doors and windows rattle, a tromometer may be at rest. They are frequently observed during a frost or thaw, and they are generally frequent when the temperature is falling and when it is low. Although waves beating on a coast may produce *frétillements* upon a surface of mercury, such actions are apparently unconnected with the swinging movements of tromometers.

Because tremors are seldom observed in a very dry building or in an instrument beneath a well-ventilated covering, I am inclined to the opinion that many of these perplexing disturbances can be explained on the assumption that, from time to time, beneath cases which are even air-tight a circulation of air is established. This is brought about, as may be shown experimentally, either in consequence of a difference in temperature in different parts of a case, or, as is shown by the introduction of a desiccating agent like calcium chloride, by the difference in the rate at which moisture is condensed, absorbed or given off at different points within such a cover.

Although a suggestion like this tends to destroy many of the records of so-called earth tremors, and for years daily maps were issued showing the microseismic activity of the Italian peninsula, we are left confronted with phenomena which it is the interest of all who work with instruments susceptible to these influences to understand more clearly

Most particularly we should like to know the reason of their frequency at particular hours and seasons, but above all things, how to avoid visitors which may accelerate or retard the swinging of a pendulum, or cause inaccuracy in the weighings of the assayer.

[J. M.]

WEEKLY EVENING MEETING,

Friday, February 19, 1897.

Sir Frederick Abel, Bart. K.C.B. D.C.L. LL.D. F.R.S. Vice-President, in the Chair.

G. Johnstone Stoney, Esq. M.A. D.Sc. F.R.S. *M.R.I.*

he Approaching Return of the Great Swarm of November Meteors.

e present discourse was intended to supplement one delivered hteen years before, in the Theatre of the Royal Institution, on he Story of the November Meteors,' of which a copious extract l be found in vol. ix. of the Proceedings of the Institution.

Orbit of the Leonids.

In the earlier discourse an account was given of the successive ps which led up to the great discovery by the late Professor J. ich Adams of the orbit of these meteors. They now pursue, and e been for several hundreds of years pursuing, a long oval path in heavens, round which they travel three times in each century. s orbit near its distant end intersects the orbit of Uranus, and y close to its perihelion it intersects the orbit of the earth. It s not intersect the orbits of the intermediate planets, of which principal are Jupiter and Saturn, since the plane in which the eors move is so much inclined to the planes of the orbits of those nets that the meteors are carried above and below their orbits in h revolution. The swarm is extended like an immense procession, ny millions of miles in length, though only some 100,000 miles le, along a portion of its orbit. During one half of each revo- ion the stream is for sixteen years lengthening out as it approaches sun, and during the other half of the revolution, while receding m the sun, it shortens again, not, however, quite to the same size it had at the commencement of the revolution, since one revolution er another there is a gradual increase in the length of the pro- sion.

Entrance of the Leonids into the Solar System.

After the lapse of a sufficient time the swarm will of necessity e so lengthened out as to extend the whole way round its orbit; d the consideration that it is at present of limited length, viewed connection with the dynamical certainty that it must ever keep dily extending, carries our thoughts back to that past time, which nnot be very remote from the cosmical standpoint, when that ich is now a long stream was a compact cluster. It was then,

whenever that epoch was, that these meteors entered the solar system; and in the former lecture the reasons were given which led the late Professor Le Verrier to fix upon the spring of the year A.D. 126 as the date of this remarkable event, when the swarm, which had up to that time been an independent cluster, became a member of the solar system. The cluster at that time seems to have been travelling inwards from open space towards the sun, past which it would, if unimpeded, have made a single sweep, and would then have receded from the sun's neighbourhood to the same immensity of distance from which it came. But while advancing towards the sun, the great planet Uranus seems to have crossed its path. The cluster of meteors must have nearly collided with that great planet; in fact, passed so close that the planet was able to drag the group quite out of its previous path, after which the planet advanced along its own orbit, and left the individual meteors to pursue whatever orbits round the sun corresponded to the speed and direction of motion which the planet had impressed upon each of them. Previous to their encounter with the planet the great meteoric cluster seems to have had sufficient coherence from mutual attraction to be able to maintain itself as a compact group. But in sweeping past so great a planet the difference of force acting on the members of the group would probably be too great for their feeble attraction towards one another. They got a little scattered, and when abandoned by the planet, found themselves too far asunder to admit of their assembling again into a compact body; and since then each meteor has had to pursue independently its own orbit round the sun. These orbits, though very close to one another, are not quite the same; they differ a little in every respect, and amongst the rest, in their periodic times. The average period of traversing the orbit is nearly 33¼ years. For some of the meteors it seems to be a week longer, and for others a week shorter than their mean period. Hence, at the end of their first revolution, the meteors with the shortest periodic time came to their starting point a fortnight sooner than the greatest laggards. At the end of two revolutions they were a month asunder, and so on until now, at the end of 53 revolutions, the foremost of the procession comes round two years in advance of the hindermost.

Astronomers already know much which seems to support this remarkable hypothesis of Le Verrier's; but it is most desirable that probability shall be changed into certainty one way or the other: and the lecturer urged that a great effort ought to be made on the occasion of the approaching return of the great swarm, to secure observations, so full and so accurate as will enable either ourselves or our posterity to trace back with precision the history of the Leonids in the past, and so ascertain with certainty whether it was, or was not, within a few days of the end of February in the year A.D. 126, that these innumerable minute bodies began their present career within the solar system.

When the Meteors will return.

The immense procession takes two years to pass the point where it pours across the earth's orbit. This point the earth reaches every year about the middle of November, and accordingly, when the meteors return the earth will certainly, in two successive years, pass through the stream, and may also encounter the front or rear of the procession in a third year. In this way we may count on having great meteoric displays on whatever is the advancing side of our earth in each of two successive years, in November 1899, and in November 1900, with perhaps a third display in either 1898 or 1901. In the middle of November of the year 1898 the moon will be absent, and if by good fortune the head of the meteoric stream shall have arrived so soon, which, however, is doubtful, we may expect an immense display then on one half of the earth. In 1899, when it appears certain that the stream will be encountered, there will unfortunately be moonlight, which will detract from the splendour of the display, though it need not take away our prospect of securing invaluable photographic records in that year, since it has been found that such photographs may be taken even in strong moonlight.

Sporadic Leonids.

Another matter to which attention was invited was that of the few scattered Leonids which the earth meets with every year, and not only in the years of the great displays. Their presence may be accounted for as follows.

The meteoric stream is about 100,000 miles across—more than a third of the way from the earth to the moon—and through it the earth passes obliquely, occupying about five hours in the transit. The earth intercepts some of the meteors, which plunging with immense speed into our atmosphere, are first heated by the friction to brilliant incandescence, and then dissipated in vapour before they can get within miles of the earth's solid surface. This produces the splendid spectacle which we are privileged to witness on such occasions. But many as are the meteors which the earth intercepts, those are immensely more numerous which pass close enough beside it to be bent by its attraction a little out of their previous orbit—only a little, however, on account of the enormous speed with which they shoot past the earth, a speed of about 45 miles a second—so that each is not so much as three minutes in darting past the earth. The earth has plunged some sixty or seventy times through the stream, and has thus diverted from their natural course a vast number of the meteors. But however great this number may be, the number of those which were too far off to feel any influence from the earth is immeasurably greater. In fact, the meteoric stream is about as long as from Jupiter to the earth, so that the earth when it passes through the stream can affect but a very short piece of its whole length.

Those Leonids that have been thus affected are they that have

since become sporadic Leonids. They traverse new orbits a little differing from the great meteoric orbit, and also differing from one another. By a well-known dynamical law, they would, if subsequently acted on only by the sun's attraction, return accurately at the end of each revolution to the situation close to the earth's path which they occupied when the earth, after having dragged them a little aside, passed on along its own orbit. Since the sun's attraction upon them is immensely more powerful than any other, they, on the completion of every revolution, return *nearly* to that situation, which the earth passes each year in the middle of November; but since their motions are slightly perturbed, especially by the great planets Jupiter and Saturn, they get to be somewhat scattered into situations behind and in front of that point in the earth's orbit, as well as, no doubt, many of them sideways. so that a few of them may encounter, though many more of them must escape, the earth. This scattering of the sporadic Leonids is what causes the earth to meet with a few of them for some days before and after it reaches the point of intersection of its orbit with that of the main swarm.

Again, when the earth diverts a meteor from its path, it slightly alters every element of its orbit. Among others, it alters its periodic time. Hence in each subsequent revolution the meteor which has been disturbed will either draw ahead of the main swarm or fall behind it; and this has caused the sporadic meteors to be now distributed round the whole length of the orbit, so that the earth encounters some of them every year, and not only at intervals of 33 years.

Such is a sufficient picture of what happens in the case of ordinary sporadic Leonids. But there is one among them which is so peculiar that it deserves separate treatment.

Of Tempel's Comet.

Astronomers know very little of the dynamics of comets, very little of the dynamics of clusters of stars, and almost nothing of the dynamics of nebulæ. When any one of these problems shall be solved, it will probably throw much light on the other two. Meanwhile, whatever may be the dynamical relation in which the tail of a comet stands to its nucleus and to the other bodies of the solar system, we know at all events that its nucleus travels along an orbit under the same laws as an ordinary mass of ponderable matter. Now the orbit of the nucleus of Tempel's comet is nearly but not quite coincident with that of the main swarm of November meteors, as appears from the following table of the best determinations we yet have of the elements of both orbits.

	Leonids.		Tempel's Comet.
Period	33·25	..	33·18 years
Mean distance	10·3402	..	10·3248
Excentricity	0·9047	..	0·9054
Perihelion distance	0·9855	..	0·9765
Inclination	16° 46′	..	17° 18′
Longitude of node	51° 28′	..	51° 26′
Distance of perihelion from node ..	6° 51′	..	9° 2′

will be observed that each of the elements of the orbit of el's comet differs, but only differs a little, from the correspond- ment of the orbit of the meteors. Differences of this kind have ished themselves in the case of every one of the sporadic rs which have got separated from the main swarm by the earth. his gives rise to the suspicion, almost amounting to belief, he comet was at one time a member of the swarm, and was a little aside on one of the occasions when the earth passed gh the stream. Since that event Jupiter and Saturn have ncessantly perturbing its orbit and that of the meteors a little ntly, and have thus increased the divergence. Now, if we can ine the orbit of the comet with great accuracy, it will become le to ascertain with precision what these perturbations have n the last few centuries, and thus to trace back the path which met has pursued in space. If this can be done satisfactorily, ll be able to find when it was that the comet was so close to rth that the earth was able to alter its whole future history. is another problem which the lecturer invited astronomers to fore them, and in order to prepare for it, to make the most bservations that are practicable upon the comet on the occasion pproaching return.

The Main Swarm.

e may next turn to the main swarm. The inclination of the of the meteors to the planes in which Jupiter and Saturn travel en referred to above. The meteors, on account of this inclined n of their orbit, glide at a distance of many millions of miles nd under the orbits of those planets, and the planets, as they hrough the inclined orbit of the meteors, are favourably situated odifying that orbit by their attraction. One of the principal that they thus occasion is to make the meteoric orbit, instead nding out from the sun in one fixed direction, to shift slowly in the same direction in which the planets travel round the This shifting of the orbit of the meteors has caused the time the earth encounters the swarm to have gradually advanced October 12th (Old Style), when the earth encountered the swarm , 902 (this being the first visit of the meteors of which we a record) until November 13th (New Style), when the great r of 1866 was discharged upon the earth. The point on the orbit where the meteors' orbit intersects is called the node of eteors' orbit. Accordingly, the facts are usually described by that the node of the meteoric orbit has shifted forwards along rth's orbit from the place which the earth reaches each Octo- th, which is equivalent in the new style to the date which was October 12th in A.D. 902, until November 13th or 14th, which present date. Thus the shift forwards in a thousand years of te on which the showers occur has been about three weeks and a nd we know that a similar shift must have been going on before

that time. A diagram illustrating these facts will be found in vol. ix. of the Proceedings of the Royal Institution, opposite to page 43.

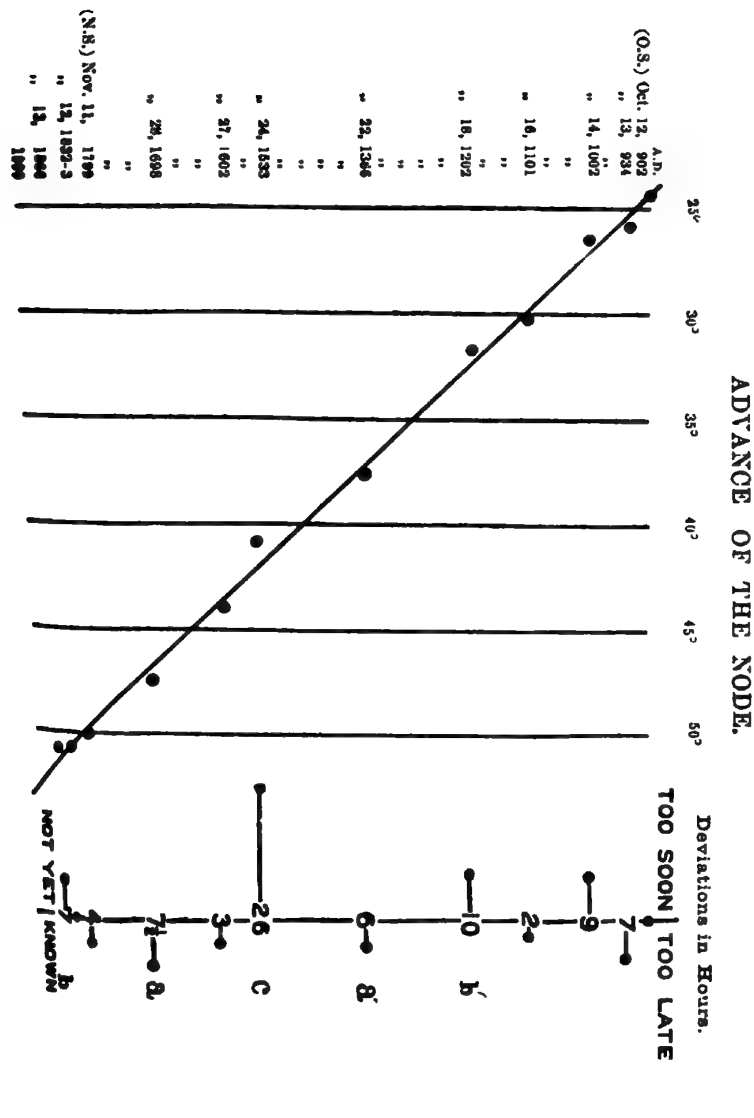
It was by a study of this advance of the node, and by referring it to its dynamical cause, that Professor Adams was able to discriminate between five different orbits which had been found by Professor Hubert Newton to be compatible with all other known facts. This enabled him, in April 1867, to announce which was the real orbit.

Professor Adams, in his computations, used a method of investigation known as Gauss's method, in which what he really computed was the perturbing effect on a meteor of two rings of attracting matter with the form, size and position of Jupiter's and Saturn's orbits, the masses of the rings being equal to the masses of the planets, and being distributed round the ring not equally, but with a preponderance where the planet, in travelling along its orbit, lingers longest. Now the actual amount by which the node shifts between successive returns of the meteors differs slightly from revolution to revolution; because the amount in any one revolution depends on what have been the distances and directions of the planets from the meteors during that particular revolution. But what Gauss's method does is to give the average amount of this shift taking one revolution with another, and this will in some revolutions be a little more, and in others a little less, than the actual amount. The difference between the actual and the average amount is well exemplified by the annexed diagram of the times at which the great showers have been observed, and the times at which they would have occurred if the advance of the node had not deviated from its average amount.

In the left-hand part of the diagram the longitudes of the node along the earth's orbit corresponding to the observed dates of the showers are plotted down. These show an irregular advance of the node towards the right-hand side of the figure. The straight line indicates where the node would have been if its advance had been uniform; and in the right-hand part of the figure are given the number of hours by which the actual shower preceded or followed the time when it would have occurred on the uniform hypothesis.

Now there is nothing except the want of more accurate data than we yet possess to prevent the calculation being carried farther than it was by Professor Adams, and made to furnish the actual amount of the shift in each individual revolution; indicating not only that, but the small difference which must exist between the perturbations upon the front, the middle and the back of the stream, so as to enable us to determine the sinuosities which must have established themselves in it.

There is a circumstance to which it may be useful to invite attention in connection with the calculation of the perturbations of the Leonids. The planets that are massive enough and so situated as to be able to affect the meteoric orbit are Jupiter, Saturn and Uranus, and in every one of these cases there is a remarkably simple numerical relation between the periodic time of the Leonids and that


ADVANCE OF THE NODE.
Deviations in Hours.
A.D.
(O.S.) Oct. 12, 902
" 13, 934
" 14, 1002
" 16, 1101
" 18, 1202
" 22, 1366
" 24, 1533
" 27, 1602
" 28, 1698
(N.S.) Nov. 11, 1799
" 12, 1832-3
" 13, 1866
25°
30°
35°
40°
45°
50°
TOO SOON
TOO LATE
NOT YET
KNOWN

of the planet perturbing their motions. The most conspicuous of these relations is that 14 revolutions of Jupiter in his orbit occupy almost exactly the same time as five revolutions of the Leonids—probably exactly the same time as five revolutions of those meteors which occupy the foremost position in the procession. This remarkable cycle has, therefore, been repeated as many as ten times since the year A.D. 126, when it is supposed that the meteors entered the solar system. Similar relations exist between the periodic time of the Leonids and those of the planets Saturn and Uranus. Now students of what is known as the "Planetary Theory" are aware that numerical relations of this kind produce a very marked effect on the perturbations, tending to make the perturbations in a short limited time conspicuously different from their mean values, and rendering it all the more necessary in the interests of physical astronomy that such observations shall be made and such data collected when the great stream returns to us, as will enable the computations to be made for each revolution separately.

At present we can only predict the return of a shower from our knowledge of the average amount of the shift of the node, and the time so determined is, as we see from the diagram, usually several hours before or after the actual time. If we could calculate the perturbations in a single revolution we should be in a position to compute the actual time. Even making use of the elements of the orbit as already determined by Professor Adams from imperfect data, it would probably be possible to make a moderate approximation to the amount of the perturbations between 1866 and 1899, so as to be able to come nearer to ascertaining the hour at which the next meteoric shower will commence than we can at present. It is to be hoped that this eminently useful computation will be made before November 1898, since it is possible that the head of the swarm will have reached the earth's orbit by that time.

But still more important information may emerge if we can calculate with sufficient accuracy the perturbations in individual revolutions. It will become possible to explore the past, to trace back the history not only of the meteoric procession as a whole, but of each part of it, and so ascertain with certainty when and through what instrumentality it was that these foreigners annexed themselves to the solar system. Similar information may be won in reference to Tempel's comet. We may discover when and on what occasion this body broke away from the main stream. These, if they can be effected, will be great achievements, and will show the observers and mathematicians of the present generation to be worthy successors of the great men—Professors Adams, Hubert Newton, Le Verrier and Schiaparelli—who made careful preparation before the return of the meteors in 1866, so that the most instructive observations might then be attempted, or who afterwards made use of the materials so collected to splendid effect.

The Observations now most wanted.

The immediate lessons we seem to learn from the whole survey are, that while observations upon sporadic Leonids are of little importance, the utmost efforts should be made to determine with more accuracy than has hitherto been possible the radiant point of each of the different parts of the main stream through which the earth will pass in 1899 and 1900, and perhaps in 1898. Every method, both by direct observation and by photography, should be carefully planned beforehand, and employed when the critical opportunity comes. It is of special importance that the observations shall be divided into sections, each extending over a short time—say not more than a quarter of an hour—and that a careful record be kept of the times of the several sections of observations, in order that it may be possible afterwards for the mathematician to compute and allow for the amount of deflection effected by the earth's attraction upon the meteors observed in each of those sections of time. This is a very necessary improvement upon the methods used in 1866. It is indeed essential where our aim is to attain great accuracy. Now very much greater accuracy in the observations than that which was attained in 1866 is imperatively required for the dynamical calculations which it is desirable that our mathematicians should be enabled to grapple with.

The matters, then, that are most immediately pressing are:—

1. To make preparation with the utmost forethought for the observations on the main stream, especially for the determinations of the radiant point in each quarter of an hour.

2. To make the fullest and most careful observations that are possible upon Tempel's comet. Some of these may probably be by photography.

3. To compute, so far as can be accomplished with our present materials, the perturbations which the planets Jupiter, Saturn and Uranus have effected on the orbit of the Leonids between November 1866 and the present time.

[G. J. S.]

WEEKLY EVENING MEETING,

Friday, February 26, 1897.

Basil Woodd Smith, Esq. F.R.A.S. F.S.A. Vice-President, in the Chair.

Lieut.-Colonel C. R. Conder, R.E. D.C.L. LL.D. M.R.A.S.

Palestine Exploration.

The object of exploration is to obtain accurate knowledge of a country, its inhabitants, and its extant monuments and texts. That of Palestine has special interest to Christian races and to Jews, as serving to explain more clearly the sacred literature of their Faith.

The results of such exploration may be judged by looking back a century to the time of Bayle, Voltaire and Astruc, when what was regarded as advanced scientific work assumed that the Hebrews were a savage race without literature, that history only began to be written about 500 B.C., and that the oldest civilisation was that of China and India. It is now known that the art of writing was practised in Egypt and Chaldea as early as 3000 B.C., that the Canaanites about the time of Joshua had a civilisation equal to that of surrounding nations, as had also the Hebrew kings; while, on the other hand, Chinese civilisation is only traceable to about 800 B.C., and that of India was derived from the later Persians, Arabs and Greeks. These results are due solely to exploration.

The requirements for exploration demand a knowledge not only of Syrian antiquities but of those of neighbouring nations. It is necessary to understand the scripts and languages in use, and to study the original records as well as the art and architecture of various ages and countries. Much of our information is derived from Egyptian and Assyrian records of conquest, as well as from the monuments of Palestine itself. As regards *scripts*, the earliest alphabetic texts date only from about 900 B.C., but previous to this period we have to deal with the cuneiform, the Egyptian, the Hittite and the Cypriote characters. The explorer must know the history of the cuneiform from 2700 B.C. down to the Greek and Roman age, and the changes which occurred in the forms of some 550 characters originally hieroglyphics, but finally reduced to a rude alphabet by the Persians, and used not only in Babylonia and Assyria but also as early as 1500 B.C. in Asia Minor, Syria, Armenia, Palestine, and even by special scribes in Egypt. He should also be able to read the various Egyptian

scripts—the 400 hieroglyphics of the monuments, the hieratic, or running hand of the papyri, and the later demotic. The Hittite characters are quite distinct and number at least 130 characters, used in Syria and Asia Minor from 15 0 B.C., or earlier, down to about 700 B.C. The study of these characters is in its infancy. The syllabary of Cyprus was a character derived from these Hittite hieroglyphics, and used by the Greeks about 300 B.C. It includes some fifty characters, and was probably the original system whence the Phœnician alphabet was derived. As regards alphabets, the explorer must study the early Phœnician, and the Hebrew, Samaritan and Moabite, with the later Aramean branch of this alphabet, whence square Hebrew is derived. He must also know the Ionian alphabet, whence Greek and Roman characters arose, and the early Arab scripts—Palmyrene, Nabathean and Sabean, whence are derived the Syriac, Cufic, Arabic and Himyaritic alphabets.

As regards *languages*, the scholars of the last century had to deal only with Hebrew, Aramaic, Syriac, Coptic and Greek, but as the result of exploration we now deal with the Ancient Egyptian whence Coptic is derived, and with various languages in cuneiform script, including the Akkadian (resembling pure Turkish) and the allied dialects of Susa, Media, Armenia and of the Hittites; the Assyrian, the earliest and most elaborate of Semitic languages; and Aryan tongues, such as the Persian, the Vannic and the Lycian.

The *art and architecture* of Western Asia also furnishes much information as to religious ideas, customs, dress and history, including inscribed seals and amulets, early coins and gems. The explorer must also study the remains of Greek, Roman, Arab and Crusader periods, in order to distinguish these from the earlier remains of the Canaanites, Phœnicians, Hebrews, Egyptians and Assyrians, as well as the art of the Jews and Gnostics about the Christian era, and the later pagan structures down to the fourth century A.D.

The monuments actually found in Palestine are few though important. The discovery at Tell el Amarna of about 150 letters written by Phœnicians, Philistines and Amorites—and in one case by a Hittite Prince—to the kings of Egypt, proves, however, the use of cuneiform on clay tablets by the Syrians as early as 1500 B.C., and one such letter has been recovered in the ruins of Lachish. The oldest monuments referring to Syria and Palestine are found at *Tell Loh*, on the Lower Euphrates, and date from 2700 B.C. Next to these are the *Karnak* lists of Thothmes III. about 1600 B.C., recording the names of 119 towns in Palestine conquered after the defeat of the Hittites at Megiddo. These lists show that the town names which occur in the Bible are mainly Canaanite and were not of Hebrew origin. The Canaanite language of this period was practically the same as the Assyrian, excepting that of the Hittites, which was akin to the Akkadian. In the next century the Tell el Amarna tablets show that the Canaanites had walled cities, temples, chariots, and a fully developed native art. They record the defeat of the

Egyptians in the north by Hittites and Amorites, and the invasion of the south by the Abiri, in whom Drs. Zimmern and Winckler recognise the Hebrews, the period coinciding with the Old Testament date for Joshua's conquest.

An inscription of Mineptah, discovered in 1896, speaks of the Israelites as already inhabiting Palestine about 1300 B.C., and agrees with the preceding. Other Egyptian records refer to the conquests of Rameses II. in Galilee and in Syria, when the Hittites retained their independence; and in the time of Rehoboam, Shishak has left a list of his conquests of 133 towns in Palestine, including the names of many towns noticed in the Bible.

The Hittite texts found at Hamath, Carchemish and Merash, as well as in Asia Minor, belonged to temples, and accompany sculptures of religious origin. They are still imperfectly understood, but the character of the languages, the Mongol origin of the people, and the equality of their civilisation to that of their neighbours, have been established, while their history is recovered from Egyptian and Assyrian notices. The Amorites were a Semitic people akin to the Assyrians, and their language and civilisation are known from their own records, while they are represented at Karnak with Semitic features.

The oldest alphabetic text is that of the Moabite stone about 900 B.C. found at Dibon, east of the Dead Sea, on a pillar of basalt, and recording the victories of King Mesha over the Hebrews, as mentioned in the Bible. Several Bible towns are noticed, with the name of King Omri, and the language, though approaching Hebrew very closely, gives us a Moabite dialect akin to the Syrian, which is preserved in texts at Samalla, in the extreme north of Syria, dating from 800 B.C. The Phœnician inscriptions found at Jaffa, Acre, Tyre, Sidon, Gebal and in Cyprus do not date earlier than 600 B.C., and show us a distinct dialect less like Hebrew than the Moabite. The most important of these early texts is the Siloam incription in the rock-cut aqueduct above the pool, found by a Jewish boy in 1880. It refers only to the cutting of the aqueduct (in the time of Hezekiah), but it gives us the alphabet of the Hebrews and a language the same as that of Isaiah's contemporary writings. It is the only true Hebrew record yet found on monuments, and confirms the Old Testament account of Hezekiah's work.

The Assyrian records refer to the capture of Damascus by Tiglath Pileser III. in 732 B.C., and of Samaria in 722 B.C., as well as to Sennacherib's attack on Jerusalem in 702 B.C. The latter record witnesses also the civilisation of the Hebrews under Hezekiah, whose name occurs as well as those of Jehu, Azariah, Menahem, Ahaz, Pekah and Hosea, who, with Manasseh, gave tribute to Assyrian kings.

About the Christian era Greek texts occur in Palestine, the most important being that of Herod's Temple at Jerusalem, forbidding strangers to enter, and those of Siah in Bashan, where also Herod

erected a temple to a pagan deity. Such texts are very numerous in Decapolis, where a Greek population appears to have settled in the time of Christ.

The geographical results of exploration are also important for critical purposes. Out of about 500 towns in Palestine noticed in the Old Testament, 400 retain their ancient names, and about 150 of these were unknown before the survey of the country in 1872–82. The result of these discoveries has been to show that the topography of the Bible is accurate, and that the writers must have had an intimate knowledge of the land. Among the most interesting Old Testament sites may be mentioned Lachish, Debir, Megiddo, Mahanaim, Gezer and Adullam as newly identified; and of New Testament sites, Bethabara, Ænon and Sychar, all noticed in the fourth Gospel.

The existing Hebrew remains are few as compared with Roman, Arab and Norman ruins of later ages. They include tombs, aqueducts and fortress walls, with seals, weights and coins. The most important are the walls of the outer court of Herod's great temple at Jerusalem, with his palace at Herodium, and buildings at Cæsarea and Samaria. The curious semi-Greek palace of Hyrcanus at Tyrus in Gilead dates from 176 B.C. In Upper Galilee and east of Jordan there are many rude stone monuments—dolmens and standing stones—probably of Canaanite origin, as are the small bronze and pottery idols found in the ruins of Lachish. Sculptured bas-reliefs are, however, not found in Palestine proper, having been probably destroyed by the Hebrews.

This slight sketch may suffice to show the advance in knowledge due to exploration during the last thirty years. The result has been a great change in educated opinion as to the antiquity of civilisation among the Hebrews and Jews, and as to the historic reliability of the Bible records. Further exploration, especially by excavation, may be expected to produce yet more interesting results, and deserves general support, as all classes of thinkers agree in the desirability of increasing actual knowledge of the past. It is no longer possible to regard the Hebrews as an ignorant and savage people, or to consider their sacred writings as belonging necessarily to the later times of subjection under the Persians. Internal criticism is checked and controlled by the results of exploration, and by the recovery of independent historical notices.

[C. R. C.]

GENERAL MONTHLY MEETING.

Monday, March 1, 1897.

SIR JAMES CRICHTON-BROWNE, M.D. LL.D. F.R.S. Treasurer and Vice-President, in the Chair.

Frederick John Beaumont, Esq.
Major Charles Turner Blewitt, R.A.
John Fowler Leece Brunner, Esq.
James Cadett, Esq.
John Corrie Carter, Esq.
John Cohen, Esq.
Mrs. Thomas Collier,
John George Craggs, Esq.
Thornycroft Donaldson, Esq. M.A.
Henry Edmunds, Esq.
Mrs. Henry Edmunds,
Gilbert Strange Elliot, Esq.
William Adams Frost, Esq. F.R.C.S.
William Terrell Garnett, Esq. J.P.
Henry Andrade Harben, Esq.
Frederic Howitt, M.D.
F. W. Hildyard, Esq.
Mrs. George King,
Henry Leitner, Esq.
Rev. James Dunne Parker, LL.D. D.C.L. F.R.A.S.
E. Mumford Preston, Esq.
John Morgan Richards, Esq.
Colonel George Sartorius,
Frederick Holland Schwann, Esq. B.A. LL.B.
William Robert Smith, M.D. D.Sc. F.R.S.E.
Henry Alfred Stern, Esq. M.A.
Charles John Stewart, Esq.
George Lawrence Stewart, Esq.
Mrs. Augustus D. Waller,
Mrs. J. Lawson Walton,

were elected Members of the Royal Institution.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FOR

Accademia dei Lincei, Reale, Roma—Classe di Scienze Fisiche, Matematiche e Naturali. Atti, Serie Quinta: Rendiconti. 1° Semestre, Vol. VI. Fasc. 2. Classe di Scienze Morali, &c. Serie Quinta, Vol. V. Fasc. 11, 12. 8vo. 1896–97.

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WEEKLY EVENING MEETING,

Friday, March 5, 1897.

Sir Frederick Bramwell, Bart. D.C.L. LL.D. F.R.S. Honorary Secretary and Vice-President, in the Chair.

Shelford Bidwell, Esq. M.A. LL.B. F.R.S. *M.R.I.*

Some Curiosities of Vision.

The function of the eye, regarded as an optical instrument, is limited to the formation of luminous images upon the retina. From a purely physical point of view it is a simple enough piece of apparatus, and, as was forcibly pointed out by Helmholtz, it is subject to a number of defects which can be demonstrated by the simplest tests, and which would, in a shop-bought instrument, be considered intolerable.

What takes place in the retina itself under luminous excitation, and how the sensation of sight is produced, are questions which belong to the sciences of physiology and psychology; and in the physiological and psychological departments of the visual machinery we meet with an additional host of objectionable peculiarities from which any humanly constructed apparatus is by the nature of the case free.

Yet in spite of all these drawbacks our eyes do us excellent service, and provided that they are free from actual malformation and have not suffered from injury or disease, we do not often find fault with them. This, however, is not because they are as good as they might be, but because with incessant practice we have acquired a very high degree of skill in their use. If anything is more remarkable than the ease and certainty with which we have learnt to interpret ocular indications when they are in some sort of conformity with external objects, it is the pertinacity with which we refuse to be misled when our eyes are doing their best to deceive us. In our earliest years we began to find out that we must not believe all we saw: experience gradually taught us that on certain points and under certain circumstances the indications of our organs of vision were uniformly meaningless or fallacious, and we soon discovered that it would save us trouble and add to the comfort of life if we cultivated a habit of completely ignoring all such visual sensations as were of no practical value. In this most of us have been remarkably successful, so much so that, if from motives of curiosity or for the sake of scientific experiment, we wish to direct our attention to the sensations in question and to see things as they actually appear, we can

only do so with the greatest difficulty; sometimes, indeed, not at all, unless with the assistance of some specially contrived artifice.

I propose to-night to discuss a few of the less familiar vagaries of the visual organs, and will do my best to assist in the illustration of them. But it will be my part merely to provide the apparatus for the experiments; the experiments themselves must be carried out by each of you individually. Some of them will, I am afraid, be found rather difficult; success will depend mainly upon your power of laying aside habit and prejudice and giving close attention to your visual sensations. I hardly dare to hope that every one present will observe all the peculiarities and defects which it is intended to demonstrate, but in case of failure I generally find that there is a comfortable tendency to attribute it not to any deficiency in the observer's power of concentrating his attention, but to the fact that his eyes are not as other mens', and are free from the particular defect which it is desired to bring into prominence. Of course any one is welcome to such an entirely satisfactory opinion.

Among the most annoying of the eccentricities which characterise the sense of vision is that known as the persistence of impressions. The sensation of sight which is produced by an illuminated object does not cease at the moment when the exciting cause is removed or changed in position, but continues for a period which is generally said to be about $\frac{1}{10}$ second, but may sometimes be much more or less. It is for this reason that we cannot see the details of anything which is in rapid motion, but only an indistinct blur, resulting from the confusion of successive impressions. When I turn this disc, which is painted in black and white sectors, you soon lose sight of the divisions, and if the speed is high enough the whole surface appears to be of a uniformly grey hue. If we illuminate the rotating disc by a properly timed series of electric flashes, it looks as if it were at rest, and in spite of the intermittent nature of the light, the black and white sectors are seen quite continuously, though as a matter of fact the intervals of darkness are very much longer than those of illumination.

The persistent impressions which we have been discussing are often spoken of as positive after-images.

There is one very remarkable phenomenon accompanying the formation of positive after-images, especially those following brief illumination, which seems, until comparatively recent times, to have entirely escaped the notice of the most acute observers. It was first observed accidentally by Prof. C. A. Young, when he was experimenting with a large electrical machine which had been newly acquired for his laboratory. He noticed that when a powerful Leyden jar discharge took place in a darkened room, any conspicuous object was seen twice at least, with an interval of a trifle less than a quarter of a second, the first time vividly, the second time faintly. Often it was seen a third time, and sometimes, but only with great difficulty, even a fourth time. He gave to this phenomenon the name of recurrent

vision: it may perhaps be more appropriately denominated the Young effect.

We have here a machine presented to the Institution by Mr. Wimshurst, which is a giant in comparison with that used by Prof. Young, and I hope by its means to be able to show the effect to every one present who will give a little attention. Look in the direction of some object which is exposed to the light of the discharge: the object will be seen for an instant at the moment when the spark passes and you hear the crack, and after a dark interval of about ⅕ second it will make another brief appearance. Some of you may perhaps see even a second recurrent image. Under certain conditions I myself have observed no less than six reappearances of an object which was illuminated by a single discharge.

Twelve years ago I called attention to a very different method of exhibiting a recurrent image. The apparatus used for the purpose consists of a vacuum tube mounted in the usual way upon a horizontal axis capable of rotation. When the tube is illuminated by a rapid succession of discharges from an induction coil, and is made to rotate very slowly (at the rate of about one turn in two or three seconds) a very curious phenomenon may be noticed. At a distance of a few degrees behind the tube, and separated from it by a clear interval of darkness, comes a ghost. This ghost is in form an exact reproduction of the tube: it is very clearly defined, and though its apparent luminosity is feeble, it can no doubt be easily seen by most of you. The varied colours of the original are, however, absent, the whole of the phantom tube being of a uniform bluish or violet tint. If the rotation is suddenly stopped, the ghost still moves steadily on until it reaches the luminous tube, with which it coalesces and so disappears. (See Fig. 1, where the recurrent image is indicated by dotted lines.)

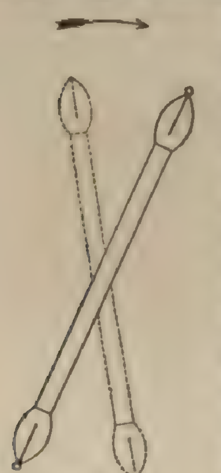

Fig. 1.

I returned to the subject three or four years ago, with the primary object of ascertaining whether or not the Young effect was identical with one which had recently been discovered by Charpentier, and which will be referred to presently. A certain phenomenon which I had attributed to the Young effect was quoted by Charpentier as exemplifying his own newly-observed one. I found, however, that the two effects, though both of an oscillatory character, were in fact quite distinct from one another. The results of my experiments in relation to this and other allied matters were embodied in a communication to the Royal Society.*

* Proc. Roy. Soc. vol. lvi. p. 132 (1894).

In investigating the influence of colour upon the Young effect, two methods of experimenting were employed. In the first, coloured light was obtained by passing white light through coloured glasses; in the second and more perfect series of experiments, the pure coloured light of the spectrum was used. Among other results, it was found that *ceteris paribus* the recurrent image was much stronger with green light than with any other, and that when the excitation was produced by pure red light, however intense, there was no recurrent image at all.

I intend to attempt a repetition of my first experiment before you. A metal disc with a small circular aperture near its edge is placed in the lantern, and its image projected upon the screen. When the disc is turned slowly the spot of light upon the screen goes round and round, and some of you may, perhaps, be able to see at once that the bright primary spot appears to be followed at a short distance by a much feebler spot of a violet colour, which is the recurrent image of the first. It is essential to keep the direction of the eyes perfectly steady, which is not an easy thing to do without practice. (See Fig. 2.) If now we place a green glass before the lens, the ghost will be at its best, and all of you should be able to see it, provided that you do not look at it. With an orange glass the ghost becomes less distinctly visible, and its colour generally appears to be bluish-green instead of violet as before. When a red glass is substituted the ghost completely disappears. If the speed of rotation is sufficiently high, the red spot is considerably elongated during its revolution, and its colour ceases to be uniform, the rear portion assuming a light bluish-pink tinge. But however great the speed, no complete separation of the spot into red and pink portions can be effected, and no recurrent image is ever formed.

Fig. 2.

The spectrum method of observation can only be carried out on a small scale, and cannot be exhibited to an audience. It, however, affords the best means of ascertaining how far the apparent colour of the recurrent image depends upon that of the primary, a matter of some theoretical interest. I found that white light was followed by a violet recurrent image; after blue and green, when the image was brightest, its colour was also violet; after yellow and orange it appeared blue or greenish-blue. On the other hand, when a complete spectrum was caused to revolve upon the screen, the whole of its recurrent image from end to end appeared violet; there was no appearance of blue or greenish-blue at the less refrangible end. For this and other reasons it was concluded that the true colour was in all cases really violet, the blue and greenish-blue apparently seen in

conjunction with the much brighter yellow and orange of the primary being merely an illusory effect of contrast. [This contrast effect was illustrated by a lantern slide.] It seems likely, then, that the effect which has been spoken of as recurrent vision, is due principally, if not entirely, to an action of the violet nerve fibres. It need hardly be pointed out that it represents only a transient phase of the well known positive after-image, and it had even been observed in a vague and uncertain sort of way long before the date of Prof. Young's experiment. Helmholtz, for example, mentions the case of a positive after-image which seemed to disappear and then to brighten up again; but he goes on to explain that the seeming disappearance was illusory.

M. Charpentier, of Nancy, whose name I have already mentioned, was the first to notice and record a remarkable phenomenon which, in some form or other, must present itself many times daily to every person who is not blind, but which, until about six years ago, had been absolutely and universally ignored. The law which is associated with Charpentier's name is this :—When darkness is followed by light, the stimulus which the retina at first receives, and which causes the sensation of luminosity, is succeeded by a brief period of insensibility, resulting in the sensation of momentary darkness. It appears that the dark period begins about $\frac{1}{60}$ second after the light has first been admitted to the eye, and lasts for about an equal time. The whole alternation from light to darkness and back again to light is performed so rapidly, that except under certain conditions, which, however, occur frequently enough, it cannot be detected.

FIG. 3.

The apparatus which Charpentier employed for demonstrating and measuring the duration of this effect is very simple. It consists of a blackened disc with a white sector mounted upon an axis. When the disc is illuminated by sunlight and turned rather slowly, there appears upon the white sector close behind its leading edge a narrow but well-defined dark band (See Fig. 3). The portion of the retina which is apparently occupied at any moment by the dark band is that upon which the light reflected by the leading edge of the white sector has fallen $\frac{1}{60}$ second previously.

But no special apparatus is required to show the dark reaction; it is, as I have said, an exceedingly common phenomenon. In Fig. 4 an attempt has been made to illustrate what any one may see if he simply moves his hand between his eyes and the sky or any strongly illuminated white surface. The hand appears to be followed by a dark outline separated from it by a bright interval. The same kind

of thing happens in a more or less marked degree whenever a dark object moves across a bright background, or a bright object across a dark background.

In order to see the effect distinctly by Charpentier's original method, the illumination must be strong. If, however, the arrangement is slightly varied, so that transmitted instead of reflected light is made use of, comparatively feeble illumination is sufficient. A very effective way is to turn a small metal disc having an open sector of about 60°, in front of a sheet of ground or opal glass behind which is a lamp. By an arrangement of this kind upon a larger scale, the effect may easily be rendered visible to an audience. The eyes should not be allowed to follow the disc in its rotation, but should be directed steadily upon the centre. [Experiment.]

The acute and educated vision of Charpentier enabled him, even when working with his black and white disc, to detect the existence,

Fig. 4.

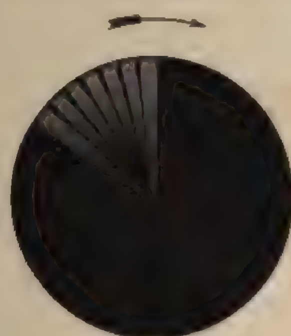

Fig. 5.

under favourable conditions, of a second, and sometimes a third dark band of greatly diminished intensity, though he remarks that the observation is a very difficult one. What is probably the same effect can, however, be shown quite easily in a different manner. If a disc with a very narrow radial slit $\frac{1}{50}$ inch or $\frac{1}{2}$ mm. wide, is caused to rotate at the rate of about one turn per second in front of a bright background, such as a sheet of ground glass with a lamp behind it, the moving slit assumes the appearance of a fan-shaped luminous patch, the brightness of which diminishes with the distance from the leading edge. And if the eyes are steadily fixed upon the centre of the disc, it will be noticed that this bright image is streaked with a number of dark radial bands, suggestive of the ribs or sticks of the fan. Near the circumference as many as four or five such dark streaks can be distinguished without difficulty; towards the centre they are less conspicuous, owing to the overlapping of the successive

images of the slit.* [The effect was demonstrated by means of a rotating disc in the lantern, and is roughly indicated in Fig. 5.]

The dark reaction known as the Charpentier effect, occurs at the beginning of a period of illumination. There is also a dark reaction of very short duration at the end of a period of illumination. I should explain that owing to what is called the proper light of the retina, ordinary darkness does not appear absolutely black: even in a dark room on a dark night with the eyes carefully covered, there is always some sensation of luminosity which would be sufficient to show up a really black image if one could be produced. Now the darkness which is experienced after the extinction of a light is for a small fraction of a second more intense than common darkness.

I believe that the first mention of this dark reaction occurs in the article which I contributed to 'Nature' in 1885, in which it was stated that when the current was cut off from an illuminated vacuum tube "the luminous image was almost instantly replaced by a corresponding image which appeared to be intensely black upon a less dark background," and which was estimated to last from $\frac{1}{4}$ to $\frac{1}{2}$ second. "Abnormal darkness," it was added, "follows as a reaction after the luminosity."

In the Royal Society paper to which I have before referred the point is further discussed, and a method is described by which the stage of reaction may be easily exhibited, and its duration approximately measured. If a translucent disc made of stout drawing-paper and having an open sector, is caused to rotate slowly in front of a luminous background, a narrow radial dark band like a streak of black paint appears upon the paper very near the edge which follows the open sector. From the space covered by this band when the disc was rotating at a known speed, the duration of the dark reaction was estimated to be about $\frac{1}{50}$ second. [The experiment was shown, and is illustrated in Fig. 6.]

Fig. 6.

One more interesting point should be noticed in the train of visual phenomena which attend a period of illumination. The sensation of luminosity which is excited when light first strikes the eye is for about $\frac{1}{50}$ second much more intense than it subsequently becomes. This is shown by the fact that the bright band intervening between the leading edge of the white sector of a Charpentier disc and the dark band, appears to be much more strongly illuminated than any other portion of the sector.

* Proc. Roy. Soc., vol. lvi. p. 142 (1894). A similar observation was described by Charpentier, Comptes Rendus, Jan. 1896.

I propose now to say a few words about a curious phenomenon of vision which occupied my attention towards the end of last year.*

Rather more than two years ago, Mr. C. E. Benham brought out a pretty little toy which he called the Artificial Spectrum Top. It consists of a cardboard disc, one half of which is painted black, while on the other half are drawn four successive groups of concentric black lines at different distances from the centre. When the disc rotates rather slowly each group of black lines generally appears to assume a different colour, the nature of which depends upon the speed of the rotation and the intensity and quality of the light. Under the best conditions the inner and outer groups of lines become bright red and dark blue; at the same time the intermediate groups also appear tinted, but the hues which they assume are rather uncertain and difficult to specify. By far the most striking of the colours exhibited by the top is the red, and next to that the blue; this latter, however, is sometimes described as bluish-green. [The top was exhibited as a lantern slide.]

My recent experiments seem to indicate pretty clearly the cause of the remarkable bright red colour and also that of the blue. The more feeble tints of the two intermediate groups of lines perhaps result from similar causes in a modified form, but these I have not yet investigated.

In the red colour we have another striking example of an exceedingly common phenomenon which is habitually disregarded; indeed, I can find no record of its ever having been noticed at all. The fact is, that whenever a bright image is suddenly formed upon the retina after a period of comparative darkness, this image appears for a short time to be surrounded by a narrow coloured border, the colour under ordinary conditions of illumination being red. If the light is very strong the transient border is greenish-blue. Sometimes both red and blue borders appear together, the blue being inside the red.† The colour generally seen is, however, red, and it is most conspicuous with good lamp-light.

This observation was first made in the following manner. A blackened zinc plate with a small round hole in it is fixed over a larger hole in a wooden board; the hole in the zinc is covered with a piece of thin white writing paper. Thus we are furnished with a sharply defined translucent disc which is surrounded by a perfectly opaque substance. An arrangement is made for covering the translucent disc with a shutter which can be opened very rapidly by means of a strong spring. If this apparatus is held between the eyes and a lamp, and the translucent disc is suddenly disclosed by working the shutter, the disc appears for a short time to be surrounded by a narrow red border. The width of the border is perhaps

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* Proc. Roy. Soc. vol. lx. p. 370 (1896).

† I have recently shown that the greenish-blue border is simply the "negative after-image" of the red one.—*April* 24th.

$\frac{1}{25}$ inch or 1 mm., and the appearance lasts for something like $\frac{1}{10}$ second. Most people are at first quite unable to recognise this effect, the difficulty being not to see it but to know that one sees it. Those who have been accustomed to visual observations generally perceive it without any difficulty when they know what to look for, and no doubt it would be quite evident to a baby a few weeks old, which had not advanced very far in the education of its eyes.

The observation is made rather less difficult by a further device. If the disc is divided into two parts by an opaque strip across the middle, it is clear that each half-disc will have its red border, and, if the strip is made sufficiently narrow, the red borders along its edges will meet, or perhaps overlap, and the whole strip will, for a moment after the shutter is opened, appear red. A disc was prepared by gumming across the paper a strip of tin foil about $\frac{1}{30}$ inch wide. The effect produced when such a disc is exposed is indicated in Fig. 7, the red colour being represented by shading.

FIG. 7.

A simpler apparatus is, however, quite sufficient for showing the effect,* and with practice one can even acquire the power of seeing it without any artificial aid at all. I have many times noticed flashes of red upon the black letters of a book that I was reading, or upon the edges of a page: bright metallic or polished objects often show it when they pass across the field of vision in consequence of a movement of the eyes, and it was an accidental observation of this kind which suggested the following easy way of exhibiting the effect experimentally.

An electric lamp was fixed behind a round hole in a sheet of metal which was attached to a board. The hole was covered with two or three thicknesses of writing paper, making a bright disc of nearly uniform luminosity. When this was moved rather quickly either backwards or forwards or round and round in a small circle,

* See 'Nature,' vol. lv. p. 367 (Feb. 18, 1897).

the edges of the streak of light thus formed appeared to be bordered with red. [Experiment shown.]

If this experiment is performed with a strong light, the hole becomes bordered with greenish-blue instead of red. With an intermediate degree of illumination both blue and red may be seen together, the blue being inside the red.

Most of the effects that have so far been described were produced by transmitted light, but reflected light will show them equally well. If you place a printed book before you near a good lamp and interpose a dark screen before your eyes, then, when the screen is suddenly withdrawn, the printed letters will for a moment appear red, quickly changing to black. Some practice is required before this observation can be made satisfactorily, but by a simple device it is possible to obliterate the image of the letters before the redness has had time to disappear; the colour then becomes quite easily perceptible. Hold two screens together side by side, a black one and a white one, in such a manner that there is a triangular opening left between them. In the first place let the black screen cover the printing, then quickly move the screens sideways so that the printed letters may be for a moment exposed to view through the gap, stopping the movement as soon as the page is covered by the white screen. During the brief glimpse that will be had of the black letters while they are beneath the gap, they will, if the illumination is suitable, appear to be bright red.

We may go a step further. Cut out a disc of white cardboard, divide it into two equal parts by a straight line through the centre, and paint one half black. At the junction of the black and white portions cut out a gap which may conveniently be of the form of a sector of about 45° (see Fig. 8). Stick a long pin through the centre and hold the arrangement by the pointed end of the pin a few inches above a printed page near a good light. Make the disc spin at the rate of about 5 or 6 turns a second by striking the edge with the finger. As before, the letters when seen through the gap will appear red, and persistence will render the repeated impressions almost continuous. Care must be taken that the disc does not cast a shadow upon the printing, and that the intensity of the illumination is properly adjusted. I have here several rather more elaborate contrivances for making discs rotate.

FIG. 8.

In none of these experiments does an extended black surface ever appear red, but only black dots or lines, which may of course have the form of letters. And the lines must not be too thick; if their thick-

ness is much more than $\frac{1}{25}$ inch or 1 mm. the lines, as seen by an observer at a distance of two or three feet, do not become red throughout but only along their edges. The red appearance is in fact not due to the black lines themselves at all; these serve merely as a background for showing up the red border which fringes externally the white portions of the paper, and the width of this border does not exceed about one-fifth of a degree.

[By means of a large rotating disc some designs in black lines and letters were made to appear red, the effect being visible in all parts of the theatre.]

When the disc is turned in the opposite direction, the black lines appear at first sight to become dark blue. Attentive observation, however, shows that the apparently blue tint is not formed upon the lines themselves as the red tint was, but upon the white ground just outside them. This introduces to our notice another border phenomenon which seems to present itself when a dark patch is suddenly

FIG. 9.

formed on a bright ground, for that is essentially what takes place when the disc is turned the reverse way. I made some attempts to obtain more direct evidence that such a dark patch appeared for a moment to have a blue border, and after some trouble succeeded in doing so.

A circular aperture was cut in a wooden board and covered with white paper: a lamp was placed behind the board, and thus a bright disc was obtained as in the former experiment. An arrangement was prepared by means of which one half of this bright disc could be suddenly covered by a metal shutter, and it was found that when this was done a narrow blue band appeared on the bright ground just beyond and adjoining the edge of the shutter when it had come to rest. The blue band lasted for about $\frac{1}{10}$ second, and it seemed to disappear by retreating into the black edge of the shutter. An attempt has been made to illustrate it in Fig. 9, where the shaded band indicates the blue border.

We have then to account, if possible, for the two facts that in the formation of these transient borders the red sensation occurs in a portion of the retina which has not been exposed to the direct action of light, while the blue occurs in a portion which is exposed to unchanged illumination. Accepting the Young-Helmholtz theory of colour vision, the effects must, I think, be attributed to a sympathetic affection of the red nerve fibres. When the various nerve fibres occupying a limited portion of the retina are suddenly stimulated by white or yellow light of moderate intensity, the immediately surrounding red nerve fibres are for a short period excited sympathetically, while the violet and green fibres are not so excited, or in a much less degree. And again, when light is suddenly cut off from a patch in a bright field, there occurs an insensitive reaction in the red fibres just outside the darkened patch, in virtue of which they cease for a moment to respond to the luminous stimulus: the green and violet fibres by continuing to respond uninterruptedly, give rise to the sensation of a blue border.

Whether or not the hypothesis which I have suggested is correct in all its details, it is, I think, sufficiently obvious that the red and blue colours of Benham's top are due to exactly the same causes as the colours observed in my own experiments, for the essential conditions are the same in both cases.

I have mentioned only a few among many curious phenomena which have presented themselves in the course of my investigation. It is not improbable that a careful study of the subjective effects produced by intermittent illumination would lead to results tending to clear up many doubtful points in the theory of colour vision.

[S. B.]

WEEKLY EVENING MEETING,

Friday, March 12, 1897.

Sir Frederick Abel, Bart. K.C.B. D.C.L. LL.D. F.R.S.
Vice-President, in the Chair.

Professor Arthur Smithells, B.Sc. F.I.C.

The Source of Light in Flames.

When hydrogen burns in oxygen the gases unite to form steam, and a flame of simple structure is obtained. The light is of very feeble intensity, so feeble when the hydrogen is highly purified and when both gases are free from dust, that the flame is scarcely visible in a room from which all other light is excluded.*

To what is the light of this flame due? It is not sufficient to say that it is the result of chemical action attended by the evolution of much heat. Light is of an undulatory nature, and the undulations arise during an oscillatory process associated with matter. We desire to know with what particular kind of atoms or molecules the light of a hydrogen flame is associated. It may be said that when hydrogen combines with oxygen the heat that is produced is necessarily contained, as it were, in the steam, and that therefore it is the steam that glows. This raises the question as to what evidence we have, apart from flames, of the possibility of making gases glow by the simple process of heating them. The evidence is nearly all negative. None of the common gases, including those contained in the best known flames, have been made to glow when heated by a purely baking or roasting process to the highest obtainable temperature. The passage of an electric discharge through the gases is not to be regarded as merely a heating process.

Among the gases that can be made to glow, the most conspicuous is iodine. The vapour of this substance shows a distinct red glow at a temperature below that at which glass is visibly red.† [Experiment shown.] It is possible that some chemical action, namely, dissociation and recombination, may be in progress in the iodine vapour, and that the emission of light may be due to this. A similar glow, however, has been obtained with bromine, and, to a less extent, with chlorine,‡ at temperatures which exclude the likelihood of dissociation.

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* Stas, Œuvres, tome iii. p. 228.
† Salet, 'Analyse Spectrale,' p. 173; see also Phil. Mag. [v] 37, p. 245 (1894).
‡ Evershed, Phil. Mag. [v] 39, p. 460 (1895).

The great difficulty, and in most cases the present impossibility, of making gases glow by a mere increase of temperature of a direct kind, leads us to hesitate before we say that a hydrogen flame glows merely because it contains hot steam. The matter may be considered from another point of view. When hydrogen burns, the atoms of hydrogen are combining chemically with atoms of oxygen. It is impossible to picture this process with any certainty of detail, but we do know that the uncombined atoms have a store of energy which is set free or becomes perceptibly kinetic when they combine. This action takes place only when the atoms are within each other's sphere of chemical attraction, or, in other words, when the new substance begins to be formed. It seems impossible not to suppose that such a process entails in the substance that is being formed a condition as regards motion which must be considered apart from any condition of temperature which is exhibited by the flame as a whole. We cannot suppose, when a number of atoms commence to form a molecular system, that the liberation of their potential energy will result directly in increased translatory motion of the newly formed molecule. The process may be compared to two oppositely electrified spheres approaching one another rapidly in space in paths sufficiently close for the mutual attraction to determine their union into a system of revolution ending in actual contact. During the coalescence the system would be in a vibratory state.

Without propounding any hypothesis as to the nature of chemical energy, it seems certain that in the process of chemical union the newly formed substance is in a state that it will be very difficult, and perhaps even impossible, for it to acquire by what we ordinarily understand as an increase of temperature, and this state being oscillatory may well occasion the emission of light.

The oscillatory motion will be short lived and will disappear in two ways, first in producing radiations, and secondly and chiefly, in communicating to other impinging molecules, and thereby to itself, an increased translatory motion which corresponds to increase of temperature. According to this view the emission of light by a burning gas is antecedent to, rather than consequent upon, a high temperature, if we used this last term in its ordinary sense.

If the number of molecules being formed in a flame at any instant is small compared with the number of other molecules in their immediate neighbourhood, we may have a flame in which the emission of light is associated with a low general temperature. This case arises with substances that enter into combination freely at low temperatures. A stream of carbon dioxide charged with a little phosphorus vapour produces a bright green flame when it issues into the air. The light is due to phosphoric oxide, that is to say, it is the formation of phosphoric oxide that occasions it. Much energy is liberated during the formation of each molecule, but the luminous molecules are so far apart, there are so many molecules of carbon dioxide round them, that the average temperature is quite incon-

siderable, and the finger perceives no heat when held in the flame. If the supply of phosphorus vapour be increased the number of luminous molecules increases, the light becomes brighter, and the temperature also rises in due proportion.

In the case of hydrogen, which does not ignite at a low temperature, it is impossible to get a cool sheet of flame, for by the addition of a neutral gas, the molecules of nascent steam are soon separated to such an extent that the energy liberated is insufficient to keep the general temperature of the sheet up to the point required to stimulate sufficiently the combination of the incoming hydrogen.

If the shell of burning gas, which constitutes what may be called the foundation of a flame, is very hot, it is always possible that a secondary source of light may be developed, due to a purely baking process. This may affect the product of combustion itself, or the unburned gas or some decomposition product. We might thus anticipate that in the hydrogen flame light would come not only from the steam, which is being formed, but also from the hydrogen within the flame, which is subjected to intense roasting as it ascends. This, however, does not appear to be the case. The occurrence of the spectrum of hydrogen in that of the oxy-hydrogen flame was described by Plücker, but experiments undertaken by Professor Liveing,* specially to test this question, have decided it in the negative. The light of the oxy-hydrogen flame has been examined spectroscopically by Professors Liveing and Dewar, Dr. Huggins and others, and the spectrum is now attributed to water alone.

The light of a hydrogen flame is very feeble compared with that of most other flames. If we ask why this is so, we are asking almost the same question that eighty years ago impelled Sir Humphry Davy to the splendid researches which laid the foundation of our scientific knowledge of flames. And it was the same question that fifty years later led Dr. Edward Frankland to investigations of flame, which rank second only to those of his illustrious predecessor. Curious to know why an explosive mixture of coal gas and air within a safety lamp burned with a pale blue flame, whilst coal gas ordinarily burnt with a bright light, Davy, after a few simple experiments, concluded that he was correct in his first surmise, viz. "that the cause of the superiority of the light from the *stream* of coal-gas might be owing to a *decomposition* of a part of the gas towards the interior of the flame where the air was in smallest quantity, and the deposition of solid charcoal which, first by its *ignition* and afterwards by its *combustion*, increased in a high degree the intensity of the light." Davy's final and general conclusion was that "whenever a flame is remarkably brilliant or dense it may be always concluded that some solid matter is produced in it; on the contrary, when a flame is extremely feeble and transparent it may be inferred that no solid matter is formed."

* Phil. Mag. [v] 34, p. 371 (1892).

In 1867 Dr. Frankland, lecturing before the Royal Institution,* gave strong reasons for dissenting from Davy's views, both as to the cause of the luminosity of flames in general and of the flames of hydrocarbons in particular. Dr. Frankland's conclusions may be summarised as follows:—

(i.) Bright flames exist which do not contain solid particles.

(ii.) The luminosity of flames depends mainly on the density of the substances contained in them.

(iii.) Feebly luminous flames may be made bright by compressing the burning gases.

(iv.) The luminosity of ordinary hydrocarbon flames, such as that of coal gas, is not due in any important degree to solid particles of carbon, but almost entirely to the glow of dense hydrocarbon vapours.

Of these conclusions, two are beyond doubt. The flame of phosphorus, or of carbon-disulphide burning in oxygen, are examples of bright flames in which no solid matter can be supposed reasonably to exist. The explosion of electrolytic gas in a eudiometer resting on an india-rubber pad produces a bright light, the gas is hindered from expanding, and hence the flame travels through the mixture under increasing pressure.

A table, in Dr. Frankland's paper, shows the kind of evidence from which he concluded that the brightness of flames depends on the density of the substances they contain, and the general agreement of fact with theory is very striking. It is important to know whether the rule holds without exception, and whether it is in harmony with other general laws. There are flames containing dense substances which are not bright, and flames which are bright though they do not contain dense substances; but these apparent exceptions are explained by supposing that the temperature in one case is very low and in the other very high. If this kind of accommodation is permissible, Dr. Frankland's principle can hardly be submitted to a rigorous test.

The fact that the light of compressed flames is so intense can hardly be held to support the general doctrine in any rational sense, for it cannot be said either physically or chemically that two gases are in a like state when they have the same density. As a *fact* the increased luminosity here accompanying increased density is undeniable, and Dr. Frankland has contended for no more than this; but the matter must be looked at in the light of the molecular theory. This theory would lead us to expect increased light from a flame containing dense matter if the density were a result of molecular crowding, whilst it can at present tell us nothing about the effect likely to ensue from an increase of density arising from the greater

* Proc. Roy. Inst. 5, p. 419. The best account of Dr. Frankland's views is contained in six lectures delivered at the Royal Institution, and admirably reported in the 'Journal of Gas Lighting.'

weight of the individual molecules. For this reason Dr. Frankland's observations on compressed flames may be considered essentially unconnected with the observations on uncompressed flames containing substances of high molecular weight, though the results may be embodied in a single statement; and to this extent the generalisation loses importance.

The development of brightness in a flame may be conveniently studied in the flame of hydrogen phosphide. When this gas is sufficiently diluted with carbon dioxide, the flame has the same green glow as has been already noticed in the case of carbon dioxide charged with phosphorus vapour. This glow is to be ascribed to the formation of an oxide of phosphorus, and since phosphorus oxide itself glows in presence of oxygen with exactly the same light,* we may reasonably conclude that the oxide whose formation determines the glow is the pentoxide. If now the proportion of hydrogen phosphide to carbon dioxide be slightly increased, an entirely new kind of luminosity is developed in the flame towards the tip. This is at first yellowish, but increases in whiteness and brilliance as the supply of carbon dioxide is diminished, until finally, when the pure hydride is burning, the flame has the appearance of brightly burning phosphorus. This yellow or white light is to be regarded as secondary in origin, and to be the result of high temperature in the ordinary sense of the word. In confirmation of this it may be stated that the light appears in exactly that place where, considering the flame as a heating agent, the effective temperature would be highest; and further, if a ring of copper wire be placed horizontally in the lower part of the flame, so as to lower the general temperature, the yellow luminosity at once disappears just as it does when the flame is cooled by an increase in the supply of carbon dioxide. It is a matter of much interest to determine what substance emits the yellow or white light. It might be supposed to be due to phosphorus separated within the flame by decomposition of the hydrogen phosphide. In that case the introduction of oxygen into the middle of the flame might be expected to diminish the luminosity; but the reverse is the case. The glow appears to be due to phosphorus pentoxide, for if the flame of a Bunsen burner be held above the hydrogen phosphide flame the yellow-white glow is extended continuously upwards into the Bunsen flame. The track of the phosphorus pentoxide can in fact be seen in the form of a white glow so long as the temperature of the surroundings reaches a certain point. The absence of solid particles from a hydrogen phosphide flame can be shown by concentrating the sun's rays upon it.

In these experiments the use of hydrogen phosphide gives a convenient method of regulating the supply of phosphorus; they may be repeated with phosphorus vapour itself diluted with carbon dioxide, and the same results are obtained. It appears, therefore, that there

* Thorpe on 'The Glow of Phosphorus,' Proc. Roy. Inst. 13, p. 72 (1890).

are two luminous effects to recognise in the combustion of phosphorus. One is due to the act of formation of phosphorus pentoxide giving the green glow, and the other due to the subsequent heating of the same substance producing the white glow. Adopting the terminology suggested by E. Wiedemann, we may say that there is chemi-luminescence and thermo-luminescence of phosphorus pentoxide. In what is ordinarily called the phosphorescence of phosphorus we have the chemi-luminescence; in the vivid combustion of phosphorus the chemi-luminescence is completely overpowered and masked by the thermo-luminescence.

It is interesting to inquire how far other combustible elements behave in the same way. The flame of silicon hydride may be subjected to similar experiments. When sufficiently diluted with carbon dioxide a pale greenish flame is obtained, silica being the product. The green colour may therefore be attributed to the formation of this compound. When the supply of carbon dioxide is reduced the flame becomes brightly luminous, but the luminosity may be removed by cooling with a wire ring. The optical test shows the bright light to be due to solid particles, and as the glow is prolonged continuously in the track of the escaping silica when a Bunsen flame is held over the silicon hydride flame, it seems clear that the secondary or bright luminosity of the flame is here, as in the case of phosphorus, to be ascribed to a purely thermal action. The chief difference in the two instances is that in the case of phosphorus hydride the product is a glowing gas, and in the case of silicon hydride a glowing solid.

Hydrocarbon flames may also be considered from the same point of view, and here the facts are well known. In the first instance we have to recognise in a hydrocarbon flame the bright yellow light and the blue or lilac light. The bright yellow light may be suppressed by cooling by means of a wire or by diluting the gas with carbon dioxide. This part of the light of a hydrocarbon flame has frequently been ascribed to a preferential burning of the hydrogen, whereby carbon is separated in the flame and glows in the state of solid particles. This view, which appears to have originated in a misinterpretation of Davy's words, has never been based on substantial evidence, and it is at variance with the most cogent experiments on the subject. There seems little doubt that the bright glow of a hydrocarbon flame is essentially a thermal phenomenon.

The glowing substance was supposed by Davy to be solid particles of carbon, by Frankland to be the vapour of dense hydrocarbons. These two rival views have been subject to considerable discussion, especially by Heumann.*

It seems extremely difficult now to find any good evidence for the dense hydrocarbon theory. One of the simplest arguments against it was supplied by Stein, who pointed out that the glowing

* Phil. Mag. [i] 89, p. 366 (1877).

substance in a hydrocarbon flame, which may be collected in the form of soot, contains a smaller quantity of hydrogen than could reasonably be expected if soot were a hydrocarbon or a mixture of hydrocarbons. He also remarked upon the non-volatile character of soot. A recent analysis of soot from an acetylene flame showed 1·4 parts of hydrogen to 98·6 parts of carbon, after the soot had been extracted with ether and dried. Now the hydrocarbon richest in carbon recognised in organic chemistry (chrysogene) contains about 5 per cent. of hydrogen. The soot, therefore, could not contain more than about 30 per cent. of it, leaving a surplus of 70 per cent. of uncombined carbon. To maintain Frankland's doctrine that the light is essentially due to dense hydrocarbons in the gaseous state, would compel us, in fact, to recognise soot as a hydrocarbon of quite exceptional composition and properties. The doctrine was, in its inception, an inference from experiments on other flames in which high luminosity was found to be associated with high density of the substances contained in the flames; but it is to be remarked that in most, if not all of these flames, the glow was ascribed to the product of oxidation, and not merely to something separated and subjected to a purely roasting process.

But even if we regard the glowing substance soot of a flame as a hydrocarbon or a mixture of hydrocarbons, and to this extent accept Frankland's view, there remains the question whether the glowing substance in the flame is solid or gaseous. The optical test, first used by Soret, shows indisputably that a finely divided solid pervades the whole of the luminous region of a hydrocarbon flame, and there seems no reason to doubt that the glow of this solid matter would be adequate to produce the light of the flame.

According to the views of Lewes, the luminosity of a hydrocarbon flame is determined essentially by the formation and subsequent decomposition of acetylene. This theory, which is certainly ingenious, need not be discussed on the present occasion.

The development of bright light in a hydrocarbon flame, whatever be the full explanation, is certainly a secondary process, demanding a particular mode of burning the gas for its production. When the hydrocarbon meets the air in other ways, as when it is burnt in a very small flame or at a very high pressure, or when air is added to the gas before it leaves the burner, the bright light disappears, and we then have the primary light of combustion which is of feeble intensity and blue colour. The changes which a hydrocarbon flame undergoes with varying air supply are well seen when benzene vapour is burned with a gradually increasing quantity of admixed air. The flame is at first very bright; the next phase, reached when the bright yellowish light has just disappeared, shows two cones of bluish light, corresponding to those of a Bunsen burner; the last phase is reached when, by adding more air, the outer cone is quenched, and the flame presents the appearance of a thin conical shell of blue light. [Experiment shown.] The two-coned phase

marks the period when the oxygen required for combustion is got partly from the air mixed with the vapour before it leaves the burner and partly from the air outside, one cone corresponding to each part of the supply. From analyses of the interconal gases, it appears that large quantities of carbon monoxide and hydrogen are generated in the inner cone, and that these are the gases which burn in the outer cone. The evidence that the formation of carbon monoxide is the first step in the combustion of carbon has been greatly strengthened by the experiments of Prof. H. B. Dixon, and is at variance with no important facts.

The source of the light in a blue-burning hydrocarbon flame has been the subject of most elaborate investigation and of prolonged controversy. The spectrum of this light was one of the first to be carefully described, and is often called the Swan spectrum, from the fact that it was first accurately mapped by Swan in 1856. It is seen in the blue part at the base of all ordinary hydrocarbon flames and in the inner cone, but not in the outer cone of flames fed with air in the manner of the Bunsen burner. In so far as the characteristic product of these parts of flames has been found to be carbonic oxide, it would be natural to attribute the Swan spectrum to this gas. This view, however, has never been adopted. The Swan spectrum has been attributed either to carbon itself or to a hydrocarbon (acetylene), and the whole discussion and investigation of the subject has centred round these alternatives. The neglect to consider the likelihood of carbon monoxide being the source has arisen from a disregard of the occurrence of this gas in flames, and from a belief that it has another distinct spectrum. At the same time the difficulty presented by the other explanations has been fully realised, and it is admitted that the support of either demands somewhat strained hypotheses.

The question of the origin of the Swan spectrum is too large and complicated to be fully discussed here. It will suffice to point out that if the formation of carbon monoxide is the first act in the oxidation of a hydrocarbon two results would follow: (1) it would hardly be supposed that carbon vapour existed free even momentarily in the flame; (2) that the preponderating product with which was associated the energy of the chemical change should contribute mainly to the emission of light. The chief difficulty opposed to the view that carbon monoxide is really the source of the Swan spectrum appears to lie in the fact that this gas may be made to yield a different spectrum by the electric discharge. A full consideration of the evidence bearing on the subject leads to the view, first, that this spectrum is not undoubtedly due to carbon monoxide, and secondly, that it may be due to carbon dioxide.

The evidence derived from the study of flames, and much other evidence, is favourable to the view that carbon monoxide is the source of the Swan spectrum, and if this be the case, the chemi-luminescence of a hydrocarbon flame like that of a flame of the hydrides of phosphorus, silica and antimony, would be attributed to the act of oxidation.

Some light is no doubt due to the completion of the oxidation, the carbon monoxide forming carbon dioxide and the hydrogen forming water, but the intensity of this portion of the light is inconsiderable in the spectroscope, and in the visible spectrum not characteristic.

The flame of cyanogen presents special points of interest. It has been shown that the sharp differentiation of the flame into an inner rose-coloured cone and an outer blue one, corresponds to the combustion of the gas in two steps, the first being the oxidation of carbon to carbon monoxide, and the second the oxidation of carbon monoxide to carbon dioxide.* Admixture of air with the gas before combustion renders it possible to separate the two parts of the flame in the cone separating apparatus, and when the distance between them exceeds a certain limit and the gases are dried, the outer cone is quenched when a bottle of dried air is held over it. [Experiment shown.] This behaviour accords with the well known experiment of Prof. Dixon on the combustion of carbon monoxide. According to the view which has been developed in the foregoing, it would be expected that the light emitted by the inner cone of a cyanogen flame should be due to the carbon monoxide which is produced there, and if the Swan spectrum be really due to that substance then the Swan spectrum should be seen. As a matter of fact, the inner cone of a cyanogen flame gives a brilliant spectrum, in which, however, only one band of the Swan spectrum is distinctly developed. It is possible that the liberation of nitrogen from cyanogen during its combustion may have a disturbing influence. In any case it is very striking that when cyanogen is burnt in oxygen instead of air the Swan spectrum is seen to be completely and brilliantly developed, and on the whole the evidence derived from a cyanogen flame appears to strengthen the view which associates the Swan spectrum with the production of carbon monoxide.

Reviewing the evidence which has been offered, it appears that the primary source of light in flames is to be found in the intense vibratory motion which is determined by the act of chemical union. This is seen in the phosphorescence of phosphorus, in the flame of hydrogen, and at the base of the flames of the hydrides of silicon and carbon. A secondary source of light arises when the temperature effect of the primary combustion causes the glow of a product or partial product of combustion. This is seen in the white flame of phosphorus, in the brightest part of the flame of silicon hydride, and in the bright yellow-white part of ordinary hydrocarbon flames.

The question of the luminosity of flames containing the vapours of salts introduces new problems, the elucidation of which is far from being complete. This question, however, cannot be considered on the present occasion.

[A. S.]

* Smithells and Dent, Journ. Chem. Soc. 65, p. 603 (1894).

WEEKLY EVENING MEETING,

Friday, March 19, 1897.

Basil Woodd Smith, Esq. F.R.A.S. F.S.A. Vice-President, in the Chair.

Sir Edward Maunde Thompson, K.C.B. D.C.L. LL.D. F.S.A. (Principal Librarian of the British Museum).

Greek and Latin Palæography.

Our knowledge of Greek and Latin Palæography has expanded so largely during the last quarter of a century, that, in response to an invitation to read a paper before the Royal Institution, I have ventured to select it as the subject for the discourse this evening. For, although palæography is a science which is, in the nature of things, confined to the enquiries of comparatively few students, yet that branch of it which deals with writings in Greek and Latin may appeal to the interest of most of us, whose education has been founded on the study of the classical authors of Greece and Rome. And, further, the derivation of the alphabet now in use throughout the greater part of the world, immediately from the alphabet of the Latins and more remotely from that of Hellas, and the various changes through which it passed before attaining a simple and regular form, are matters for the curiosity, if not for the study, of all who claim to take an interest in the history of literature.

The extension of the knowledge of our subject during recent years is due in the highest degree to the invention of photography, and to the perfection to which the art of photographic reproduction has been brought. When we regard the rude and inexact facsimiles from manuscripts, which appear in the older works on palæography, we cannot conceive the possibility of the student learning anything of value from them. For all scientific purposes they are worthless, and they could only serve to convey a very general idea of the character in which the originals were written. Next came works executed with more skill, but so costly that they were beyond the reach of all but the wealthy; and, again, careful and exact as they are, they fail to reproduce those minute variations and delicate *nuances* of the manuscript, which it is impossible for a second hand to render faithfully. Photography came and made the path smooth. Under ordinary conditions it gives us a facsimile of the original, which, next to being the original itself, is the best that we can desire. The agency of the second hand, which involuntarily but

inevitably imported its own character into the old hand-made facsimile, is dispensed with; the agency of light can never alter the character of the first hand. The collections of photographic facsimiles issued during the last five-and-twenty years form a palæographical corpus which renders the study a comparatively easy one; and, further, we now have the immense advantage of being in a position to compare side by side, through the medium of those trustworthy facsimiles, texts which are in reality scattered through the libraries of Europe. Five-and-twenty years ago the palæographer working in the public library of his own country might have a good knowledge of the handwritings of the later middle ages; the material under his hands was sufficient; but of the earlier periods his experience was limited, and he could scarcely speak without hesitation on questions of the palæography of manuscripts, of which his library contained only a few examples. We are in a very different position to-day. The abundant supply of facsimiles has given us the means of training the eye and of familiarising it with the handwritings of all periods.

And while our material has thus been concentrated by photography, it has also actually increased in amount. Recent excavations in Egypt have placed us in possession of documents which, for the first time, have brought us almost in touch with the classical period of Greek literature. Greek writing of the third century before Christ was scarcely known to us before these modern discoveries; we now know that at that age writing was a common and widespread accomplishment under the Ptolemies in Egypt. Nor in this direction alone have we profited: the numerous papyri which have been and are being found of the early centuries of the Christian era supply the links, formerly wanting, to trace the descent of the uncial writing of the earliest extant Biblical codices of the fourth and fifth centuries from the earlier examples. The chain is now nearly complete, and the history of Greek handwriting can be followed with more or less precision through a period of some seventeen centuries before it became fixed by the printing press. The additions to our material for Latin palæography have not been so abundant, but they have been scarcely less interesting. Excavations on the site of Pompeii and in other places have given us an insight into the character of the handwriting of the Roman people in the early time of the empire: and, even if no great classical work has been recovered, we have in the wall scribblings that have been laid bare, and in the waxen tablets that have been found, invaluable examples of the writing of everyday life and of the business transactions of the people.

The connection between the Greek and Latin alphabets is obvious when we compare their early forms. The primitive Greek alphabet of two-and-twenty signs borrowed from the Phœnicians—written at first from right to left, and eventually from left to right, after passing through that curious period of *boustrophedon* writing, in which a line written from the right was succeeded by one written from the

left, and so on, just as the ploughing ox cuts the furrows in the field—this primitive alphabet, under local influences which cannot now be defined, developed into two main branches or groups, to which the designations of Eastern and Western have been applied. The Eastern or Ionian branch was that current in Asia Minor and the neighbouring islands, and in certain states of Greece; the Western branch was employed more extensively in Greece and in most of the states of the Peloponnese, and also in the Achæan and Chalcidian colonies of Italy and Sicily. The most special mark of distinction between the two branches is the symbol or letter representing the sound *x*. In the Eastern branch this sound is represented by Ξ, and the letters X and Ψ have the sounds of *kh* and *ps*, as we know them in ordinary usage in Greek literature, Athens having naturally followed the Ionian system. In the Western branch the letter Ξ is wanting, while X and Ψ have the values of *x* and *kh*; the sound *ps* being expressed in separate letters πς or φς, or rarely by a special sign ✳. No satisfactory explanation has yet been found for this remarkable distinction The Latins borrowed the Western Greek alphabet from the Chalcidian colonies, such as Cumæ, planted on the Campanian coast. The Greek double letters (or aspirates) *th*, *ph*, *kh*, representing no sounds in the Latin tongue, were dropped; the third letter, at first used to express the hard *g* sound, came to be also used for the *k* sound, and the letter K, though it remained in the alphabet, became almost a dead letter. Gradually the *k* sound ousted the *g* sound in the third letter, and for expression of the latter another symbol had to be invented. This was found by differentiating the C by a stroke or tail, thus creating the letter G. A place for this new letter had been meanwhile left vacant by the gradual extinction of the soft *s* or *z* sound in Latin, whereby the presence of Z was dispensed with. In Quintilian's time X was "ultima nostrarum" and closed the alphabet. Later, Y and Z were added, not for the purpose of expressing native sounds, but for the more exact transliteration of Greek.

To find illustrations of the use of the early forms of the Greek and Latin alphabets, we should have recourse to inscriptions on stone or metal, but this would take us beyond the limits of our present subject, which is confined to the history of the development of handwriting, as distinct from epigraphy. And yet, while we thus lay aside the more ancient examples of texts, either Greek or Latin, we must not assume that handwriting only began where the early inscriptions leave off. In consequence of the recent discoveries in Egypt, our former views in regard to the antiquity of the practice of writing in Greece have undergone considerable modification. There is always, and I imagine there always has been, a tendency to refuse to bygone generations that capacity for acquiring and diffusing knowledge which we flatter ourselves is an attribute of modern intelligence; and all unexplored periods of history are dark ages. But we now know that three hundred years before the Christian era the Greeks in Egypt, in different classes of society, the professional man and man

of business, just as well as the literary man, could write with as much ease and fluency as we can ourselves. Their handwriting is fully matured and bears on its face the evidence of a development which must have been the growth of a long period. The knowledge of writing in Greece, we fully believe, must be at least coeval with the oldest Greek inscriptions; and we are not to assume that, because those inscriptions are laboured and painfully executed, therefore the handwriting of the same time was equally laboured and painful. On the contrary, the handwriting may have been, and probably was, tolerably fluent; and it would be as unjust to measure the ancient Greek's capacity for expressing himself with the pen by the standard of his inscriptions, as it would be to take the rustic lettering of our provincial tombstones as a measure for deciding the proficiency of modern penmanship.

As I have already said, we have to depend, for our acquaintance with the earliest examples of Greek writing, upon the papyri which have been found in Egypt. These may be broadly classified in two divisions: the first, literary; the second, official and domestic. The literary documents, naturally, are generally written with more care than those of the other class. Texts intended for the market were inscribed in a formal style which would correspond to the printing of the present day. But others, even though of a literary character, if written for the scholar's own use, would not be necessarily transcribed in this formal fashion, but might appear in the ordinary current handwriting of the scholar himself or of his amanuensis. On the other hand, official and domestic documents are generally written in cursive handwritings, more or less exact or careless, according to the education or skill of the writer. In dating the domestic documents we have not the same difficulty—as a rule—as in dealing with literary works, for a large proportion bear actual dates, and thus form standards of comparison for those documents which have not been so dated. In dealing with literary works written in the cursive handwritings we have the same advantage of comparison with the dated cursive examples of the official and domestic division. But, when we come to the formally written works, our real difficulty begins.

The faculty of deciding the age of handwritings of a formal character of any period must chiefly grow from familiarity; and this familiarity, of course, can only be acquired by the survey of a large number of examples. Every palæographer knows how easy it is to assign dates to manuscripts of the middle ages, say from the twelfth to the fifteenth centuries, of which there are plentiful examples; his difficulties begin when he moves back into the earlier centuries when his material is more limited; and when he comes to examine, for example, such a formal handwriting as the uncials of the fourth, fifth and sixth centuries, he does not venture to be dogmatic. When we go back to a period still more remote, such as the third, second and first centuries B.C., our difficulties become extreme. It is not to

be wondered at, then, that the dates formerly assigned to some of the examples of classical papyri must be reconsidered by the light of recent discoveries.

If we take up a table of alphabets, drawn from the oldest examples of Greek writing extant, and glance along the lines of the different letters, we see how various their formation was under different conditions, even at that early period.* In the first two columns we have the formal letters used in the classical fragments; in the others we have the letters used in documents, all of a more or less cursive character. How very cursive some of them could become is evident, if we examine the examples of the letters Lambda, Mu, Nu, Pi, Tau and Omega. With regard to the last letter, the transition, which without those examples it would not be easy to explain, from the original horseshoe-shaped letter to the later ω form is readily followed. How easily there might have been a confusion between a Lambda and a Mu and a Pi! for each of those letters in some instances is formed simply by a curved stroke. The Tau with the horizontal only on the left is an example of a rapid method of constructing the letter, which has a modern parallel in the *t* of somewhat similar shape in use among the French. A second table will carry us on to the third century after Christ, missing, however, one century, the first century B.C.; for it is a remarkable circumstance that among the large number of papyri that have been recovered there are scarcely any that actually bear dates within that hundred years. However, comparing the forms of letters of the second century B.C. with those of the first century of our era, we conclude that it was a period of decadence in Greek handwriting, the letters of the later century being inferior to those of the earlier time.

Probably the very oldest example of Greek writing is the papyrus fragment, now in the Imperial Library at Vienna, inscribed with an invocation of a certain Artemisia against the father of her child. It is probably as early as the first half of the third century B.C. The handwriting is rough, every letter being written separately in the style of an inscription; and, judging by the fluent character of other extant specimens of nearly contemporary current handwriting, we are justified in assuming this papyrus to represent, not the educated style of the time, but rather the imperfect effort of one not much accustomed to use the pen.†

It is, however, even though an illiterate production, a document of much value in that it shows exactly individual forms of letters of the formal alphabet of the time. The contemporary literary hand is seen at its best in some fragments of the 'Phædo' of Plato, which had been employed, together with other papyrus documents, as the

* See the carefully drawn table in 'The Flinders Petrie Papyri,' ed. Prof. J. P. Mahaffy; in the 'Cunningham Memoirs' of the Royal Irish Academy, 1891.

† Facsimiles of the Palæographical Society, ii. 141.

material for cartonnage mummy-cases in the Greek colony of Gurob in the Fayum. The official deeds found among these fragments date from about the year 260 B.C.; this manuscript of Plato may therefore be placed rather earlier, for it is not probable that a literary work such as this would have been destroyed immediately after it had been written, although ordinary documents would cease to have any value after a few years. It is to be regretted that what remains of this once beautiful manuscript is in such a fragmentary condition; but there is still enough to show that a very perfect style of handwriting was employed in the production of classical works intended for the book market in the third century B.C. The chief characteristic of the writing is the great breadth—almost flatness—of many of the letters, as compared with their height.*

The same invaluable Gurob collection of papyri also provides us with material for ascertaining the capabilities of persons in different ranks of life to express themselves in writing—not in the formal literary hand of the 'Phædo,' but in the ordinary running hand of the day. A beautiful document of the middle of the century, written in a particularly clear and well-shaped character, is the letter of a young man, well educated, named Polykrates, who addresses his father with affectionate frankness, and invites him to come and stimulate the writer to shake off his present idleness; but assures him also that in money matters his son is quite solvent. Another letter, equally well written, is addressed in the year 242 B.C., by one Horos, an official, to a colleague named Armais, and seems to be prompted by professional jealousy at his correspondent making a good thing by the sale of oil at a price higher than that fixed by royal decree. The writing is an excellent example of that fine linked hand which appears to have come into vogue at this time and which is so particularly characteristic of the best written cursive documents of the next hundred years. A third letter of the same time shows how a man of the agricultural class could handle his pen. It is a communication from a farm bailiff to his master, telling him of the vineyard, the olive-yard, and the dearth of water. The writing is the rough hand of a practical man, not highly educated, but with knowledge enough to express himself in a business-like way. In this example there is none of the beautiful linking together of the letters which appeared in the practised hand of the official's epistle; here, every letter stands apart, and perhaps we may style the bailiff's handwriting as rather of the pothook order.

In the third century, then, before Christ we have evidence that the Greeks in Egypt practised the two styles of handwriting: the literary and the cursive. And the possession of a literary hand implied a long course of practice. Like all things, handwriting is subject to the regular laws of nature. It developes, reaches perfection, and then decays. And it is when in the stage of perfection, that a style of

* Mahaffy, 'Flinders Petrie Papyri.'

handwriting is adopted for a literary hand. Hence as a literary hand it seems to burst upon us in full life: Athenè springs ready armed from the head of Zeus. But it has been previously passing through a long period of preparation and development, the evidences of which are lost; and it is only because it succeeds in reaching perfection, that it is then employed as a literary hand. When once in that position, it may maintain its excellence for a time, but not for a long time. It gradually becomes a formal hand, and then an artificial hand, and, as such, is doomed to deterioration. Meanwhile, the natural cursive hand continues its course, and again developes a new style, which in turn reaches perfection and then supersedes the old literary hand, which has by this time lost all life and has become a mere imitative script. And thus the process goes on repeating itself. The best illustration of this law of change is to be seen in the general adoption, both for Greek and for Latin manuscripts, of the minuscule or small hand, as the literary hand, in place of the uncial or large hand, early in the ninth century. The creation of minuscule writing is naturally a long process. The large letters have to be ground down by a long course of cursive writing, and the small letters thus formed have to take shape and be cast in an artistic mould before they can aspire to be used in the production of literary manuscripts. But in the end, because they can be more fluently formed, and thus become the more natural means of the expression of thought, they cannot fail to supersede the older and more slowly written uncials.

The time at our disposal this evening will not allow me to take you down to the moment of this great change. I propose to limit my further remarks on Greek palæography to the early centuries, and only to touch the boundary of the mediæval period.

To illustrate the handwriting of the first half of the second century B.C., we may turn to two literary documents, the one written in a cursive hand, the other in a formal hand. The first is an astronomical treatise, now in Paris, which must be earlier than the year 164 B.C., as some documents of that date are written on the back of the papyrus. The hand is of a good bold character, the prominent feature being the linking together of the letters by connecting strokes which has been already referred to. This papyrus was no doubt a copy made for a scholar's own use, and not for sale. It is copied in the ordinary character which he would write naturally. The second papyrus, containing a dialectical treatise, of the same age, is inscribed in the formal literary hand by a professional writer, working for the book market. Comparing these two works with those of the preceding century we should pronounce a deterioration in the formal hand, being a style which naturally tends to become artificial; but we do not perceive any great change in the cursive hand, which is the natural hand, except that it may be rather more fluent than that of the previous century.*

* 'Notices et Extraits des MSS. de la Bibl. Impériale,' xviii. pt. 2.

We now turn the century and glance at one or two of the classical papyri representing the first century B.C. The literary hand assumes in some of these a more compact style. The manuscript of the 18th book of the Iliad, known as the Harris Homer, now in the British Museum, is an excellent specimen, but rather discoloured. Here the writing is again of the formal literary type, the letters delicately shaped and slightly inclining to the left. Somewhat of the same cast of hand and of the same period is the quite recently discovered papyrus of the odes of the poet Bacchylides. The writing is beautifully clear, and, had the roll not been unfortunately broken up and a portion of it reduced to a confusion of small fragments, the editing of the book would not have presented most of the difficulties which now have to be encountered.

Towards the end of the first century a more ornamental class of writing for literary purposes appears to have been coming into vogue. It was essentially a calligraphic style, and in the rounded shapes of the letters we see an indication of the form that Greek literary writing was to assume when the writing material changed from the frail papyrus, on which the strokes were necessarily of a light character, to the substantial vellum which would bear the impress of a firmer hand. A fragment of the Odyssey, now in the British Museum, which may be dated in the closing years of the century, is in this style. And again, the beautifully written papyrus which contains the oration of Hyperides for Lycophron and Euxenippus, and which may be placed in the first century of our era, is another example of this precise but rather artificial hand.

But, now and again, a scholar, perhaps too poor to buy costly papyri, perhaps living too far away in the country, or, it may be, preferring his own transcript to the handsomer but less correct text which he might purchase, wrote out some favourite book for his own use. The long-lost work of Aristotle on the Constitution of Athens, which was recovered only a few years ago, is an instance of this personal industry. Written on the back of some farm accounts of the year 78–79 A.D., the text is in the involved and cramped cursive hand found in documents of the end of the century. But such home-made books were no doubt comparatively rare by the side of those turned out by the professional literary scribe, whose writing was now approaching nearer to the perfect round uncial hand which we find in the earliest vellum manuscripts. The papyrus document which comes nearest to that round hand is the Bankes Homer of the second century.

How this hand was taught in the schools we learn from an interesting little diptych or pair of waxen tablets belonging to a schoolboy of about the second century.*

This copy-book, clumsily made of wood, with a sunken surface coated with wax in the usual way, contains two columns of the

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* Brit. Mus., Add. MS. 34,186.

The master has written the sigma of σοφου rather lightly, ...ly the wax was too thin, close to the edge, for the stilus to ... a good impression: and the pupil leaves it out altogether. But ...y turn the laugh against the pedagogue. The word πιστευεται ... have been πιστευετε. The master discovered his error, but he ...n his epsilon at the end of the wrong line.

...e descent of the beautiful uncial writing of the vellum manu... from this earlier hand requires no further demonstration. The ... great codices of the Bible—the "Codex Vaticanus" of the ... century, the "Codex Sinaiticus" of the fourth or fifth century, ...e "Codex Alexandrinus" of the fifth century—are great palæo...cal monuments as well as all-important texts.

...r our earliest specimens of Latin handwriting we have recourse ... excavations of Pompeii, and of Herculaneum, and of Rome. ...Pompeii we have a large collection of wall inscriptions which ...een carefully collected by the Germans and published by them ... volumes of the 'Corpus Inscriptionum Latinarum.' We have ...om the same source a very valuable set of waxen tablets which ...ound a few years ago, and which have been partly published by ...ciety of the Lincei of Rome. A complete edition has been long ...promised by Professor Zangemeister of Heidelberg.

...e wall inscriptions of Pompeii are of two kinds: first, those ... with a brush in large letters, generally in capital letters, ...ting chiefly of advertisements, recommendations of candidates, ...ncements of public games, losses, houses to let, &c.—in fact, ...ch advertisements as we may see placarded in print on our ...alls at the present day. Some few of these are of early date, ...ost of them lie between the years 63 and 79 of our era, the ... year being the date of the destruction of the city. The second ...of the wall inscriptions is composed of scrawls, a few in ...al or chalk, but most of them scratched with a sharp point, that ...fiti: they are in cursive letters, and consist of all kinds of idle

They were found in 1875 in the house of a pawnbroker or banker named Lucius Cæcilius Jucundus. Enclosed in a box placed in a recess above the portico, they fortunately escaped absolute destruction, although much blackened and damaged by the heat. They comprise two classes of documents, viz. deeds connected with auctions, and receipts for payments of taxes. They range in date mostly from A.D. 53 to A.D. 62, and they are generally triptychs, that is, tablets formed of three boards or *leaves* of wood. Of the same period are a few fragments of Latin-written papyri found among the Greek collection recovered at Herculaneum. They are, however, very scanty.

The next important material consists of twenty-four waxen tablets, which were recovered in the ancient mining works of Verespatak, in Dacia, the ancient Alburnus Major, and concern the private affairs of the miners. Twelve of them bear dates between the years 131 and 167 of our era. These tablets were probably left in the mines when the Roman colony was suddenly attacked by the barbarians; and it has been suggested that the destruction of the place was effected in the war with the Marcomanni, A.D. 166–180. They are published in the 'Corpus Inscriptionum Latinarum.'

Contemporary with these collections we may also count a few documents and stray tiles and such fragments found at various sites, which are scratched with alphabets or verses or haphazard memoranda.

The greater part of the materials which have just been enumerated consist of documents or fragments written in cursive handwriting, and afford us means of tracing pretty clearly the course which that form of Roman writing took in the early centuries, leading on to the current handwriting which we find in the papyri of Italy of the early middle ages, and forming eventually the type upon which the national handwritings of Italy, France, and Spain were developed.

Two tables of alphabets in the 'Corpus Inscriptionum' show the forms of letters used in the wall inscriptions and those used in the waxen tablets of Dacia. In the first division of the first plate, we have the oldest forms of letters painted with a brush: in the first row, square capitals, formed precisely; in the second and third rows, the more careless and quickly written alphabet, which, from its negligent style, has been called *Rustic*. In the third and fourth divisions are the cursive alphabets of the *graffiti*. Running the eye vertically down the several columns of the letters, we can follow their changes and see the history of the development of certain forms very plainly. In writing quickly, all parts of the letters which may be dispensed with without obscuring their forms naturally fall away; the cross stroke of A is soon found to be a trouble, it drops into a tag, and in many cases altogether disappears. The letter B, even in the early stage in the second division, begins to lose the upper bow. In the third division, the main stroke, instead of being drawn in its proper vertical line, runs off to the line of the bow, and then a bow is added on the left, giving the letter the appearance of a

small *d* or tall *a*; this development is seen pretty well completed in the fourth division. The letter E, besides the capital form, is also written in two vertical strokes, a form found in inscriptions and which appears in the old Faliscan alphabet. In the waxen tablets this form is very generally used, no doubt because it was so very easily written. In the letter F, again, the cross stroke gradually drops away, and the letter is formed eventually of merely two strokes, both of them vertical. The development of the tail of G can be traced in the column as we descend. In the fourth division, the four strokes of the letter M fall into a perpendicular arrangement. But this form of the letter does not occur in the Dacian tablets; it was probably found confusing in a class of writing which contained so many verticals. The letter N goes through the same course, falling into three vertical lines. The breaking up of the letter O is very interesting: when it is formed by the double action of two curves meeting, the second curve tends to become concave like the first, the letter thus assuming the form of a badly made cursive *a*. In the letter P we see the gradual loss of the bow—or rather its change from a curve to a mere oblique tag or stroke. Important changes pass over the letter R; first comes the opening of the bow, then the gradual change in the direction of the stroke, which becomes a mere waved line.

The second table of alphabets represents the forms of letters found in the Dacian waxen tablets of the second century. Here is a still further development of the letters of the *graffiti*, and in writing on such a material as wax there would be even more temptation to get rid of superfluities in the letters, than when writing on a plaster-covered wall. Further, the tendency of the action of the hand would be to write letters sloping rather to the left, the curves would all tend to become concave, the stilus being held with its point inwards. The principal difficulty in reading the writing on the waxen tablets is caused by the linking of the letters, many of the combinations forming almost monograms; these are all collected in the lower division of the plate. Accurate facsimiles of the wall inscriptions are collected in the 'Corpus Inscriptionum' and may there be studied in all their details.

From the tables of alphabets it is seen how the cursive hand of everyday life developes from the capital letters; and those capital letters are of course nothing more than the later development of the archaic alphabet. To find the Roman literary hand, we must start again from the capitals, but move in a different direction from that followed by the cursive writing. For public inscriptions a refined and artistic form of letters was naturally soon required; and the creation of very perfect alphabets of capital letters, both square and rustic, resulted. To apply this large style to literary purposes may appear to us a costly and cumbersome method; and it is certainly remarkable that the practice of producing manuscripts in large letters, or majuscules, should have endured so many centuries as it did. On

the other hand, we must remember that the many examples that have survived probably owe their long life to the fact that they have been always regarded as of special value, and have thus been carefully kept, while ordinary copies, transcribed in the common handwriting of the day, and probably far more numerous than the majuscule codices, have been allowed to perish. However, extant examples prove to us that capital writing was employed in the production of important manuscripts, both in the square letter and in the rustic letter. But, as the latter form could be more expeditiously written, it was more frequently used than the square type. Again, the inconvenience of the square type almost immediately caused another modification; the scribe took to rounding off the angles of the letters, and a script which has received the name of *Uncial* writing was developed. From the fourth century, then, we have surviving examples of manuscript volumes in these large letters. But the system could not last; the square letter seems to have soon fallen into desuetude; then the rustic hand gradually dies out, leaving the uncial in possession of the field, only, however, to fall eventually into a decrepit and imitative state, and to disappear before the beautiful literary small hand which, by the beginning of the ninth century, had at length, after many vicissitudes, fully developed from the current forms of handwriting.

One or two fragments exist to show us the early practice of writing in capital letters. A fragmentary papyrus was recovered from the ashes of Herculaneum, inscribed with a poem on the battle of Actium in a light style of rustic letters, which was probably in fairly general use for literary purposes in the first half of the first century. The words are separated from one another by a full point, as in inscriptions; and long vowels are in many instances marked with an accent—long I being indicated by doubling the letter in height.

Another fragment of interest is a scrap of a sheet of papyrus, which contained a writing exercise of some young scholar in Egypt, perhaps of the first or second century; now in the British Museum. A line from the second book of the Æneid was the text chosen for this copy:—

"Non tibi Tyndaridis facies invisa Lacænæ."

The fragment shows a few imperfect repetitions of this line copied in rustic capitals, with some slight variations from the normal shapes. The letter D is exaggerated; and (a matter of more interest) the *d*-shaped B, the development of which in the cursive alphabet has already been noticed, is employed instead of the usual capital.

But, as already said, we have to descend to the fourth century to find examples of complete volumes in this large character. The "Codex Palatinus" of Virgil, now in the Vatican Library, is the best written manuscript of that time, and in the beautiful regularity of its rustic writing resembles the sculptured inscriptions of an

examples is in its full strength. To see what it became in its
decadence, we may glance at the manuscript of Prudentius at
written about the year 500, in which the character, though still
is artificial; and an instance of pure imitation, as late as about
ear 800, is afforded by the manuscript known as the Utrecht
r.*

he evidence of the employment of the square capital for sump-
manuscripts is more scanty. No volume in this style has
ed; but a few leaves from different manuscripts are still in
nce. At St. Gall, in Switzerland, there are the remains of
must have been a manuscript of immense size, for each page
ned only nineteen lines. Again, the author chosen for this
tion is Virgil, and the manuscript may have been written early
fifth century.

e third class of majuscule writing is the uncial; and the
t example of it is probably to be found in the palimpsest
ents of Cicero 'de Republica,' of the fourth century, in the
n Library. Here again the manuscript when perfect must
een of unusual size. The upper writing is the commentary of
gustine on the Psalms, written late in the seventh century. The
ntary copy of the Gospels at Vercelli in North Italy, of the
the fourth century, shows the uncial hand in a perfect and
us form; and the manuscript of Livy in the Imperial Library of
a is one of the best examples of the characters in the fifth
y. For the three following centuries, the uncial was destined
the chief literary hand of Western Europe; but we must take
f it at this point to trace in outline the development of the
or minuscule hand which was to supersede it.

o return to the early Roman cursive hand, and take up the
with the Dacian waxen tablets of the second century, selecting
them of the year 139.†

is tablet originally consisted of three leaves, and, counting six
to the tablet, we open it to show pages 2 and 3 of the triptych.

string or wire was passed through them and was secured on the back of the second leaf, that is, on page 4, by the seals of the witnesses; and on the same page the deed is repeated, in accordance with the legal practice of the Romans. Had waxen tablets been the principal writing material of the Roman world and continued to be so through the middle ages, we should at this day be writing a script quite different from the one which we actually employ. The character of the writing material has necessarily had at all times an important influence on the character of the handwriting; a most notable example being the development of the cuneiform writing in Babylonia and Assyria, where clay was the writing material in general use. On such a surface as moist clay the letters could be more easily formed by punctures than by strokes; and so it would have been with a prevalent use of waxed surfaces. We have seen the disjointed character that the Roman writing assumed in the tablets; confined to the same material it would have broken up still more, links and curves would gradually have disappeared, and in the end the alphabet would have consisted of a series of straight strokes and angles. But waxen tablets did not constitute the only, or even the principal, writing material of the Romans; and a connected current hand, gradually changing from capital forms to minuscule forms, was developing on papyrus and vellum, alongside the disjointed cursive letters of the waxen tablets. Unfortunately scarcely any specimens of this current hand of early date have been found—nothing more, in fact, than a few subscriptions of witnesses; we can only hope that some fortunate discovery in Egypt may put us in possession of documents to supply the links missing in the chain. Coming down, however, to the fifth and sixth centuries we find ourselves again upon firm ground with the papyrus documents of Ravenna and Naples and other places in Italy, in which we see the cursive Roman hand developed into a bold, rather straggling character. As an example we may select a Ravenna deed of the year 572, which is a good typical specimen, and, to analyse it the better, we may add a table of the forms of the letters, which frequently changed their shape when in combination with others.*

To follow the history of this hand, I should have to trace its course in the early middle ages through the national handwritings of Italy and of the Frankish empire and of Spain, of which it was the parent. Each of those national hands, the Lombardic, the Merovingian, and the Visigothic, as they have been termed, succeeded also in developing a literary form of writing of its own, not inelegant, but still, even at its best, rather intricate. In their cursive forms they became more and more involved and illegible; and, to the lasting advantage of Western European handwriting, they were swept away by the new hand which grew up in the reign of Charlemagne. It is, however, not without interest to know that the genius of the Roman cursive

* Pal. Soc., i. 2; and table of Latin cursive alphabets in my 'Handbook of Greek and Latin Palæography.'

of deterioration is always at work, that a literary hand becomes an artificial hand, and that the natural hand is the ive hand of ordinary life, we shall be prepared to find, what ly took place, that cursive forms soon began to intrude among majuscule forms in those manuscripts which were not of the first r; in other words, the scribes would allow the minuscule cursive s which they wrote as their ordinary hand to slip in among the artificial literary letters. In fact, absolute purity of the script ld only be maintained in very carefully written books. Hence a class of writing which has been called *Half-uncial*, because it mposed of a mixture of uncial and small letters. No doubt it some little time for this kind of writing to be reduced to a m; and we can see it in an incipient stage of development in technical works as law books where this incipient style may become traditional. In marginal notes too, the writing space g limited, this mixed hand was often preferred to the ordinary ive writing, just as we write a half-printing style of letters he narrow margins of our books. But those stages must have also passed through in much earlier times than the periods of xtant examples; for the half-uncial hand had become a recognised of literary handwriting, at least by the beginning of the sixth ury. A manuscript of St. Hilary, now in the archives of St. r's at Rome, is written in this character and bears a date of ion in the year 509–510.*

Judging from extant examples, the literary half-uncial hand appears ave been specially in favour in Southern France and Italy; and tually it has had the largest career of any form of Western writing. can here only mention the fact that it was the hand on which the scribes of the seventh century modelled their national writing, h became the parent of our own Anglo-Saxon character. When, r the fostering care of Charlemagne, the school of writing in the

national hands began in most countries to pass into slovenly age in the fifteenth century, we owe it to the sense of beauty in the Italians that a better model than that period could afford was found for the choicest types for the newly invented art of printing. The Carlovingian writing had passed into a beautiful form under the hands of the Italian scribes of the eleventh and twelfth centuries; and when, in the Renaissance, fastidious taste rejected contemporary writing as not being excellent enough for the highest standard, it was to that earlier form that men again turned as the only pattern fit for the reproduction of manuscripts of the classics, and then for the printing of books, in the type, so perfect in its simplicity, which we call Roman.

[E. M. T.]

WEEKLY EVENING MEETING,

Friday, March 26th, 1897.

...ames Crichton-Browne, M.D. LL.D. F.R.S. Treasurer and Vice-President, in the Chair.

Sir William Turner, D.C.L. LL.D. F.R.S.

Early Man in Scotland.

...nd, as in other countries, man existed before the time of ...story. The conditions under which his remains are found, ...orks which he has left behind him, provide the data for ...ng their age, not absolutely or capable of being expressed ...s of years, but relatively to each other.

...d differences existed in the physical conditions of Scotland, ...l in the northern parts of England also, as compared with ...rn districts of England and the adjoining parts of France ...am at the first appearance of primeval man in those countries. ...ore necessary, therefore, that the conditions then prevailing ...d should not be overlooked.

...idence sufficient to satisfy geologists has been advanced to ... man existed in Britain during the period called Tertiary. ...leed, as Scotland is concerned, even if it were admitted that ...arts of the globe man had been on the earth during Tertiary ...re is little likelihood that his remains could have been pre-...ar in that country the Tertiary is represented chiefly by ...ocks, and a few patches of sand and gravel with rolled sea ...nging to the closing stages of that period.

...the careful study which geologists have given to the surface ...nd, it is evident that at the commencement of the period ...uaternary or Pleistocene, immediately succeeding the Ter-...whole of the country was covered with ice which formed a ...ut 3000 or 4000 feet thick in the low grounds, of which ... boulder clay, or till, as it is termed, was the ground-

... upper boulder clay also occurs, which is often separated ... lower boulder clay by stratified deposits, some of which ... marine and other fresh water and terrestrial organic remains, ...us that the Ice Age was not one uninterrupted period of con-...old.* The lower and upper tills are the ground-moraines of

...he evidence on which these statements are based, consult the 'Great ...by Professor James Geikie, edition 1894, also his 'Classification of Glacial Deposits,' in Journal of Geology, vol. iii. April–May, 1895.

independent ice sheets, each indicating a distinct epoch, separated by an interglacial period. The earlier epoch was that of maximum glaciation, and the ice sheet extended over the north and middle of England, as far south as the Thames Valley and the foot of the Cotswold Hills, but the high moors in Derbyshire and Yorkshire and the tops of the highest mountains in Wales and Scotland rose above its surface. The great Mer de Glace stretched westward over Ireland into the Atlantic, whilst on the east it was continuous across the North Sea, with a similar ice sheet which covered Scandinavia and the region of the Baltic, and extended south to the foot of the hills of central Europe, and overspread much of the great central plain. In the extreme south of England, therefore, the conditions differed from those that obtained in the country further north. Although not actually covered with a sheet of ice, yet the more southern counties had been of necessity under the influence of cold, and must have been subjected to the effects produced by rain and snow, by freezing and thawing.

During the succeeding interglacial epoch the climate eventually became temperate and genial, and vegetable and animal life abounded. It is to this stage that most of the Pleistocene river alluvia and cave deposits of England and the adjacent parts of the Continent are assigned. The British Islands appear at that time to have been joined to the Continent, and the same mammalian fauna then occupied Britain, France and Belgium, which implied similar climatic conditions. As examples of these, it may be sufficient to name the larger mammals, as the cave and grizzly bear, the hyæna, lion, Irish deer, reindeer, hippopotamus, woolly rhinoceros, straight-tusked elephant and mammoth, all of which are now either locally or wholly extinct.

Abundant evidence exists that man was contemporaneous with these mammals in western Europe, as is shown by the presence of his bones alongside of theirs, and of numerous works of his hands, more especially the implements and tools which he had manufactured and employed. To a large extent these consisted of flint, rudely chipped and fashioned. To these implements, and to the men who made them, the well-known term "Palæolithic" is applied. But along with these, other implements have been discovered, made from the bones, horns and teeth of the larger mammals, on some of which animal forms and incidents of the chase have been sculptured both with taste and skill. Up to now, however, no trace of pottery which can without question be referred to Palæolithic men has been found, and no habitations, except the caves and rock shelters which nature provided for them.

One may now consider how far northwards in Britain Palæolithic man and the large mammals, with which he was contemporaneous, have been traced. The exploration of caverns made by Professor Boyd Dawkins, and other geologists associated with him, has proved that bones of certain of the mammals of this epoch were present in caves in Derbyshire, Yorkshire and North Wales, and that human

remains and implements of Palæolithic type have been found along with them in the Robin Hood cave in the Cresswell Crags, and in caverns in North and South Wales.

When Scotland is considered, evidence of the existence of the mammals of this epoch is not so abundant, yet the interglacial beds of that country have yielded remains of mammoth, reindeer, Irish elk, urus and horse. But notwithstanding the keen scrutiny to which the superficial deposits in Scotland have been subjected by the members of the Geological Survey and others, no traces either of the bones of Palæolithic man or of the work of his hands have been discovered in North Britain. This, indeed, is not much a matter of surprise, for it must be remembered that, subsequent to the genial interglacial epoch, another ice sheet, that of the upper boulder clay, made its appearance, grinding over the surface of the land, wearing away alluvia, and largely obliterating the relics of interglacial times. Hence interglacial beds occur only at intervals and are very fragmentary. Nor in Scotland are there any caves similar in dimensions to those which in England and elsewhere have yielded such abundant traces of Palæolithic man and his mammalian congeners. If Palæolithic man ever did exist in Scotland, and there is no reason why he might not have migrated northward from Yorkshire and Wales, yet one could hardly expect to discover traces of his former presence. In Scotland there are no massive limestones, with extensive caverns, in which man could have sheltered, and in which his relics and remains could have been secure from destruction during the advance of the second ice sheet. It is only in the alluvial deposits of interglacial times that such traces have been preserved, but these deposits, as we have seen, were ploughed out and to a great extent demolished by the later sheet of ice. The shreds that remain, however, are of extreme interest, from the fact that they contain relics of the Pleistocene mammals, with which Palæolithic man was contemporaneous; and there is a bare chance that some day traces of man himself may be encountered in the same deposits.

Geologists have shown that in the regions which were overflowed by the second or minor ice sheet no traces of Palæolithic man, or of the southern mammals with which he was associated, have ever been met with in British superficial alluvia. When found in those regions out of Scotland, they occurred in caves chiefly, and sometimes in the stratified deposits which here and there underlie the upper boulder-clay and its accompanying gravels.

So far as Scotland is concerned, one must look for a period subsequent to the melting of the second great ice sheet for evidence of the existence of early man. After its disappearance important fluctuations in temperature and in the relative level of land and sea took place from time to time, so that the climate and the area of land in Scotland differed in some measure from what is known at the present day. Eventually a period of cold again occurred, not so severe, undoubtedly, as in the two preceding glacial epochs, but sufficient to bring into

existence considerable district ice sheets and extensive valley-glaciers in the Highlands and Southern Uplands. Scotland at this stage was partially submerged, and many of the Highland glaciers reached the sea and gave origin to icebergs. The submergence slightly exceeded 100 feet, and the marine deposits formed at the time are charged with arctic shells and many erratic blocks and débris of rocks. On a subsequent elevation of the land, the beach formed at this level constituted a terrace, well marked on the coast line in many districts, and now known as the 100-foot beach.

There is good reason to believe that the elevation referred to was of sufficient extent to join Britain again to the Continent. It is to this stage that the great timber trees which underlie the old peat bogs of Scotland are referred. The peat with its underlying forest bed passes out to sea, and is overlaid in the carse lands of the Tay and the Forth by marine deposits, which form another well-marked terrace, the 45 to 50 foot raised beach of geologists.

Thus the elevation of the land that followed after the formation of the 100-foot beach coincided with an amelioration of climate and with the presence of an abundant vegetation, and large mammals, such as the red-deer, the elk, and the *Bos primigenius* roamed through the woods. While these conditions obtained partial submergence again ensued, and the sea rose to 50 feet, or thereabouts, above its present level. Within recent years it has been shown that during this period of partial submergence glaciers reached the sea in certain Highland firths, which would seem to show that the climate was hardly so genial as during the preceding continental condition of the British area, when that region was clothed with great forests. Ere long, however, elevation once more supervened, and the sea retreated to a lower level. Here it paused for some time, and so another well-marked terrace was formed, that which is known as the 25 to 30 foot beach.

There is not any evidence of the presence of man in Scotland during the formation of the 100-foot beach or terrace, but one can speak with certainty of his presence there during the period of formation of the later beaches. If one could put oneself into the position of an observer, who at the time of the 40–50 foot submergence had stood on the rock on which Stirling Castle is now built, instead of the present carse lands growing abundant grass and grain, and studded with towns, villages, and farm-houses, one would have seen a great arm of the sea extending almost if not quite across the country from east to west, and separating the land south of the Forth from that to the north. In this sea great whales and other marine animals disported themselves, and sought for their food. Abundant evidence, that this was the condition at that time in the Carse of Stirling, is furnished by the discovery during the present century of no fewer than twelve skeletons of whalebone whales belonging to the genus Balænoptera or Finner whales, imbedded in the deposit of mud, blue

silt and clay which formed the bed of the estuary.* This carse clay, as it is called, is now in places from 45 to 50 feet above the present high-water mark, and is extensively used for the manufacture of bricks and tiles. At a still lower level lies the carse clay of the 25–30 foot terrace. Until the beginning of the present century the clay had been covered by an extensive peat moss, which the proprietors of the land have removed. The question which has now to be considered is—Did man exist in Scotland at the period of the formation of the carse clays and of the two lower sea beaches? There is undoubted evidence that he did.

Along the margin of the 45–50 foot terrace in the neighbourhood of Falkirk one comes upon the shell-mounds and kitchen-middens of Neolithic man. All these occur on or at the base of the bluffs which overlook the carse lands—or, in other words, upon the old sea-coast. Again, in the Carse of Gowrie, a dug-out canoe was seen at the very base of the deposits, and immediately above the buried forest-bed of the Tay Valley. The 25–30 foot beach has been excavated out of the 40-50 foot terrace; it is largely a plain of erosion rather than of accumulation. It is probable, therefore, that many of the relics of man and his congeners which have been obtained at certain depths in the 25–30 foot beach may really belong to the period of the 40–50 foot beach. Some of these finds will now be referred to.

In 1819 the bones of a great whale, estimated at about 72 feet long, were exposed in the carse land adjoining the gate leading into the grounds of Airthrey Castle, near Bridge of Allan, about 25 feet above the level of high water of spring tides. Two pieces of stag's horn, through one of which a hole about an inch in diameter had been bored, were found close to the skeleton. In 1824, on the estate of Blair Drummond, in the district of Menteith, a whale's skeleton was exposed, and along with it a fragment of a stag's horn which was said to have a hole in it and to have been like that found along with the Airthrey whale. Mr. Home Drummond also states that a small piece of wood was present in the hole, which fitted it, but on drying, shrunk considerably. Unfortunately, these specimens have been lost, and no drawings or more detailed descriptions were ever apparently published, though in some geological and archeological works they have been stated, without any authority, to have been lances or harpoons. Twenty years ago the skeleton of another whale was exposed at Meiklewood, Gargunnock, a few miles to the west of Stirling, and resting upon the front of its skull was a portion of the beam of the antler of a red deer, fashioned into an implement eleven inches long, and six and a half inches in greatest girth; a hole had been bored through the beam, in which was a piece of wood one inch and three-quarters long, apparently the remains of a handle. The implement

* See more particularly Mr. Milne Home's 'Ancient Water Lines,' Edinburgh, 1882, and 'The Raised Beaches of the Forth Valley' by D. B. Morris, Stirling, 1892.

was truncated at one end, and shaped so that it could have been as a hammer, whilst the opposite end was smooth and bevelled chisel or axe-shaped edge formed by the hard external part of antler.* There can be no doubt that this implement resembled found alongside of the Airthrey and Blair Drummond whales ea in the century, and it effectually disposes of the statement that were lances or harpoons. Dug-out canoes have indeed been f imbedded in the Carse clays at a similar level, so that the peopl that day had discovered a means of chasing the whale in the wa one can, however, scarcely conceive it possible to manufacture a implement sufficient to penetrate the tough skin and blubber of of these huge animals, and to hold it in its efforts to escape. much more probable that the whale had been stranded at the eb the tide in the shallower water near the shore, and that the pe had descended from the neighbouring heights, and had used horn implements, with their chisel-like edges, to flense the carca its load of flesh and blubber, and had carried the spoil to respective habitations. There can be little doubt that these im ments rank, along with the dug-out canoes, as the oldest relics with human hands which have up to this time been found in Scotl and that they belong to the earliest period of occupation by Neol man.

After the oscillations in the relative level of land and sea ceased, and the beach found at the present day had been for evidence of the presence of Neolithic man and of mammals, both and domesticated, such as now exist in Scotland, becomes gr multiplied.

Shallow caves or rock shelters situated in the cliff which bo the esplanade at Oban Bay, which, after being closed for cent by a landslide from the adjacent height, had recently been qua into in obtaining stone for building purposes, were described in lecture.† The caves were as a rule 100 yards inland, and 30 feet or more above the present high-water mark. They ha doubt, been formed by the action of the waves at the period of fo tion of the 25–30 foot beach, for the floor of one of the caves covered by a layer of gravel and pebbles, which had been wa there when the sea had had access to it.

In these caves, bones representing fifteen human skeletons, women, and children, were found; also bones of the *Bos longif* red and roe deer, pig, dog, goat, badger, and otter, shells of ed molluscs, bones of fish and claws of crabs; flint scrapers, ham stones, implements of bone and horn fashioned into the form of p borers and chisel-shaped instruments. In one cave several harp

* I described this implement in Reports of British Association, 1889, p. It has subsequently been figured in a Report by Dr. Munro in the 'Proceed of the Society of Antiquaries,' 1896.

† For a detailed description, see papers by Dr. Joseph Anderson and Author in Proc. Scot. Soc. Antiquaries, 1895.

or fish spears made of the horns of deer were obtained; similar in form to those found in the Victoria Cave, Settle, in Kent's Cavern, and in the grotto of La Madelaine, France, which in some of these instances have been associated with Palæolithic objects.

An account was then given of the construction and contents of the chambered horned cairns in Caithness and the north-west of Scotland, which have been so carefully investigated and described by Dr. Joseph Anderson.* The presence of incinerated bones and of unburnt skeletons showed the cairns to have been places of interment, whilst flint flakes and scrapers, bone and polished stone implements, and shallow vessels of coarse clay, associated them with Neolithic man, obviously the same race as the builders of the English long barrows.

Stone abounds in Scotland, and the polished stone implements, which have been found in every county, in the soil and near the surface of the ground, are often of large size and beautifully ground and polished. Flint, on the other hand, is confined to a few localities, as the island of Mull and limited areas in the counties of Banff and Aberdeen. The nodules are as a rule small in size, and though adapted for the manufacture of arrow-heads and scrapers, flint does not seem to have attained the same importance in Scotland as the raw material provided by nature for the manufacture of articles used by Neolithic man, as was the case in England and Ireland.

Although there is ample evidence of the nature of the implements and weapons manufactured by Neolithic man, and of his methods of interment in rock shelters and chambered cairns, no traces of built dwellings which can be ascribed to the people of this period have been discovered. Doubtless their habitations were constructed of loose stones and turf, and sun-dried clay, or of the skins of animals killed in the chase spread over the branches of trees, which, from their fragile and destructible character, have not been preserved.

In the course of time stone and bone, readily procurable, and which are directly provided by nature for the use of man, gave place to materials which required for their manufacture considerable skill and knowledge. The introduction of bronze as a substance out of which useful articles could be made, marked an important step in human development, and could only take place after men had learnt by observation the ores of copper and tin, and by experiment the methods of extracting the metals from them, and the proportions in which they should be combined in the alloy in order to secure the necessary hardness. So far as Scotland is concerned, bronze must have been introduced from without; its manufacture could not have been of indigenous development, as the ores of tin and copper do not occur in North Britain. Doubtless it came from the southern part of our island, and was extensively employed in South Britain long before it became substituted in the north for the more primitive materials.

There is abundant information that Scotland had a Bronze Age.

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* 'Scotland in Pagan Times,' Edinburgh, 1886.

Swords, spears, bucklers, bracelets, rings, fish hooks, axes, chisels, sickles and other implements made of this metal have been found in considerable numbers. These objects occur sometimes singly, at others in collections or hoards in peat mosses, or even at the bottom of lochs and rivers, or buried in the soil as if they had been placed there with a view to concealment, and then, through the death or removal of their owners, had been lost sight of. In many instances these weapons and implements are elegant in design, show great mechanical ability in their construction, and are ornamented with much taste and skill. Instances also are not uncommon in which objects of bronze are found in the sepulchres of the period.

In the study of the Bronze Age in Scotland a want is experienced similar to that felt in a review of the Neolithic period. There are no buildings which can be distinctly regarded as dwelling-places for the men of this time. With them, however, as in the polished Stone Age, there is evidence of the mode in which they disposed of their dead friends and relatives. Interments which there are good grounds for associating with these people, have been exposed in the formation of roads and railways, and in agricultural operations. Where the surface of the ground has not been cultivated or otherwise disturbed, in almost every county tumuli, mounds, hillocks and cairns occur, the exploration of which has in many cases yielded interesting results. In no instance, however, have chambered cairns, divided into compartments, and possessing an entrance passage, been found associated with articles made of bronze. The sepulchral arrangements of the period possessed a greater simplicity than is shown in the chambered cairn.

The interments in the Bronze Age were sometimes that of a single individual in a knoll or mound, or under a cairn artificially constructed, and now overgrown with grass, heather and whin bushes, or, as is not uncommon, in the collection of sand or gravel near the sea shore, or on a river bank, or in the moraine of some long-vanished glacier. At other times, in similar localities, two to six interments had been made as if in a family burying ground. At others the interments were much more numerous, and represented doubtless the cemetery of a tribe or clan; one of the best known of these was observed some years ago at Law Park, near St. Andrews, in which about twenty interments were recognised. In another at Alloa, twenty-two separate interments were exposed. Quite recently, immediately to the east of Edinburgh, in the districts now known as Inveresk and Musselburgh, not less than fifty interments of this period have been brought to light, in connection with building operations, which implies that then, as now, this part of the country was settled and had a considerable population.

Two very distinct types of interment prevailed, viz. Cremation, with, or without cinerary urns; and Inhumation, the unburnt body being enclosed in a stone cist or coffin. From an analysis of 144 localities in Scotland of burials which may be associated with the

Bronze Age,* and which included about 400 distinct interments, it would appear that in fifty-one of these localities the bodies had all been cremated; in sixty they had been buried in stone cists; in fifteen the same mound or cemetery furnished examples of both kinds of sepulchre, and in the rest the kind of interment was not precisely recorded. These diversities did not express tribal differences, but seemed to have prevailed generally throughout Scotland. Both cremation and inhumation are found in counties so remote from each other as Sutherland in the north and Wigtown in the south, in Fife and the Lothians on the east, and in Argyll and the distant Hebrides in the west, as well as in the intermediate districts.

The cremation had been effected by wood fires, for in many localities charcoal has been found in considerable quantity at the place of interment. The heat generated was sufficient to reduce the body to ashes, and to burn the organic matter out of the bones, which fell into greyish-white fragments, often curiously cracked and contorted, which were not very friable. They were then collected and usually placed in an urn of a form and size which we now call cinerary. When a bank of sand or gravel was convenient, a hole three or four feet deep was made and the urn lodged in it. Sometimes the urn stood erect and a flat stone was placed across the mouth before the hole was filled in with sand and earth; at others a bed of compacted earth, or of small stones, or of a flat stone, was made at the bottom of the hole, and the urn, with its contents, was inverted. In some cases the urn was protected by loose stones arranged around it. In obviously exceptional instances, it may be perhaps of a tribal chieftain, a small stone cist was built to enclose the urn, and even a cairn of stones was piled above and around to protect it and to mark the spot.

Cremated interments not contained in urns have been recorded in a few instances, and in them the surrounding sand or gravel has usually been discoloured from the blackened remains and charcoal having to some extent become diffused through it.

The largest examples of cinerary urns were from 12 to 16 inches in height, with a flat narrow bottom, and 10 to 12 inches wide at the mouth. About one-third the distance below the mouth the urn swelled out to its widest diameter, and was surrounded by one or two mouldings, between which and the mouth the outer surface was often decorated with lines which ran horizontally, or vertically, or obliquely; sometimes they intersected and formed a chevron or a diamond-shaped pattern. Below the mouldings, the surface was without pattern, though sometimes raised into an additional simple circular moulding.

When the inhumation of an unburnt body was decided on, a rude

* Most of these are recorded in the 'Archæologica Scotica,' the 'Proceedings of the Scottish Society of Antiquaries,' and Dr Joseph Anderson's 'Scotland in Pagan Times'; whilst others, in the author's note books, have not yet been published.

cist or coffin, formed of undressed flattened stones, was built for its reception. As a rule the sides and ends of the cist were formed each of a single slab of sandstone, schist, gneiss, granite or other stones provided by the rock in the neighbourhood; but in some instances of a stone of a different character from the adjoining rocks, and obviously brought from a distance. The stones were set on edge and supported a great slab, which being laid horizontally formed the lid or cover of the cist, and which was much thicker and heavier than the side and end stones; sometimes, as if for additional protection, a second massive slab was placed on the top of the proper cover. The floor of the cist was formed, when the earth was shallow, of the native rock, and at other times of compacted earth, or a layer of pebbles, or of flat stones. Usually the stone walls and the cover of the cist were simply in apposition, but sometimes they were cemented together with clay. In some cists exposed a few years ago on the farm of Cousland, near Dalkeith, the peculiarity was observed of the cist being divided in its long direction into two by a stone slab down the middle.

The cists were oblong, the length exceeding the breadth, and although they varied in size, those for adults being larger than for children, they were always shorter than would have been required for a body to be extended at full length. As the end stones were usually set within the extremities of the side stones, the internal measurement of length was some inches less than the external. The average dimensions may be given for the interior about 4 feet in length, 2 feet in breadth and 2 feet in depth. The cover slab was much larger both in length and breadth, as it overlapped both the sides and ends.

These cists remind one in their general form and plan, but on a much smaller scale, both as regards the size of the enclosed space and the magnitude of the stones, of the dolmens so frequent in Brittany. As survivals in modern times we may point to the empty stone boxes, on the cover stone of which an inscription is incised, to be seen in so many country churchyards, built on the ground superficial to the pit in which the body in its wooden coffin has been inhumed.

Owing to the shortness of the cist the body could not be extended at full length, but was laid upon its side, with the elbows bent, so that the hands were close to the face; the hips and knee joints were also bent so that the knees were in front of the body.

Usually only a single skeleton has been found in a cist, either a man or a woman as the case may be. Sometimes two skeletons have been seen, at times a man's and a woman's, doubtless husband and wife; in others the second skeleton has been that of a child. Sometimes the cist was below the average in size, and contained only the skeleton of a child or young person. Such examples throw light upon the family relations of the people of this period. They show that they desired to preserve the associations of kinsfolk even after death; and when the cist contained the remains only of a child it was constructed with the same care as if it had been the tomb of a chief.

When cremated bodies are found associated with stone cists in the

same cemetery, the cinerary urns in which the ashes were customarily deposited lie outside the cists, and in quite independent excavations in the soil, but in such close proximity as to show that they belonged to the same period. In two instances short cists have been opened, in which, alongside of the skeleton of an unburnt body were cremated human bones, not contained in a cinerary urn, but scattered on the floor of the cist, which conclusively prove that both cremation and inhumation were sometimes in practice at the same interment.

One may now inquire into the reason why cinerary urns, with their contained ashes, and short cists, enclosing bodies which had been buried in a bent or stooping attitude, should be associated with the men of the Bronze Age. The first and most important is the presence of objects made of bronze. In the 144 localities under analysis in which interments ascribed to the Bronze Age have been examined, bronze articles were found in 34 directly associated with the interments. In four of these the bronze was along with objects made of gold. In seven other interments of the same character gold ornaments without bronze were present. The men of this period were, therefore, workers in gold also, and as it has been, and indeed still can be, mined in Scotland, it is not unlikely that the ornaments had been wrought from native metal. Additional proof that the burials in short cists, and after cremation in cinerary urns, both belonged to the same period, and were practised by the same people, is furnished by the presence of articles of bronze and gold in both groups of interment.

But, in addition to metallic objects, the graves sometimes contained other implements and ornaments. In many localities articles made of flint, stone, or bone and jet beads were associated with bronze. In others flints in the form of chips, knives, arrow heads and spear heads; stone implements in the form of whetstones and hammers; bone and jet ornaments and bone pins were found in short cists, and some of these articles also in cremation interments, unaccompanied by bronze.

Attention has been called by Dr. Joseph Anderson to the character of the bronze objects usually associated with these burials.* For the most part they have been thin blades, leaf-like or triangular in form, and either with or without a tang for the attachment of a handle. From their shape they might have been used as spear-heads, daggers, or knives. Not unfrequently the surfaces of the blade were ornamented with a punctated or incised pattern. Sometimes bronze pins, rings, and bracelets have been obtained from these interments. It should, however, be stated that the bronze articles and ornaments of gold found in association with the burials are of a more simple character, and present less variety in form, purpose and decoration than those which have been got in hoards in various parts of Scotland. It would seem, therefore, as if the people of this period, even if they were in possession of such finished and beautifully decorated swords,

* 'Scotland in Pagan Times.'

bucklers, axes and bronze vessels as have been got in the hoards just referred to, did not deposit them in the graves of their deceased friends and relatives. It may be, however, that the simpler articles found in the interments represent a period in the Bronze Age earlier than that in which the art of making the more elaborate articles had been acquired, when perhaps the custom of depositing grave goods had been more or less departed from.

Cinerary urns are not the only utensils formed of baked clay to which the term urn has been applied, and archæologists recognise by the names of "incense cups," "food vessels," and "drinking cups" three other varieties.

The examples of so-called incense cups are not numerous in Scotland; they were associated with cremation interments and have usually been contained in cinerary urns; they are the smallest of all the varieties of urn, and are as a rule from 2 to 3 inches high, and about 3 inches wide. In one specimen from Genoch, Ayrshire, the cup possessed a movable lid. Not unfrequently the outer surface was patterned with horizontal, vertical, and zig-zag arrangements of lines. In a few cases the sides were perforated as if to allow the escape of fumes, and it is probably from this character, as well as from their small size which fitted them for being easily carried in the hand, that they have been termed incense cups. The burning of incense would, however, imply, on the part of the people of the Bronze Age, the possession of fragrant gums and resins such as are not indigenous to Britain, and which the ancient Caledonians were not at all likely to be in a position to procure. In most instances the contents of these cups were not preserved by the finders. An example which was discovered in 1857 at Craig Dhu, North Queensferry, covered by a larger urn, and about the size of a teacup, was filled with calcined human bones; the specimen from Genoch, found a number of years ago by Dr. James Macdonald, of Ayr, contained the burned bones and ashes of a child in its fifth or sixth year. Of the conflicting theories as to the purpose to which these cups were applied, the view that, like the large urns with which they were associated, they were cinerary, and were intended for the reception of the ashes of an infant or young child, seems the most probable.

Numerous examples of the variety of urn termed "food vessel" have been found in Scotland, and "drinking cups," although not quite so numerous, are fairly represented. In the 144 localities under analysis the bowl-shaped food urns were found in 31, drinking cups in 2[illegible] and in seven instances the size and form of the urn is not stated with sufficient precision. With a few exceptions, in which the character of the burial had not been fully described, the urns were contained in short cists, in which also the skeleton of an unburnt body in the bent or contracted position, was lying. In several instances it is stated that the urn, either food or drinking vessel, contained black dust, or earth, or greasy matter, but burnt bones are never said to constitute their contents. Not unfrequently, although this is not an invariable

rule, the urn was placed in proximity to the head and raised hands of the skeleton.

These varieties of urn are by no means invariably present in short cists. In twenty-five localities where this kind of grave was seen, there is no record of either form of urn being present. It is obvious therefore that, though associated with so many inhumation interments, they were not regarded as necessary accompaniments, and they obviously discharged in the minds of the people of the time a different function from that of cinerary urns. The term food-urns applied to the bowl-shaped variety is probably appropriate, as indicating that edible substances were placed in them, in the belief that food should be provided for the use of the corpse. It is questionable, however, if the taller variety were drinking cups, as the unglazed clay would not fit them for the retention of liquids for any length of time. Their presence in the stone cists, along with, in some instances, implements and weapons, would point to the belief, in the minds of those practising this form of interment, in a resurrection of the body, and a restoration to the wants and habits of the previous life. It may be that placing the body in the crouching position, lying on one side, was regarded as the attitude best fitted, when the proper time came, to enable it to spring into the erect position and assume an active state of existence. The practice of cremation, however, to an almost equal extent as inhumation, by people of the same period, shows that they may not all have shared in the belief in a corporeal resurrection. But it should not be forgotten that, even in many cremation interments, blades and other objects made of bronze have been found along with the burnt bones and cinerary urns, as if for use in a future life.

The association of bronze objects, both with short cists and cinerary urns, establishes these forms of interment as practised at a time when bronze was the characteristic metal used in many purposes of life. The crouching attitude of the dead body, the contracted grave, and the varieties of urns already described, are therefore to be regarded as equally characteristic of this period, even if bronze is not found in a particular instance associated with the interment, and this view is generally held by archæologists in Scotland.

In a preceding paragraph implements and weapons made of stone, flint and bone were referred to as having been sometimes associated with bronze, and also of similar objects having been found in graves, in which, though obviously of the same class and period, no article made of metal was observed. Such an association proves that there was no sharp line of demarcation between the employment of the more simple substances used by Neolithic man in the manufacture of implements and weapons, and the use of bronze for similar purposes. The two periods undoubtedly overlapped. It has been customary to regard this overlapping as if bronze-using man had continued for a period to employ the same substances in making useful articles as did his Neolithic predecessors; that time was required before the more costly bronze, imported from foreign sources, replaced the native

material, and that consequently both groups of objects became associated in the same grave.

Additional light is thrown on the mixture of objects representing different stages of culture in the same interment by a collection of goods from the grave of an aboriginal Australian, buried about fifty years ago, recently brought under my notice by Dr. R. Broom. Along with the skeleton were found a clay pipe, an iron spoon, the remains of a rusted pannikin, the handle of a pocket-knife, and a large piece of flint. The handle of the knife, with its steel back, had doubtless been used along with the flint for the purpose of obtaining fire, as in Neolithic times a similar office was discharged by flint and a nodule of pyrites. These accompaniments of the Australian interments show that men in a lower grade of culture and intellectual power utilise, as opportunity offers, objects representing a much higher civilisation. It is possible, therefore, that some of the mixed interments ascribed to the Bronze Age may be the graves of Neolithic men who, in conjunction with articles of their own manufacture, had employed the material introduced by a bronze-using race, with whom they had been brought in contact, and whose usages they had more or less imitated.

That the inhabitants of prehistoric Scotland were not a homogeneous people, but exhibited different types in their physical configuration, so as to justify the conclusion that they were not all of the same race, has long been accepted by archæologists. The first observer who made a definite statement, based on anatomical data, was the late Sir Daniel Wilson, in his well-known 'Prehistoric Annals of Scotland.' Whilst admitting that the material at his disposal was scanty, he thought that he was justified in stating that the primitive race in Scotland possessed an elongated dolichocephalic head, which he termed boat-shaped, or kumbecephalic. This race, he said, was succeeded by a people with shorter and wider skulls, which possessed brachycephalic proportions. Further, he considered that both these races preceded the intrusion of the Celtæ into Scotland. But the evidence is by no means satisfactory that the interments from which Wilson obtained the long kumbecephalic skulls were of an older date than those which yielded the brachycephalic specimens. So far, therefore, as rests upon these data, one cannot consider it as proved that a long-headed race preceded a broad-headed race in Scotland, and that both were antecedent to the Celtæ.

Evidence from other quarters must be looked for, especially from the extensive researches of Thurnam, Greenwell, Rolleston and other archæologists into prehistoric interments in England; and by the study of the material which has accumulated in Scotland since the publication of Sir Daniel Wilson's 'Prehistoric Annals.'

The remains of prehistoric man in England subsequent to the Palæolithic Age have for the most part been found in mounds and tumuli, some of which were very elongated in form, others more rounded, so that they have been divided into the two groups of Long

and Round barrows. There is a consensus of opinion that the long barrows were constructed by a race which inhabited England prior to the construction of the round barrows. The long barrows are indeed the most ancient sepulchral monuments in South Britain; obviously they were erected before the use of bronze or other metal became known to the people. They belonged, therefore, to the Neolithic Age, as is testified by the implements and weapons found in them being formed of stone, flint, bone and horn, and by the absence of metals. They are not widely distributed in England, but are found especially in a few counties in the north, as Yorkshire and Westmorland, and in the Western counties in the south. The builders of these barrows in their interments practised both inhumation and cremation, but the burnt bones were never found in urns.

The study of the human remains obtained from the English long barrows by Drs. Thurnam and Rolleston proves that the crania were distinctly dolichocephalic, and that the height was greater than the breadth. Those measured by Dr. Thurnam gave a mean length-breadth index 71·4, whilst Dr. Rolleston's series were 72·6.

The round barrows were constructed by a bronze-using people. The crania obtained in them were, as a rule, brachycephalic. Of twenty-five skulls measured by Dr. Thurnam seventeen had the length-breadth index 80 and upwards, and in six of these the index was 85 and upwards. Only four were dolichocephalic, whilst in three the index ranged from 77 to 79. In the brachycephalic skulls the height was less than the breadth.

As similar physical conditions prevailed both in England and Scotland during the Polished Stone and Bronze periods, there is a strong presumption that the two races had, in succession to each other, migrated from South to North Britain. Unfortunately very few skulls have been preserved which can with certainty be ascribed to Neolithic man in Scotland, but those that have been examined from Papa Westray, the cairn of Get and Oban, are dolichocephalic, and doubtless of the same race as the builders of the English long barrows.

Seventeen skulls from interments belonging to the Bronze period have been examined by the author. The mean length-breadth index of twelve was 81·4, and the highest index was 88·6. In each skull the height was less than the breadth. In the other five specimens the mean index was 74; the majority, therefore, were brachycephalic. In only one specimen was the jaw prognathic; the nose was almost always long and narrow; the upper border of the orbit was, as a rule, thickened, and the height of the orbit was materially less than the width. The capacity of the cranium in three men ranged from 1380 to 1555 c.c.; the mean being 1462 c.c. In stature the Bronze men were somewhat taller than Neolithic men. The thigh bones of the Bronze Age skeletons gave a mean platymeric index 75·1, materially below the average of 81·8 obtained by Dr. Hepburn from measurements of the femora of modern Scots. The tibiæ of the same

skeletons gave a mean platyknemic index 68·3; intermediate, therefore, between their Neolithic predecessors and the present inhabitants of Britain. Many of the tibiæ also possessed a retroverted direction of the head of the bone; but the plane of the condylar articular surfaces was not thereby affected, so that the backward direction of the head exercised no adverse influence on the assumption of the erect attitude.

Whilst in England the Bronze Age round barrows are numerous and the burials in short cists are comparatively rare, in Scotland the opposite prevails. Whilst part of Dr. Thurnam's aphorism, viz. "long barrows, long skulls," applies to both countries; the remaining part, "short barrows, short skulls," should be modified in Scotland to "short cists, short or round skulls."

The presence of dolichocephalic skulls in the interments of the Bronze Age shows that the Neolithic people had commingled with the brachycephalic race. Similarly the Bronze men, though subject to successive invasions by Romans, Angles, and Scandinavians, have persisted as a constituent element of the people of Great Britain. The author has found a strong brachycephalic admixture in the crania of modern Scots, in Fife, the Lothians, Peebles and as far north as Shetland. In 116 specimens measured, 29, i.e. one-quarter, had a length-breadth index 80 and upwards, and in five of these the index was more than 85.

The question has been much discussed whether the people of the Polished Stone Age were descended from the men of the Ruder Stone Age, or were separated from them by a distinct interval of time. The latter view has been supported by Professor Boyd Dawkins, who contends that there is a great zoological break between the fauna of the Palæolithic, Pleistocene period and that of the Neolithic Age, and that the two periods are separated from each other by a revolution in climate, geography and animal life.*

Undoubtedly many large characteristic mammals of the Palæolithic fauna had entirely disappeared from Britain and western Europe, but some nine or ten species, as the otter, wolf, wild cat, wild boar, stag, roe, urus and horse, were continued into the Neolithic period; at which time the dog, small ox, pig, goat and perhaps the sheep, as is shown by their osseous remains, were also naturalised in Britain. The continuity of our island with the Continent by intermediate land, which existed during Palæolithic times, also became severed, and a genial temperate climate replaced more or less arctic conditions.

Man, however, possesses a power of accommodation, and of adapting himself to changes in his environment, such as is not possessed by a mere animal. The locus of an animal is regulated by the climate and the nature of the food, so that a change of climate which would destroy the special food on which an animal lives, would

* Cave Hunting and Journal of Anthropological Institute, vol. xxiii. Feb. 1894.

lead to the extinction of the animal in that locality. Man, on the other hand, is omnivorous, and can sustain himself alike on the flesh of seals, whales and bears in the Arctic circle, and on the fruits which ripen under a tropical sun. Man can produce fire to cook his food and to protect himself from cold, and can also manufacture clothing when necessary. Palæolithic man has left evidence that he had the capability to improve, for the cave men were undoubtedly in advance of the men who made the flint implements found in the river drifts. The capacity of the few crania of Palæolithic man which have been preserved is quite equal to, and in some cases superior to that of modern savages. So far as regards the implements which he manufactured and employed, Neolithic man showed no material advance over the Palæolithic cave dweller.

The association of the bones of domestic mammals, which were not present in Palæolithic strata, along with the remains of Neolithic man, proves that additional species had been introduced into Western Europe at a particular period, probably by another race which had migrated northward and westward; but it by no means follows that Palæolithic man had of necessity disappeared prior to this migration, and that when Neolithic man reached Western Europe he found it, as regards his own species, a desolate solitude. How then did Neolithic man with his associated animals find his way into Britain?

Was it whilst the land remained, which connected Britain with the Continent in interglacial times, and along which Palæolithic man had travelled, or was it at some subsequent period after the formation of intermediate arms of the sea? If the latter, then the further question arises, how was the transit effected? Neolithic man, so far as is known, had no other means of conveyance by water than was afforded by a canoe dug out of the stem of a tree. Although such rude boats might in calm weather serve as the means of transporting a few individuals across a river or narrow strait from one shore to the other, they can scarcely be regarded as fitted for an extensive migration of people; still less as a means of conveying their pigs, dogs, goats and oxen. Hence one is led to the hypothesis that, after the sea had submerged the intermediate land of interglacial times, there had been a subsequent elevation so that Britain again became a part of the continent of Europe. If one may use the expression, a "Neolithic land bridge" was produced, continental relations and climate were for a time re-established, and a free immigration of Neolithic man with his domestic animals became possible. This may have been at the period when an abundant forest growth in Scotland succeeded the elevation of what is now called the 100-foot terrace. There is no evidence of the presence of Neolithic man in Scotland until about that period. Before this island with its surrounding and protecting "silver streak" settled down to the present distribution of land and water, there are ample data, as is shown by the three sea beaches at different levels seen so distinctly on the coast of Scotland, that frequent oscillations changed the relative positions of land and sea to each other.

From the consideration of what may be called the biological data the conclusion seems not to be justified, that because climatic changes had led to a disappearance of certain characteristic Palæolithic mammals, but by no means of all, therefore Palæolithic man had vanished along with them. When Neolithic man reached western Europe, he in all likelihood found his Palæolithic predecessor settled there, and a greater or less degree of fusion took place between them. Hence, as the present inhabitants of Britain may claim the men both of the Neolithic and Bronze Ages as their ancestors, it is possible that as Neolithic man migrated northward into Scotland he may have carried with him a strain of Palæolithic blood.

[W. T.]

Charles T. Heycock, Esq. M.A. F.R.S.

Metallic Alloys and the Theory of Solution.

rm alloy in its technical sense is used to indicate a solid of two or more metals. The earlier investigators in this uch as Matthiesen, Riche and many others, worked mainly lid alloys, and they endeavoured to investigate the change in ies of the alloy, such as conductivity for heat and electricity, ility, ductility and the like, with successive small changes in ition.

method, although well adapted to bring out properties of suitable for use in the arts, has not till recently shed much the real constitution of this interesting group of substances. ts have neglected the subject because the ordinary processes ch they attack problems fail them when dealing with alloys, unt of their opacity, want of volatility and power of being d from one another by crystallisation. Another difficulty from the fact that the resulting alloy has usually the same as the metals from which it is produced, except in a few cases, the rich purple alloy of gold and aluminium investigated by or Roberts-Austen, and the alloy of zinc and silver noticed thiesen and investigated by Neville and Heycock, which has perty of taking a superficial rose tint when heated and ly cooled.

ing the past twelve years considerable advance has been made tudy of alloys by investigating some of their properties whilst liquid state, such as the temperature at which solidification nces; it is convenient to term this temperature the freezing Le Chatelier, Roberts-Austen, Neville, myself and others worked in this way. The result of this work may be very stated as follows.

determining the freezing point of the alloy we find that it is lowered in direct proportion to the weight of gold added, notwithstanding the fact that pure gold by itself melts at a temperature of 1060° C. It is remarkable that the effect of increasing the quantity of gold in the alloy continues to depress the freezing point of the sodium, until the alloy contains more than 20 per cent. of gold when the minimum freezing temperature 81·9° C (eutectic temperature) is reached. The case of gold dissolving in sodium may be taken as a very general one, for a large number of pairs of metals have been examined, and with but few exceptions, such as antimony dissolved in bismuth, the effect is almost always to produce a lowering of the freezing point of the solvent metal. By the solvent metal we generally mean the metal which is present in the largest quantity.

A second point in which metallic alloys resemble ordinary solutions is in the fact that the depression of the freezing point is inversely proportional to the molecular weight of the dissolved substance. Thus, if we dissolve 342 grams (molecular weight in grams) of cane sugar in 10 litres of water, and determine the freezing point of the solution, it is found to be depressed a definite number of degrees below that of pure water. But the same depression of the freezing point is produced by the solution of 126 grams of crystallised oxalic acid, or only 32 grams of formic acid, in 10 litres of water.* Alloys again appear to obey the same law; thus it is found that if we dissolve 197 grams of gold, or 112 grams of cadmium, or 39 grams of potassium, respectively, in a constant weight of sodium, the freezing point of the sodium will be lowered by almost the same number of degrees in each case. Now the numbers 197, 112 and 39 are the atomic weights of the metals, and it can be shown that these numbers are also probably the molecular weights of these elements. Hence we conclude that metals dissolved in each other obey the same laws as ordinary solutions.

The above facts for the behaviour of solutions of substances in water and organic liquids have been gradually accumulated by the work of Blagden, Rüdorff, Coppet and Raoult, extending from about 1780 to the present time, but no general explanation of them was brought forward until Van'tHoff advanced the remarkable theory that a dissolved substance was in a condition somewhat analogous to that of a gas, the solvent substance serving the part of the vessel in which the gas is confined, but also exerting other effects.

He further gave strong reasons for believing that substances in dilute solution obeyed the same laws that gases do—i.e. the laws of Boyle and Charles for temperature and pressure. Several other theories of solution, besides what may be termed the gaseous theory,

* Although water is used as a solvent by way of illustration in these cases, it should be stated that it is by no means a suitable liquid for such experiments, owing to the changes it brings about in the substances dissolved. In making such experiments it is far preferable to use benzene or acetic as a solvent.

have been proposed. Notwithstanding that some weighty objections can be urged against this theory, it is remarkable that we can by aid of it predict the numerical values for the fall of the freezing point of different solvents produced by the solution of other substances, provided that we know the latent heat of fusion of the solvent.

On applying the same reasoning to alloys, we find that the theory holds good, as the table below shows.* We see from this table that

OBSERVED DEPRESSION IN THE FREEZING POINT OF A SOLVENT METAL, CAUSED BY THE ADDITION OF ONE ATOMIC PER CENT. OF A SECOND METAL.

Solvent.		Tin.	Bismuth.	Cadmium.	Lead.	Zinc.
Depression calculated on theory of Van'tHoff.		3·0° C.	2·08° C.	4·5° C.	6·5° C.	5·11° C.
Metal dissolved	At. Wt.					
Sodium	23	2·8	2·0	4·5	1·2	..
Copper	63	2·9	1·2	3·6	6·3	1·5 (rise)
Silver	108	2·9	2·0	10·8 (rise)	6·6	5·15 (rise)
Platinum ..	195	..	2·1	4·5	6·4	..
Gold	197	2·9	2·1	1·6	6·4	3·4 (rise)
Bismuth ..	209	2·4	..	4·5	3·0	5·1

in no cases are the observed depressions of the freezing points greater than those calculated from the theory, but in many cases they fall below this quantity; this latter fact admits of explanation.

On the theory of Van'tHoff it is necessary that when a solution begins to freeze the pure solvent should separate out first. This admits, in case of aqueous solutions, of simple proof, for if we take a dilute solution of potassium permanganate and make it freeze slowly, we find that pure colourless ice separates out on the walls of the vessel, whilst the purple permanganate is concentrated towards the centre. This experiment led Neville and myself to try if a similar state of things could be shown for metallic alloys.

We have great pleasure in bringing before the Royal Institution this evening the first announcement of the results we have obtained. For this purpose we took two metals, gold and sodium, the former being very opaque to X-rays, whilst the latter is very transparent to them. A quantity of sodium was melted in a tube, and gold dissolved in it to the extent of about ten per cent. The alloy was then allowed to cool extremely slowly, and sections (about ½ inch thick) were cut from different parts of the solid alloy and placed between thin plates

* For the nature of this calculation, *vide* Heycock & Neville, Chem. Soc. Jour. vol. lvii. p. 339. Also Neville, 'Science Progress,' October 1895.

of aluminium to protect them from the air. These sections were then placed on a photographic plate, enclosed in a light-tight bag, and exposed to the action of the X-rays. On developing the plate we found a complete picture of the inside of the alloy. Positives obtained from these negatives are thrown upon the screen. The sodium is seen to have crystallised out in plates, as is evident from its transparency, whilst the opaque gold is seen to have become concentrated in the mother liquor between these plates, where it finally solidified along with some of the sodium.

Very similar results are produced with other pairs of metals, such as aluminium and gold and aluminium and copper. Behrens, Roberts-Austen, Osmond and others have examined alloys, after superficial etching, with high microscopic powers, and they find a similar separation of the constituents.

We thus see that solution of metals in one another follows extremely closely the same laws that regulate solutions with which we are ordinarily familiar. I should like to state here that the matter of this lecture is largely drawn from the work carried out by Mr. Neville, F.R.S. and myself during the past six years.

[C. T. H.]

GENERAL MONTHLY MEETING.

Monday, April 5, 1897.

[J]AMES CRICHTON-BROWNE, M.D. LL.D. F.R.S. Treasurer and Vice-President, in the Chair.

John Howard Colls, Esq.
Hugh Ernest Diamond, Esq.
Mrs. J. Dundas Grant,
Douglas Hall, Esq.
Walter Hunter, Esq.
Frederick Morell Mackenzie, Esq. M.R.C.S.

[e]lected Members of the Royal Institution.

[Th]e Special Thanks of the Members were returned for the following [donati]on to the Fund for the Promotion of Experimental Research [at low] Temperatures:—

Sir William J. Farrer £50

[Th]e following Lecture Arrangements were announced:—

[RO]BERT ANDERSON, M.D. B.Sc. Four Lectures on VOLCANOES. (The [Tyndal]l Lectures.) On *Tuesdays*, April 27, May 4, 11, 18.

[ERN]EST HENRY STARLING, M.D. Three Lectures on THE HEART AND ITS [WORK]. On *Tuesdays*, May 25, June 1, 8.

[PRO]FESSOR DEWAR, M.A. LL.D. F.R.S. *M.R.I.* Three Lectures on LIQUID [AIR AS] AN AGENT OF RESEARCH. On *Thursdays*, April 29, May 6, 13.

[CHUR]TON COLLINS, ESQ., M.A. Four Lecture son THE FRENCH REVOLUTION [AND EN]GLISH LITERATURE. On *Thursdays*, May 20, 27, June 3, 10.

[The] REV. J. P. MAHAFFY, D.D. Professor of Ancient History in the Uni[versity] of Dublin. Three Lectures on THE GREEK THEATRE ACCORDING TO [RECENT] DISCOVERIES. On *Saturdays*, May 1, 8, 15.

[J.] A. FULLER MAITLAND, ESQ., M.A. F.S.A. Four Lectures on MUSIC IN [ENGLAN]D DURING THE REIGN OF QUEEN VICTORIA (with Musical Illustrations). [On *Sat*]*urdays*, May 22, 29, June 5, 12.

[Th]e PRESENTS received since the last Meeting were laid on the [table] and the thanks of the Members returned for the same, viz.:—

[Br]*itish Museum Trustees*—Catalogue of the Cuneiform Tablets in the [Ko]uyunjik Collection. By C. Bezold. Vol. IV. 4to. 1896.

[Accadem]*ia dei Lincei, Reale, Roma*—Atti, Serie Quinta: Rendiconti. Classe di [Sci]enze Fisiche, etc. 1° Semestre, Vol. VI. Fasc. 3–5. 8vo. 1897.

[Agricult]*ural Society of England, Royal*—Journal, Third Series, Vol. VIII. Part 1. [8vo]. 1897.

[America]*n Academy of Arts and Sciences*—Proceedings, Vol. XXXII. No. 1. 8vo. [189]6.

[America]*n Philosophical Society*—Proceedings, No. 152. 8vo. 1896.

[Asiatic] *Society, Royal*—Catalogue of the Library of the Royal Asiatic Society. [8vo]. 1893.

[Journ]al for 1888, 1889 and 1890. 8vo.

Astronomical Society, Royal—Monthly Notices, Vol. LVII. No. 4. 8vo. 1897.
Bankers, Institute of—Journal, Vol. XVIII. Part 3. 8vo. 1897.
Berlin, Königlich Preussische Akademie der Wissenschaften—Sitzungsberichte, Nos. 49–53. 8vo. 1896.
Bevan, The Rev. J. O. M.R.I. (the Author)—An Archæological Survey of Herefordshire. By J. O. Bevan and others. 4to. 1896.
Boston, U.S.A. Public Library—Monthly Bulletin of Books added to the Library, Vol. II. No. 3. 8vo. 1897.
Boston Society of Natural History—Proceedings, Vol. XXVII. (pp. 201-241). 8vo. 1896.
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Camera Club—Journal for March 1897. 8vo.
Chemical Industry, Society of—Journal, Vol. XVI. No. 2. 8vo. 1897.
Chemical Society—Journal for Jan. and Feb. 1897. 8vo.
Proceedings, No. 173. 8vo. 1897.
Civil Engineers, Institution of—Minutes of Proceedings, Vol. CXXVII. 8vo. 1897.
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De Candolle, Casimir, Esq. M.R.I.—Genève et la Société de Lecture. Par F. De Crue (1818-96), avec portraits. 8vo. 1896.
Editors—American Journal of Science for March, 1897. 8vo.
Analyst for March, 1897. 8vo.
Anthony's Photographic Bulletin for March, 1897. 8vo.
Astrophysical Journal for March, 1897. 8vo.
Athenæum for March, 1897. 4to.
Author for March, 1897.
Bimetallist for March, 1897.
Brewers' Journal for March, 1897. 8vo.
Chemical News for March, 1897. 4to.
Chemist and Druggist for March, 1897. 8vo.
Education for March, 1897. 8vo.
Electrical Engineer for March, 1897. fol.
Electrical Engineering for March, 1897.
Electrical Review for March, 1897. 8vo.
Engineer for March, 1897. fol.
Engineering for March, 1897. fol.
Homœopathic Review for March, 1897.
Horological Journal for March, 1897. 8vo.
Industries and Iron for March, 1897. fol.
Invention for March, 1897. 8vo.
Journal of Physical Chemistry, Vol. I. Nos. 6, 7. 8vo. 1897.
Journal of State Medicine for March, 1897. 8vo.
Law Journal for March, 1897. 8vo.
Machinery Market for March, 1897. 8vo.
Nature for March, 1897. 4to.
New Book List for March, 1897. 8vo.
New Church Magazine for March, 1897. 8vo.
Nuovo Cimento for Feb. 1897. 8vo.
Physical Review for March–April, 1897. 8vo.
Science Siftings for March, 1897. 8vo.
Travel for March, 1897.
Tropical Agriculturist for Feb. 1897. 8vo.
Zoophilist for March, 1897. 4to.
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Biennial Report of the Biological Experiment Station. 8vo. 1897.
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anchester Geological Society—Transactions, Vol. XXV. Part 3. 8vo. 1897.
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Liste des Publications de l'Auteur. 8vo. 1896.
Sur la théorie de l'explosion d'une bulle de savon très mince. 8vo. 1897.
Principes généraux d'une nouvelle théorie capillaire. 8vo. 1896.
Démonstration très simple de la cause commune de la tension superficielle et l'évaporation des liquides. 8vo. 1896.
Conservation des Toiles peintes. 8vo.
Sur une analogie très importante entre la constitution des solides et des liquides. 8vo. 1895.
Sur les pressions exercées par les liquides en mouvement ou en repos. 8vo.
Quelques expériences propres à faire comprendre la constitution des liquides. 8vo. 1895.
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United States Department of Agriculture—Experiment Station Record, Vol. VIII. No. 4. 8vo. 1896.
Monthly Weather Review for Dec. 1896. 8vo.
United States Patent Office—Official Gazette, Vol. LXXVII. Nos. 9–13; Vol. LXXVIII. Nos. 1–3. 8vo. 1896.
United States Geological Survey—Seventeenth Annual Report, 1895–96, Part 3 (2 vols.). 4to. 1896.
Verein zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, 1897, Heft 2. 4to.
Vienna, Geological Institute, Imperial—Verhandlungen, 1897, Nos. 1–3. 8vo.
Wright, Messrs. J. & Co. (the Publishers)—The Medical Annual for 1897. 8vo.

WEEKLY EVENING MEETING,

Friday, April 9, 1897.

SIR FREDERICK BRAMWELL, Bart. D.C.L. LL.D. F.R.S. Honorary Secretary and Vice-President, in the Chair.

HE RIGHT HON. LORD RAYLEIGH, M.A. D.C.L. LL.D. F.R.S. *M.R.I.* Professor of Natural Philosophy, *R.I.*

The Limits of Audition.

(Abstract.)

order to be audible, sounds must be restricted to a certain range pitch. Thus a sound from a hydrogen flame vibrating in a large sonator was inaudible, as being too low in pitch. On the other le, a bird-call, giving about 20,000 vibrations per second, was audible, although a sensitive flame readily gave evidence of the brations and permitted the wave-length to be measured. Near e limit of hearing the ear is very rapidly fatigued; a sound in the st instance loud enough to be disagreeable, disappearing after a w seconds. A momentary intermission, due, for example, to a rapid ssage of the hand past the ear, again allows the sound to be heard.

The magnitude of vibration necessary for audition at a favourable ch is an important subject for investigation. The earliest estimate hat of Boltzmann. An easy road to a superior limit is to find amount of energy required to blow a whistle and the distance to h the sound can be heard (e.g. one-half a mile). Experiments this plan gave for the amplitude 8×10^{-8} cm., a distance would need to be multiplied 100 times in order to make it e in any possible microscope. Better results may be obtained ng a vibrating fork as a source of sound. The energy resident fork at any time may be deduced from the amplitude as ob- under a microscope. From this the rate at which energy is follows when we know the rate at which the vibrations of k die down (say to one-half). In this way the distance ility may be reduced to 30 metres, and the results are less be disturbed by atmospheric irregularities. If s be the nal condensation in the waves which are just capable of udition, the results may be expressed:—

c'	frequency = 256	$s = 6{\cdot}0 \times 10^{-9}$
g'	" = 384	$s = 4{\cdot}6 \times 10^{-9}$
c''	" = 512	$s = 4{\cdot}6 \times 10^{-9}$

at the ear is capable of recognising vibrations which less changes of pressure than the total pressure out- our highest vacua.

In such experiments the whole energy emitted is very small, and contrasts strangely with the 60 horse-power thrown into the fog-signals of the Trinity House. If we calculate according to the law of inverse squares how far a sound absorbing 60 horse-power should be audible, the answer is 2700 kilometres! The conclusion plainly follows that there is some important source of loss beyond the mere diffusion over a larger surface. Many years ago Sir George Stokes calculated the effect of radiation upon the propagation of sound. His conclusion may be thus stated. The amplitude of sound propagated in plane waves would fall to half its value in six times the interval of time occupied by a mass of air heated above its surroundings in cooling through half the excess of temperature. There appear to be no data by which the latter interval can be fixed with any approach to precision; but if we take it at one minute, the conclusion is that sound would be propagated for six minutes, or travel over about seventy miles, without very serious loss from this cause.

The real reason for the falling off at great distances is doubtless to be found principally in atmospheric refraction due to variation of temperature, and of wind, with height. In a normal state of things the air is cooler overhead, sound is propagated more slowly, and a wave is tilted up so as to pass over the head of an observer at a distance. [Illustrated by a model.] The theory of these effects has been given by Stokes and Reynolds, and their application to the explanation of the vagaries of fog signals by Henry. Progress would be promoted by a better knowledge of what is passing in the atmosphere over our heads.

The lecture concluded with an account of the observations of Preyer upon the delicacy of pitch perception, and of the results of Kohlrausch upon the estimation of pitch when the total number of vibrations is small. In illustration of the latter subject an experiment (after Lodge) was shown, in which the sound was due to the oscillating discharge of a Leyden battery through coils of insulated wire. Observation of the spark proved that the total number of (aerial) vibrations was four or five. The effect upon the pitch of moving one of the coils so as to vary the self-induction was very apparent.

[R.]

WEEKLY EVENING MEETING,

Friday, April 30, 1897.

Sir Frederick Bramwell, Bart. D.C.L. LL.D. F.R.S. Honorary Secretary and Vice-President, in the Chair.

Professor J. J. Thomson, M.A. LL.D. Sc.D. F.R.S.

Cathode Rays.

The first observer to leave any record of what are now known as the Cathode Rays seems to have been Plücker, who in 1859 observed the now well known green phosphorescence on the glass in the neighbourhood of the negative electrode. Plücker was the first physicist to make experiments on the discharge through a tube, in a state anything approaching what we should now call a high vacuum: he owed the opportunity to do this to his fellow townsman Geissler, who first made such vacua attainable. Plücker, who had made a very minute study of the effect of a magnetic field on the ordinary discharge which stretches from one terminal to the other, distinguished the discharge which produced the green phosphorescence from the ordinary discharge, by the difference in its behaviour when in a magnetic field. Plücker ascribed these phosphorescent patches to currents of electricity which went from the cathode to the walls of the tube, and then for some reason or other retraced their steps.

The subject was next taken up by Plücker's pupil, Hittorf, who greatly extended our knowledge of the subject, and to whom we owe the observation that a solid body placed between a pointed cathode and the walls of the tube cast a well defined shadow. This observa- ... was extended by Goldstein, who found that a well marked, ...gh not very sharply defined shadow was cast by a small body ...d near a cathode of considerable area; this was a very important ...rvation, for it showed that the rays casting the shadow came in ...inite direction from the cathode. If the cathode were replaced ... luminous disc of the same size, this disc would not cast a shadow ...mall object placed near it, for though the object might intercept ...ays which came out normally from the disc, yet enough light ...d be given out sideways from other parts of the disc to prevent ...hadow being at all well marked. Goldstein seems to have been ...rst to advance the theory, which has attained a good deal of ...ence in Germany, that these cathode rays are transversal ...ons in the ether.

... physicist, however, who did more than any one else to direct ...on to these rays was Mr. Crookes, whose experiments, by their ... and importance, attracted the attention of all physicists to this

subject, and who not only greatly increased our knowledge of the properties of the rays, but by his application of them to radiant matter spectroscopy has rendered them most important agents in chemical research.

Recently a great renewal of interest in these rays has taken place, owing to the remarkable properties possessed by an offspring of theirs, for the cathode rays are the parents of the Röntgen rays.

I shall confine myself this evening to endeavouring to give an account of some of the more recent investigations which have been made on the cathode rays. In the first place, when these rays fall on a substance they produce changes physical or chemical in the nature of the substance. In some cases this change is marked by a change in the colour of the substance, as in the case of the chlorides of the alkaline metals. Goldstein found that these when exposed to the cathode rays changed colour, the change, according to E. Wiedemann and Ebert, being due to the formation of a subchloride. Elster and Geitel have recently shown that these substances become photo-electric, i.e. acquire the power of discharging negative electricity under the action of light, after exposure to the cathode rays. But though it is only in comparatively few cases that the change produced by the cathode rays shows itself in such a conspicuous way as by a change of colour, there is a much more widely spread phenomenon which shows the permanence of the effect produced by the impact of these rays. This is the phenomenon called by its discoverer, Prof. E. Wiedemann, thermoluminescence. Prof. Wiedemann finds that if bodies are exposed to the cathode rays for some time, when the bombardment stops the substance resumes to all appearance its original condition; when, however, we heat the substance, we find that a change has taken place, for the substance now, when heated, becomes luminous at a comparatively low temperature, one far below that of incandescence; the substance retains this property for months after the exposure to the rays has ceased. The phenomenon of thermoluminescence is especially marked in bodies which are called by Van t'Hoff solid solutions; these are formed when two salts, one greatly in excess of the other, are simultaneously precipitated from a solution. Under these circumstances the connection between the salts seems of a more intimate character than that existing in a mechanical mixture. I have here a solid solution of $CaSo_4$ with trace of $MnSo_4$, and you will see that after exposure to the cathode rays it becomes luminous when heated. Another proof of the alteration produced by these rays is the fact, discovered by Crookes, that after glass has been exposed for a long time to the impact of these rays, the intensity of its phosphorescence is less than when the rays first began to fall upon it. This alteration lasts for a long time, certainly for months, and Mr. Crookes has shown that it is able to survive the heating up of the glass to allow of the remaking of the bulb. I will now leave the chemical effects produced by these rays, and pass on to consider their behaviour when in a magnetic field.

...st, let us consider for a moment the effect of magnetic force ... ordinary discharge between terminals at a pressure much ... than that at which the cathode rays begin to come off. I have

FIG. 1. FIG. 2.

...hotographs (see Figs. 1 and 2) of the spark in a magnetic field. ...e that when the discharge, which passes as a thin bright line ...n the terminals, is acted upon by the magnetic field, it is pulled ...s a stretched string would be if acted upon by a force at right

FIG. 3. FIG. 4.

... to its length. The curve is quite continuous, and though there ...be gaps in the luminosity of the discharge, yet there are no ...at such points in the curve, into which the discharge is bent by

a magnet. Again, if the discharge, instead of taking place between points, passes between flat discs, the effect of the magnetic force is to move the sparks as a whole, the sparks keeping straight until their terminations reach the edges of the discs. The fine thread-like discharge is not much spread out by the action of the magnetic field. The appearance of the discharge indicates that when the discharge passes through the gas it manufactures out of the gas something stretching from terminal to terminal, which, unlike a gas, is capable of sustaining a tension. The amount of deflection produced, other circumstances being the same, depends on the nature of the gas; as the photographs (Figs. 3 and 4) show, the deflection is very small in the case of hydrogen, and very considerable in the case of carbonic acid; as a general rule it seems smaller in elementary than in compound gases.

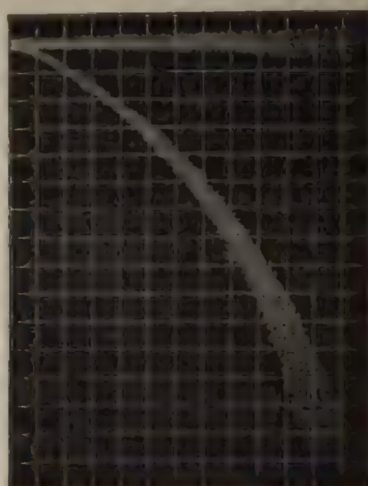

FIG. 5.—Hydrogen (Ammeter, 12; Voltmeter, 1600).

Let us contrast the behaviour of this kind of discharge under the action of a magnetic field with that of the cathode rays. I have here some photographs (Figs 5, 6 and 7) taken of a narrow beam formed by sending the cathode rays through a tube in which there was a plug with a slit in it, the plug being used as an anode and connected with the earth, these rays traversing a uniform magnetic field. The narrow beam spreads out under the action of the magnetic force into a broad fan-shaped luminosity in the gas. The luminosity in this fan is not uniformly distributed, but is condensed along certain lines. The phosphorescence produced when the rays reach the glass is also not uniformly distributed; it is much spread out, showing that the beam consists of rays which are not all deflected to the same extent

magnet. The luminous patch on the glass is crossed by bands which the luminosity is very much greater than in the adjacent

FIG. 6.—Air.

FIG. 7.—Carbonic Acid Gas (Ammeter, 12; Voltmeter, 1600).

These bright and dark bands are called by Birkeland, who served them, "the magnetic spectrum." The brightest places

on the glass are by no means always the terminations of the brightest streaks of luminosity in the gas; in fact, in some cases a very bright spot on the glass is not connected with the cathode by any appreciable luminosity, though there is plenty of luminosity in other parts of the gas.

One very interesting point brought out by the photographs is that in a given magnetic field, with a given mean potential difference between the terminals, the path of the rays is independent of the nature of the gas; photographs were taken of the discharge in hydrogen, air, carbonic acid, methyl iodide, i.e. in gases whose densities range from 1 to 70, and yet not only were the paths of the most deflected rays the same in all cases, but even the details, such as the distribution of the bright and dark spaces, were the same; in fact, the photographs could hardly be distinguished from each other. It is to be noted that the pressures were not the same; the pressures were adjusted until the mean potential difference was the same. When the pressure of the gas is lowered, the potential difference between the terminals increases, and the deflection of the rays produced by a magnet diminishes, or at any rate the deflection of the rays where the phosphorescence is a maximum diminishes. If an air break is inserted in the circuit an effect of the same kind is produced. In all the photographs of the cathode rays one sees indications of rays which stretch far into the bulb, but which are not deflected at all by a magnet. Though they stretch for some two or three inches, yet in none of these photographs do they actually reach the glass. In some experiments, however, I placed inside the tube a screen, near to the slit through which the cathode rays came, and found that no appreciable phosphorescence was produced when the non-deflected rays struck the screen, while there was vivid phosphorescence at the places where the deflected rays struck the screen. These non-deflected rays do not seem to exhibit any of the characteristics of cathode rays, and it seems possible that they are merely jets of uncharged luminous gas shot out through the slit from the neighbourhood of the cathode by a kind of explosion when the discharge passes.

The curves described by the cathode rays in a uniform magnetic field are, very approximately at any rate, circular for a large part of their course; this is the path which would be described if the cathode rays marked the path of negatively electrified particles projected with great velocities from the neighbourhood of the negative electrode. Indeed, all the effects produced by a magnet on these rays, and some of these are complicated, as, for example, when the rays are curled up into spirals under the action of a magnetic force, are in exact agreement with the consequences of this view.

We can, moreover, show by direct experiment that a charge of negative electricity follows the course of the cathode rays. One way in which this has been done is by an experiment due to Perrin, the details of which are shown in the accompanying figure (Fig. 8.) In this experiment the rays are allowed to pass inside a metallic cylinder

through a small hole, and the cylinder, when these rays enter it, gets a negative charge, while if the rays are deflected by a magnet, so as to escape the hole, the cylinder remains without charge. It seems to me that to the experiment in this form it might be objected that, though the experiment shows that negatively electrified bodies are projected normally from the cathode, and are deflected by a magnet, it does not show that when the cathode rays are deflected by a magnet the path of the electrified particles coincides with the path of the cathode rays. The supporters of the theory that these rays are waves

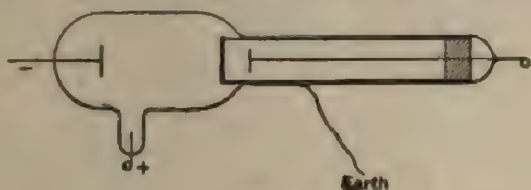

Fig. 8.

in the ether might say, and indeed have said, that while they did not deny that electrified particles might be shot off from the cathode, these particles were, in their opinion, merely accidental accompaniments of the rays, and were no more to do with the rays than the bullet has with the flash of a rifle. The following modification of Perrin's experiment is not, however, open to this objection: Two co-axial cylinders (Fig. 9), with slits cut in them, the outer cylinder being connected with earth, the inner with the electrometer, are placed in the discharge tube, but in such a position that the cathode

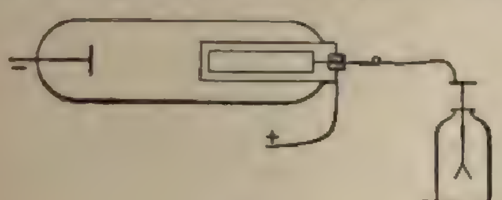

Fig. 9.

rays do not fall upon them unless deflected by a magnet; by means of a magnet, however, we can deflect the cathode rays until they fall on the slit in the cylinder. If under these circumstances the cylinder gets a negative charge when the cathode rays fall on the slit, and remains uncharged unless they do so, we may conclude, I think, the stream of negatively-electrified particles is an invariable accompaniment of the cathode rays. I will now try the experiment. You notice that when there is no magnetic force, though the rays do not fall on the cylinder, there is a slight deflection of the electrometer,

showing that it has acquired a small negative charge. This is, I think, due to the plug getting negatively charged under the torrent of negatively electrified particles from the cathode, and getting out cathode rays on its own account which have not come through the slit. I will now deflect the rays by a magnet, and you will see that at first there is little or no change in the deflection of the electrometer, but that when the rays reach the cylinder there is at once a great increase in the deflection, showing that the rays are pouring a charge of negative electricity into the cylinder. The deflection of the electrometer reaches a certain value and then stops and remains constant, though the rays continue to pour into the cylinder. This is due to the fact that the gas traversed by the cathode rays becomes a conductor of electricity, and thus, though the inner cylinder is perfectly insulated when the rays are not passing, yet as soon as the rays pass through the bulb the air between the inner cylinder and the outer one, which is connected with the earth, becomes a conductor, and the electricity escapes from the inner cylinder to the earth. For this reason the charge within the inner cylinder does not go on continually increasing: the cylinder settles into a state of equilibrium in which the rate at which it gains negative electricity from the rays is equal to the rate at which it loses it by conduction through the air. If we charge up the cylinder positively it rapidly loses its positive charge and acquires a negative one, while if we charge it up negatively it will leak if its initial negative potential is greater than its equilibrium value.

I have lately made some experiments which are interesting from the bearing they have on the charges carried by the cathode rays, as well as on the production of cathode rays outside the tube. The experiments are of the following kind. In the tube (Fig. 10) A and B are terminals. C is a long side tube into which a closed metallic cylinder fits lightly. This cylinder is made entirely of metal except the end furthest from the terminals, which is stopped by an ebonite plug, perforated by a small hole so as to make the pressure inside the cylinder equal to that in the discharge tube. Inside the cylinder there is a metal disc supported by a metal rod which passes through the ebonite plug, and is connected with an electrometer, the wires making this connection being surrounded by tubes connected with the earth so as to screen off electrostatic induction. If the end of the cylinder is made of thin aluminium about $\frac{1}{20}$th of a millimetre thick, and a discharge sent between the terminals, A being the cathode, then at pressures far higher than those at which the cathode rays come off, the disc inside the cylinder acquires a positive charge. And if it is charged up independently the charge leaks away, and it leaks more rapidly when the disc is charged negatively than when it is charged positively; there is, however, a leak in both cases, showing that conduction has taken place through the gas between the cylinder and the disc. As the pressure in the tube is diminished the positive charge on the disc diminishes until it becomes unappreciable. The

leak from the disc when it is charged still continues, and is now equally rapid, whether the original charge on the disc is positive or negative. When the pressure falls so low that cathode rays begin to fall on the end of the cylinder, then the disc acquires a negative charge, and the leak from the disc is more rapid when it is charged positively than when it is charged negatively. If the cathode rays are pulled off the end of the cylinder by a magnet, then the negative charge on the disc and the rate of leak from the disc when it is positively charged is very much diminished. A very interesting point is that these effects, due to the cathode rays, are observed behind comparatively thick walls. I have here a cylinder whose base is brass about 1 mm. thick, and yet when this is exposed to the cathode rays the disc behind it gets a negative charge, and leaks if charged positively. The effect is small compared with that in the cylinder with the thin aluminium base, but is quite appreciable. With the cylinder with the thick end I have never been able to observe any effect at the higher pressures when no cathode rays were coming off. The effect with the cylinder with the thin end was observed when the discharge was produced by a large number of small storage cells, as well as when it was produced by an induction coil.

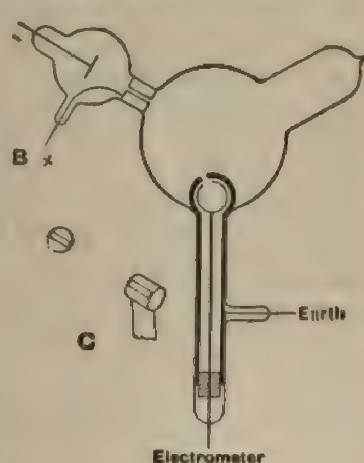

Fig. 10.

It would seem from this experiment that the incidence of the cathode rays on a brass plate as much as 1 mm. thick, and connected with the earth, can put a rarefied gas shielded by the plate into a condition in which it can conduct electricity, and that a body placed behind this screen gets a negative charge, so that the side of the brass away from the cathode rays acts itself like a cathode though kept permanently to earth. In the case of the thick brass the effect seems much more likely to be due to a sudden change in the potential of the outer cylinder at the places where the rays strike, rather than to the penetration of any kinds of waves or rays. If the discharge in the tube was perfectly continuous the potential of the outer cylinder would be constant, and since it is connected to earth by a wire through which no considerable current flows, the potential must be approximately that of the earth. The discharge there cannot be continuous; the negative charge must come in gusts against the ends of the cylinder, coming so suddenly that the electricity has no time to distribute itself over the cylinder so as to shield off the inside from the

electrostatic action of the cathode rays; this force penetrates the cylinder and produces a discharge of electricity from the far side of the brass.

Another effect which I believe is due to the negative electrification carried by the rays is the following. In a very highly exhausted tube provided with a metal plug, I have sometimes observed, after the coil has been turned off, bright patches on the glass; these are deflected by a magnet, and seem to be caused by the plug getting such a large negative charge that the negative electricity continues to stream from it after the coil is stopped.

An objection sometimes urged against the view that these cathode rays consist of charged particles, is that they are not deflected by an electrostatic force. If, for example, we make, as Hertz did, the rays pass between plates connected with a battery, so that an electrostatic force acts between these plates, the cathode ray is able to traverse this space without being deflected one way or the other. We must remember, however, that the cathode rays, when they pass through a gas make it a conductor, so that the gas acting like a conductor screens off the electric force from the charged particle, and when the plates are immersed in the gas, and a definite potential difference established between the plates, the conductivity of the gas close to the cathode rays is probably enormously greater than the average conductivity of the gas between the plates, and the potential gradient on the cathode rays is therefore very small compared with the average potential gradient. We can, however, produce electrostatic results if we put the conductors which are to deflect the rays in the dark space next the cathode. I have here a tube in which, inside the dark space next the cathode, two conductors are inserted; the cathode rays start from the cathode and have to pass between these conductors; if, now, I connect one of these conductors to earth there is a decided deflection of the cathode rays, while if I connect the other electrode to earth there is a deflection in the opposite direction. I ascribe this deflection to the gas in the dark space either not being a conductor at all, or if a conductor, a poor one compared to the gas in the main body of the tube.

Goldstein has shown that if a tube is furnished with two cathodes, when the rays from one cathode pass near the other they are repelled from it. This is just what would happen if the dark space round the electrode were an insulator, and so able to transmit electrostatic attractions or repulsions. To show that the gas in the dark space differs in its properties from the rest of the gas, I will try the following experiment. I have here two spherical bulbs connected together by a glass tube; one of these bulbs is small, the other large; they each contain a cathode, and the pressure of the gas is such that the dark space round the cathode in the small bulb completely fills the bulb, while that round the one in the larger bulb does not extend to the walls of the bulb. The two bulbs are wound with wire, which connects the outsides of two Leyden jars; the insides of these jars

are connected with the terminals of a Wimshurst machine. When sparks pass between these terminals currents pass through the wire which induce currents in the bulbs, and cause a ring discharge to pass through them. Things are so arranged that the ring is faint in the larger bulb, bright in the smaller one. On making the wires in these bulbs cathodes, however, the discharge in the small bulb, which is filled by the dark space, is completely stopped, while that in the larger one becomes brighter. Thus the gas in the dark space is changed, and in the opposite way from that in the rest of the tube. It is remarkable that when the coil is stopped the ring discharge on both bulbs stops, and it is some time before it starts again.

The deflection excited on each other by two cathodic streams would seem to have a great deal to do with the beautiful phosphorescent figures which Goldstein obtained by using cathodes of different shapes. I have here two bulbs containing cathodes shaped like a cross; they are curved, and of the same radius as the bulb, so that if the rays came off these cathodes normally the phosphorescent picture ought to be a cross of the same size as the cathode, instead of being of the same size. You see that in one of these bulbs the image of the cross consists of two large sectors at right angles to each other, bounded by bright lines, and in the other, which is at a lower pressure, the geometrical image of the cross, instead of being bright, is dark, while the luminosity occupies the space between the arms of the cross.

So far I have only considered the behaviour of the cathode rays inside the bulb, but Lenard has been able to get these rays outside the tube. To this he let the rays fall on a window in the tube, made of thin aluminium about $\frac{1}{100}$th of a millimetre thick, and he found that from this window there proceeded in all directions rays which were deflected by a magnet, and which produced phosphorescence when they fell upon certain substances, notably upon tissue paper soaked in a solution of pentadekaparalolylketon. The very thin aluminium is difficult to get, and Mr. McClelland has found that if it is not necessary to maintain the vacuum for a long time, oiled silk answers admirably for a window. As the window is small the phosphorescent patch produced by it is not bright, so that I will show instead the other property of the cathode rays, that of carrying with them a negative charge. I will place this cylinder in front of the hole, connect it with the electrometer, turn on the rays, and you will see the cylinder gets a negative charge; indeed this charge is large enough to produce the well known negative figures when the rays fall on a piece of ebonite which is afterwards dusted with a mixture of red lead and sulphur.

From the experiments with the closed cylinder we have seen that when the negative rays come up to a surface even as thick as a millimetre, the opposite side of that surface acts like a cathode, and gives off the cathodic rays; and from this point of view we can understand the very interesting result of Lenard that the magnetic deflection of

the rays outside the tube is independent of the density and chemical composition of the gas outside the tube, though it varies very much with the pressure of the gas inside the tube. The cathode rays could be started by an electric impulse which would depend entirely on what was going on inside the tube; since the impulse is the same the momentum acquired by the particles outside would be the same; and as the curvature of the path only depends on the momentum, the path of these particles outside the tube would only depend on the state of affairs inside the tube.

The investigation by Lenard on the absorption of these rays shows that there is more in his experiment than is covered by this consideration. Lenard measured the distance these rays would have to travel before the intensity of the rays fell to one-half their original value. The results are given in the following table:—

Substance.	Coefficient of Absorption.	Density.	$\frac{\text{Absorption}}{\text{Density}}$
Hydrogen (3 mm. press.)	0·00149	0·000000368	4040
,, (760)	0·476	0·0000484	5640
Air (0·760 mm. press.)	3·42	0·00123	2780
SO_2	8·51	0·00271	3110
Collodion	3,310	1·1	3010
Glass	7,810	2·47	3160
Aluminium	7,150	2·70	2650
Silver	32,200	10·5	3070
Gold	53,600	19·3	2880

We see that though the densities and the coefficient of absorption vary enormously, yet the ratio of the two varies very little, and the results justify, I think, Lenard's conclusion that the distance through which these rays travel only depends on the density of the substance —that is, the mass of matter per unit volume, and not upon the nature of the matter.

These numbers raise a question which I have not yet touched upon, and that is the size of the carriers of the electric charge. Are they or are they not the dimensions of ordinary matter?

We see from Lenard's table that a cathode ray can travel through air at atmospheric pressure a distance of about half a centimetre before the brightness of the phosphorescence falls to about one-half of its original value. Now the mean free path of the molecule of air at this pressure is about 10^{-5} cm., and if a molecule of air were projected it would lose half its momentum in a space comparable with the mean free path. Even if we suppose that it is not the same molecule that is carried, the effect of the obliquity of the collisions would reduce the momentum to one-half in a short multiple of that path.

Thus, from Lenard's experiments on the absorption of the rays outside the tube, it follows on the hypothesis that the cathode rays

are charged particles moving with high velocities, that the size of the carriers must be small compared with the dimensions of ordinary atoms or molecules. The assumption of a state of matter more finely subdivided than the atom of an element is a somewhat startling one; but a hypothesis that would involve somewhat similar consequences—viz. that the so-called elements are compounds of some primordial element—has been put forward from time to time by various chemists. Thus, Prout believed that the atoms of all the elements were built up of atoms of hydrogen, and Mr. Norman Lockyer has advanced weighty arguments, founded on spectroscopic consideration, in favour of the composite nature of the elements.

Let us trace the consequence of supposing that the atoms of the elements are aggregations of very small particles, all similar to each other; we shall call such particles corpuscles, so that the atoms of the ordinary elements are made up of corpuscles and holes, the holes being predominant. Let us suppose that at the cathode some of the molecules of the gas get split up into these corpuscles, and that these, charged with negative electricity and moving at a high velocity, form the cathode rays. The distance these rays would travel before losing a given fraction of their momentum would be proportional to the mean free path of the corpuscles. Now, the things these corpuscles strike against are other corpuscles, and not against the molecules as a whole; they are supposed to be able to thread their way between the interstices in the molecule. Thus the mean free path would be proportional to the number of these corpuscles; and, therefore, since each corpuscle has the same mass to the mass of unit volume—that is, to the density of the substance, whatever be its chemical nature or physical state. Thus the mean free path, and therefore the coefficient of absorption, would depend only on the density; this is precisely Lenard's result.

We see, too, on this hypothesis, why the magnetic deflection is the same inside the tube whatever be the nature of the gas, for the carriers of the charge are the corpuscles, and these are the same whatever gas be used. All the carriers may not be reduced to their lowest dimensions; some may be aggregates of two or more corpuscles; these would be differently deflected from the single corpuscle, thus we should get the magnetic spectrum.

I have endeavoured by the following method to get a measurement of the ratio of the mass of these corpuscles to the charge carried by them. A double cylinder with slits in it, such as that used in a former experiment, was placed in front of a cathode which was curved so as to focus to some extent the cathode rays on the slit; behind the slit, in the inner cylinder, a thermal junction was placed which covered the opening so that all the rays which entered the slit struck against the junction, the junction got heated, and knowing the thermal capacity of the junction, we could get the mechanical equivalent of the heat communicated to it. The deflection of the electrometer gave the charge which entered the cylinder.

Thus, if there are N particles entering the cylinder each with a charge e, and Q is the charge inside the cylinder,

$$N e = Q.$$

The kinetic energy of these

$$\tfrac{1}{2} N m v^2 = W$$

where W is the mechanical equivalent of the heat given to the thermal junction. By measuring the curvature of the rays for a magnetic field, we get

$$\frac{m}{e} v = I.$$

Thus

$$\frac{m}{e} = \tfrac{1}{2} \frac{Q I^2}{W}.$$

In an experiment made at a very low pressure, when the rays were kept on for about one second, the charge was sufficient to raise a capacity of 1·5 microfarads to a potential of 16 volts. Thus

$$Q = 2 \cdot 4 \times 10^{-6}.$$

The temperature of the thermo junction, whose thermal capacity was 0·005 was raised 3·3° C. by the impact of the rays, thus

$$\begin{aligned} W &= 3 \cdot 3 \times 0 \cdot 005 \times 4 \cdot 2 \times 10^7 \\ &= 6 \cdot 3 \times 10^5. \end{aligned}$$

The value of I was 280, thus

$$\frac{m}{e} = 1 \cdot 6 \times 10^{-7}.$$

This is very small compared with the value 10^{-4} for the ratio of the mass of an atom of hydrogen to the charge carried by it. If the result stood by itself we might think that it was probable that e was greater than the atomic charge of atom rather than that m was less than the mass of a hydrogen atom. Taken, however, in conjunction with Lenard's results for the absorption of the cathode rays, these numbers seem to favour the hypothesis that the carriers of the charges are smaller than the atoms of hydrogen.

It is interesting to notice that the value of e/m, which we have found from the cathode rays, is of the same order as the value 10^{-7} deduced by Zeeman from his experiments on the effect of a magnetic field on the period of the sodium light.

[J. J. T.]

ANNUAL MEETING,

Saturday, May 1, 1897.

SIR JAMES CRICHTON-BROWNE, M.D. LL.D. F.R.S. Treasurer and Vice-President, in the Chair.

The Annual Report of the Committee of Visitors for the year 896, testifying to the continued prosperity and efficient management f the Institution, was read and adopted.

Fifty-eight new Members were elected in 1896.

Sixty-four Lectures and Nineteen Evening Discourses were livered in 1896.

The Books and Pamphlets presented in 1896 amounted to about 74 volumes, making, with 621 volumes (including Periodicals bound) urchased by the Managers, a total of 895 volumes added to the ibrary in the year.

Thanks were voted to the President, Treasurer, and the Honorary ecretary, to the Committees of Managers and Visitors, and to the rofessors, for their valuable services to the Institution during the ast year.

The following Gentlemen were unanimously elected as Officers r the ensuing year:

PRESIDENT—The Duke of Northumberland, K.G. D.C.L. LL.D.
TREASURER—Sir James Crichton-Browne, M.D. LL.D. F.R.S.
SECRETARY—Sir Frederick Bramwell, Bart. D.C.L. LL.D. F.R.S. M. Inst. C.E.

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ht Hon. Arthur James Balfour, M.P. LL.D. F.R.S.
olfe Barry, Esq. C.B. F.R.S. M. Inst. C.E.
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Frankland, Esq. D.C.L. LL.D. F.R.S.
Hawksley, Esq. M. Inst. C.E.
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Sir James Stirling, M.A. LL.D.
ry Thompson, F.R.C.S. F.R.A.S.

VISITORS.

Sir James Blyth, Bart.
William Arthur Brailey, M.D. M.R.C.S.
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John Ambrose Fleming, Esq. M.A. D.Sc. F.R.S.
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Thomas Tyrer, Esq. F.C.S. F.I.C.
Roger William Wallace, Esq. Q.C.
John Westlake, Esq. Q.C. LL.D.
His Honour Judge Frederick Meadows White, Q.C.
James Wimshurst, Esq.

GENERAL MONTHLY MEETING.

Monday, May 3, 1897.

SIR JAMES CRICHTON-BROWNE, M.D. LL.D. F.R.S. Treasurer and Vice-President, in the Chair.

Charles Elmer Southwell, Esq.
Mrs. Silvanus P. Thompson,

were elected Members of the Royal Institution.

The Right Hon. Lord Rayleigh was re-elected Professor of Natural Philosophy in the Royal Institution.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz. :—

FOR

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Zurich, Naturforschende Gesellschaft—Vierteljahrsschrift, Jahrg. XLI. Suppl. 8vo. 1896.
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WEEKLY EVENING MEETING,

Friday, May 7, 1897.

Sir James Crichton-Browne, M.D. LL.D. F.R.S. Treasurer and Vice-President, in the Chair.

Anthony Hope Hawkins, Esq.

Romance.

My object in the remarks which I am to have the honour of addressing to you to-night is to attempt to define in some degree the meaning and function of romance as a quality in literature; and although romance is to be found in many kinds of literature, I think you will not only forgive, but will also approve, if I discuss it from the point of view of the species on which alone even your indulgence could seem to give me any right to speak—that of prose fiction. As regards nomenclature, there is at the present time a tendency in some quarters to distinguish between *novels* and *romances*; but I think that the older and more authoritative usage in English is to employ *novel* as the generic, *romance* as the specific term. In this latter way I shall use the words to-night; and I shall ask, to put my questions broadly, What are the characteristics whose presence in a novel leads us to call that novel a romance; and what is the share of romance, as a quality, in the work that novels have to do? The terms which are popularly opposed to romance—realism and the realistic—I shall not deal with further than in so far as they may occur incidentally in the course of my proper inquiry. It may be doubted whether the antithesis, admittedly rough and ready, is not in fact so partial and so clumsy as to be devoid of any merit as a guide in thinking, though it may by familiarity have acquired some convenience as a catchword. Speaking in a place mainly devoted to the study of exact sciences, I would add that I must beg for some allowance if, in treating of a subject of an inexact nature, and of an art not very amenable to strict rules, my conclusions are affected by a certain degree of vagueness and tentativeness. The true meaning which underlies ordinary phraseology is not always easy to discover, and rigid dogmatism of statement would befit neither the topic nor the speaker. At the same time I may here and there, owing to a desire for brevity, seem to assert, where my real intent is only to suggest matter for your consideration.

Romance, then, being a certain quality in literature, and literature being (so far, anyhow, as novels represent it) a picture of some side or aspect of life—for these two preliminary steps in the argument it seems safe to assume—the presence or absence of romance must be

due either to the choice of the aspect, or to its treatment, or to a combination of these two. Now every novel which (if I may use the phrase) knows its own mind, may be analysed into, first, the theme, and secondly, the things which exist for the sake of the theme—the auxiliaries; that is to say, into the thing which it was the writer's end and object to exhibit, and the various means and devices by which he endeavours to make the exhibition of it as clear, as complete, and as striking as possible. For the essential character of the book we must look not at the auxiliaries but at the theme; indeed it is not a rare case that much of the auxiliaries should be in violent contrast with the theme, seeking that means of heightening the theme's effect. We should go very wrong, then, if we judged the character of the book from them: it is always the theme which decides that. To put it briefly, the auxiliaries subserve the theme, the theme classes the book.

Again, the theme is not concerned with incidents as such. I need not approach the borders of metaphysics and ask whether there is any such thing as an incident as such, or could be; I am happily at liberty to waive that question, and to content myself with observing that at any rate incidents as such—incidents not in relation to a mind perceptive of them, I mean—are not the subject of novels. The theme deals with people passing through incidents, and shows how they are affected thereby: their thoughts, feelings, emotions, and volitions. The incidents are means, not ends, and, to use the common metaphor, just as truly a background to the picture as any particular locality or any historical period which the writer may select for the staging of his story. The truth of this, if not self-evident, yet becomes immediately apparent when we observe that we can go a very long way towards changing incidents, or even towards dispensing entirely with external incidents, without affecting the identity of the theme; but we can take hardly a single step in the direction of changing the character of the people with whom the theme is concerned: it becomes plain at once that a pursuit of that path will end by depriving us altogether of what we set out to tell, and leaving us either with no story at all or with a very different one. Novels, then, are not about things or incidents, but about people. It may be objected that they are also, in some cases, about non-human animals. Yes, but only when such animals are treated as people—that is to say, with an artificiality which the writer's talent makes us accept in spite of a more or less obstinate sense of ultimate falsity.

It follows that the quality which is the subject of my inquiry, since it is to be found in the theme, must be found in the people and not in the incidents. Here common ways of speaking and thinking seem to be to some extent against us. When the ordinary man—when anybody who is not at the moment trying or caring to think exactly—speaks of a romance, no doubt he most often has external incidents in his mind; he thinks of fighting perhaps,

> "the lance points slantingly—
> Athwart the morning air."

Or perhaps he has in his mind murders and dark intrigues. None the less he does not mean the same thing when he says "A Romance" as he does when he says "A Detective Story." Nor does he really mean to assert the necessary introduction of improbability of incident, or of "sensations," or of strange scenes or strange places—though he would say that all these things were certainly often present in romances, and we should be obliged to admit the justice of his remark. Or perhaps he would maintain that a plentiful supply of love making is the hall-mark of the romance; and again we should agree that love-making is very common and is apt to be a predominant subject in romance. But he would admit on reflection that there might be a romance of ambition, or of religious emotion, or of devotion to truth, or of the love of humanity.. His mistake, in fact, would seem to be the very ordinary one of taking separable, though frequent, accidents for the essence. And it is worth noticing that the common speech is sometimes more nearly right. If I say of a man, "He hasn't a bit of romance in him," I do not mean that nothing happens to him—the Tower of Siloam would fall on romantic and unromantic alike. Nor do I mean that he never makes love. He may make it very often. I am characterising the quality of the man's mind, not his fortunes or his doings. We shall see later on, perhaps, how the venial error of everyday speech finds its excuse.

The theme in which we are to discover the romance is concerned, then, not with things or with incidents but with people. But it is concerned only with parts of people. Sometimes we read of a book, "It shows us the whole man," and the remark is meant as praise. But it is not to be read literally, or it is not praise. You must add to it, "so far as relevant to the theme." No book should, or perhaps could, show the whole man any more than it should show his whole life. This is familiar ground, and I need not labour it. A book shows more or less of a man, first, in relation to a similar more or less of other people, and secondly, as acted on by the chosen incidents, not by all that happens to him, for the greater part of that either has no material influence at all, or such a common and obvious one that the experience of the reader may safely be left to presuppose it. Certain feelings of a man or several men are the theme of a novel, and are therefore the place in which romance is to be found or the absence of it to be noted.

But does romance lie in the choice of these feelings or in the treatment of them? The question cannot be answered quite simply. Not in the choice in one sense, for probably any *sort* of emotion might be selected, nor merely in the treatment, for there must be a material of the appropriate nature. Miserliness does not sound like a good subject for romance, yet there might be a romance of miserliness; but it would have to be miserliness *in excelsis*, and unless it were, no skill of treatment would make a romance out of the theme. We must answer, I think, that the basis of romance is to be found in the choice

of a special case of some emotion, and in imparting to it certain special qualities by means of treatment.

And first in romance, the emotion is taken at a high pitch. It is *strong and strongly felt*; it is one of the salient features of the man's character, one of the determining influences of his life. Almost of necessity it follows that it is imaginative in character; that it does not acquiesce in limitations which to another mind might seem insuperable; that it sees a way for itself, and foresees its satisfaction with a clearness which gives to it perseverance and resolution. It may be noble, but will not be too meek; it may be wicked, but it must not be petty; it may be in fact, temporary, but no decay is visible in it as yet. This strength of emotion seems to me the first characteristic of romance. But by itself it is insufficient for our purpose. It must be taken in conjunction with the second.

All literature demands abstraction, just as any other inquiry does. In romance abstraction is carried further than in writings where this quality is not. Not merely is the vain attempt to show the whole man and his whole life abandoned, but attention is directed in a special degree to the one great emotion—or perhaps to two or three great and conflicting emotions, whether all in the mind of one person or assigned to the leading actors in the story. The small emotions drop out or are minimised; the infinite complication of motives is avoided. This high degree of abstraction results in giving to the chosen emotion a character of *simplicity*; it is cleared from the intrusion of rivals; it is exhibited in possession of the field; it is disentangled from the affairs of life; or if the theme be a battle between two great enemies, then the arena is cleared for their struggle, and the small fry are kept out.

We may add another quality, which is really a resultant of this union of strength and simplicity. The emotions of romance are *confident*. As their strength causes them to make little of external hindrances, as their simplicity frees them from being lost in the entanglements of circumstances, so their confidence makes them not self-questioning but self-asserting. They do not doubt themselves, or impute unreality to themselves, or ask whether they are worth having in the end, or whether the objects to which they are directed are worth the trouble of winning. They are sure of themselves, ready to give an account of themselves, finding in themselves their own justification.

In these three qualities which I have tried to indicate are to be found, I think, the leading characteristics of the emotions as they are selected for and treated in writings of a romantic character. Anything so definite as a definition is perhaps rather repugnant to the subject, and certainly is, as it always is, dangerous to the speaker. In literary matters to make a definition is—if you will allow me a professional comparison—hardly less rash than to write a sequel; both acts cause the critical eye to glance towards the critical tomahawk. But I think we shall not be very far wrong if at this stage we venture to

say that the aim of romance is to exhibit in action a strong, simple, confident emotion, either in exclusive domination, or in conflict with and ultimately triumphing over one or more emotions possessing the same qualities, but proving in the end either less persistent or less fortunate. No particular class of incidents is essential, no special scenes, no special surroundings. Neither is any particular sort of emotion essential: to take our old illustration, a sublime miserliness might struggle with a keen parental affection, and a good romance describe the conflict. But whatever the incidents, the scene or the emotion, the qualities will remain. Some strong, simple and confident emotion will dominate the persons, shape the events, and determine the character of the story. The task of incidents and scene is simply to afford a stage and to enhance the effectiveness of the drama.

Let me illustrate what I mean by a glance at one or two sorts of novels which are not romances. Remember, I am not saying that they are not—or may not be—good novels, only that they have not the marks of romance. I will take the emotion of Love—Love between man and woman. This is treated in novels of all sorts, and in many forms of literature besides; that is due to its universality, to the fact that it appeals to most writers and the certainty that it will appeal to most readers. But it is a favourite of romance not only for its universality, but even more because it lends itself most readily to the characteristically romantic treatment. Above all other emotions it is strong and resents control, it is simple and rises above circumstances, it is confident and self approved. But every novel which deals with love is not romance. For example, there is a large class of novels which give pictures of the life that is about us every day, and in which love plays a part, perhaps, so far as the incidents go, a leading part. But the love is not a subject, it is rather a *datum*, it happens, it is not felt; it occurs at a certain point because it is the proper thing to occur, the natural feature of the young man's twenty-fifth and the young lady's twentieth year, the suitable winding up of the series of social sketches of which the novel consists, the suitable recognition of what our national customs in regard to matrimony happen to be. All this is not of necessity untrue to life, nor of necessity uninteresting or unamusing or uninforming; it may be almost anything in the world except romance. We are told indeed that Mr. A. and Miss B. are in love. Even so did Stage Managers in old times stick up a board and write on it "This is Verona." Well, we take your word for it, but otherwise it might as well have been the Arctic regions. In this sort of book love is merely a premiss from which we draw the conclusion—marriage—but what the emotion of love itself is remains undiscussed, undescribed, to all appearance uncomprehended. And it may be noticed that not a few of the novels which have love for their theme, and are generally called, and perhaps call themselves, romances, fail in this respect. The love-making is itself mechanical; it does not rule the book, and we are forced to suspect the writer either of failing to understand his theme,

or of having confused his theme and his auxiliaries to such a point that the passion which it is the real work of the book to exhibit becomes no more than a subordinate and sometimes a tedious incident in it. Why are these books not romances? It is because the strength of the emotion is not realised or exhibited, there is no power, no imagination. If any such love-affair, or rather marriage-arrangement, as I have indicated, is to be found in a true romance of which love is the theme, it is there, not for its own sake, but as an auxiliary, useful by way of contrast, by its tameness heightening the effect of the great emotion whose exhibition is the real purpose of the book.

Take another class of novels. I am in a difficulty about naming it. If I say analytical, I confuse manner and matter; if I say realistic, neither you nor I will be sure what I mean, and I shall probably give a wrong impression. Perhaps I may take refuge in the semi-slang phrase which came into vogue a little while ago, and speak of the "problem novel." Problem novels are not romance; the reason is not the same as in the previous case; there may be strength enough and to spare in the emotions described. Nor is it because the emotion is sometimes, as we say, illicit, being in conflict with law, or morality, or convention; there is in that nothing in the smallest degree inconsistent with romance—rather does romance find some of its finest opportunities in situations so created. From the point of view of romance, the fault here is the absence of simplicity and the resulting want of confidence. The emotion is encumbered and complicated; it is surrounded by rivals; it is tortured by problems social and ethical; it is mixed up with and obscured by questions of the relative duties, the relative rights, the relative standards of men and women. Interesting as all these questions are, they are not in the way of romance. Or, again, the emotion is sapped from within; it is hesitating, fearful, doubtful; it asks whether it really exists, or, if it exists, whether it isn't something else than it seems to be, or if it really exists and really is what it seems to be, then whether it has any business to exist, or at any rate to be what it is; or again, it does not know what it wants, much less whether, if it wants it, it ought to want it, and so on. There is no simplicity, no confidence; in their place we find complexity and self-distrust.

But of course it is not always so easy to draw the line, and even though we assume every confidence in the formula we have adopted, we should still be puzzled from time to time how we ought to class a novel. We should not hesitate to call the 'Vicar of Wakefield' a romance, a true case of romance, notwithstanding its everyday characters and scenes. But take the great novels of manners—'Tom Jones,' or 'Vanity Fair,' or 'Pendennis.' In the broad sweep of books like these there will be found matter of a romantic character, and we are tempted to the easy course of some such division as one of pure romances and mixed romances. But I fear that to adopt such a distinction would be rather a concession to mental indolence than an obedience to the truth of the argument. We must ask again, What is

the theme? and by that, when we have discovered it, we may judge. We shall find, I think, that books like these are not romances, because the romance that is in them is subordinate and subsidiary. Take either 'Tom Jones' or 'Pendennis,' and the theme seems to be (I need not say that I speak with diffidence) something more varied and something more complicated than romance deals with. We have the picture of a young man, not only passing through a great variety of incidents, but himself very variously, and often very temporarily, affected by them. If you judge chapter by chapter you may say here and there, "This is romance"; but if you take the book as a whole you will say, "No, there is not here the abstraction, the simplicity, the concentration on two or three great emotions." There is abstraction, of course, but not in the high degree characteristic of romance; nor, again, has any one or any two emotions the pride of place which romance assigns to them. You can hardly tie the writer down to any narrower theme than "The Way of the World." The reason does not lie in the number of characters or of incidents, although this is a probable accompaniment of themes of such a nature. Take a novel, or a series of novels, no less expansive in treatment, no less crowded with incidents and characters—the story of D'Artagnan and the Musketeers We say at once, "Here is romance." Why? As it seems to me, because, in spite of all complexity, in spite of all deviations, in spite of the elaborate and minute tracing out of purely subsidiary incidents, you have running through the whole book, inspiring it all and exhibited in it all, one strong, simple, imperious passion or emotion, which rules the lives of the leading characters and above all of the great hero. Dumas' trilogy of the Musketeers is a romance of the joy of action—of doing, of using hand and brain. These men do not much mind what they are at, but they must be at something, and this great desire of theirs despotically overrides every other emotion and every consideration that endeavours to oppose it. They cannot keep still; they are in love with living. This temper of theirs—again, above all, of D'Artagnan's—shapes and inspires the whole book, so that kings and queens and cardinals, wars and plots and amours, exist only as the stage on which it may exhibit itself, and as the material from which it may satisfy its monstrous appetite for joyful activity. I do not say that there is nothing of this temper in 'Tom Jones,' or even in 'Pendennis,' but it does not set the tone of the book; it is not unimpeded, it is no more than an element. Would it be possible to say, in a rough attempt at a summary, that the great Englishmen use their heroes to illustrate the world, but that the great Frenchman uses the world to satisfy and glorify his hero?

But all writers of romance are not such as the creator of D'Artagnan—I mean, of course, of D'Artagnan as we find him in the novels. They cannot wring simplicity out of an almost limitless complication of persons and incidents; they cannot follow the thread through so enormous and infinitely winding a maze. The result is one which was foreshadowed by the fact that the ordinary man—ourselves at

han others, yet at all events more easily and more obviously. y tend to suggest themselves to the writer of romance; he line of least resistance along which his mind travels; him at once as supplying the most effective stage for his emotion. Suppose, once more, that the passion of love is 's theme. It is to be strong, persistent, not to be turned e readiest way to display these qualities is to confront it obstacles, to demand of it great sacrifices and efforts, to an who feels it with the peril of death. There may be s great as that of life, or greater; but life is very obviously at sacrifice, and appeals as such to everybody, even to those miss the poignancy of some not less great but less obvious f-devotion. Again, a mark of love is that it takes joy in e object of love, and perhaps we may add, takes an especial he applause of the object of love. How better show this ove, and thereby reinforce the impression of the love's han by causing the lover to preserve his mistress who in as come into great distress? We see at once how fighting, , and all sorts of adventures, come to be so common in s to have been mistaken for the essence of that of which they ccidental concomitants, and to seem to be the theme where nly particularly handy and convenient auxiliaries; for you erse the parts and make the theme patriotism or courage, as an auxiliary; the same incidents would serve, only you e, so to say, to shift the centre of gravity; or you might uggle between the two, using still the same framework of

from the point of view of the simplicity and confidence of n, it is naturally felt that these qualities are most readily in hours of action, and are at their prime in moments of itement, such as arise in view of imminent danger or of

phrase, to take them in the spirit in which they are meant. Their remoteness from his everyday experience clears from his mind the everyday atmosphere in which he lives, and persuades him into an acquiescence in the justice of the picture; he knows that, as a general rule, he does not feel his emotions in just this form, but the novelty and stirring nature of the incidents easily convince him that, placed as the hero was, he would feel as the hero felt. In this way, then, what are generally called romantic incidents and romantic surroundings are of real assistance to romance in the proper sense; they both aid in the exhibition of the matter of the theme, and dispose the reader to accept, approve and endorse it; they harmonise with the high pitch of the emotions shown in action, and afford a fit setting for them. But it must be repeated that they are only one of many settings, not better than others, but only more obvious, more ready, and in fact more easy to handle. The writer propo es to himself a less difficult task than that which he would attempt if he dispensed with these auxiliaries. Very much the same considerations are applicable to what are called historical romances. Here again the strangeness of scene, the remoteness from common experience, and the sense that everyday criteria cannot be applied, help the reader to put himself at the standpoint of the characters, and thus materially assist the writer in his task. There is, in a word, less chance of the reader saying, "I shouldn't feel like that, or act like that, and no more would he."

I have approached the borders of a question which I must not wholly avoid. The romancer is often accused of dwelling in and of inviting his readers to join him in an entirely artificial world, corresponding to nothing *in rerum natura*, and of shirking that grappling with the facts of life in which novelists of another school find their hardest task and their highest glory. This charge of unreality is one which romance must not shirk, but must face and analyse. I believe myself that the accusation owes its origin in a great degree to the same confusion of thought which has been already noted—to the idea that the essence of romance is to be found in the incidents, rather than in the emotions. For the emotions surely are not unreal; they are deep, fundamental, universal in human nature. But although we must sturdily assert their reality, we may, without shame and without hesitation, admit their rarity in the precise form in which romance presents them. The "simple case" is, I take it, always rare in nature; it has to be extracted; it is attained as the result of a very high degree of abstraction. So it is in literature; and if all that is charged against the characteristic themes of romance is that they are not often to be seen in undisturbed operation in life as we live it, the charge may be confessed. But rarity is not falsity; and not to happen very often, if it be a fault, is a fault which affects many of the most important events in the world's history. Abstraction is not the falsification of facts ordinarily apparent, but rather the means of exhibiting truths ordinarily hidden—overlaid, as it were—by the multitude of circumstances

and the complications of common feelings. Romance does not claim to reflect all life, but certain aspects of life to which it gives prominence. These are not the aspects with which the physician or the statistician, or even the logician, is primarily concerned, but they are true and important aspects. Romance comes to be false only when it allows itself to forget its own true nature and its own true function. But for every form of literature the same penalty waits on the same sin. What is called the realistic novel becomes false when through an intemperate adoration of mere fact it forgets that its business is with the minds of men, and that, given a certain number of characters in the story, that only is essential which in some way acts on the minds of those characters, and is, so to say, a *differentia* of them as compared with the rest of the world; what they have for breakfast is of no matter unless it should give them indigestion, and indigestion should produce irritation or otherwise affect the course of their thoughts and emotions. In like manner romance becomes false when it forgets what its true theme is, lets itself be carried away by the incidents, thinks only of them, and instead of representing people influencing and being influenced by events, gives us a series of mechanical stage effects happening to a number of no less mechanical stage puppets. This sin is indeed common; perhaps no writer could show quite a clean sheet in regard to it. But no cleverness, no inventiveness, no accomplishment in mere technique, compensate for an error so fatal—just as no minuteness of observation or diligence in collecting what are called "documents," compensates for the corresponding sin of the writer whose watchword is reality. In both sorts of books the thing in the end is—the one thing in the end is, *the temper of the characters.* To that we come back with a persistence only to be excused because here lies the foundation of the whole matter. In romance the thing is always the love of the woman, not the machinations of the villain —the high mind of ambition, not the means it seeks or the prize it aims at—the spirit of adventure, not the adventures—the joy in action, not the precise actions by which the impulse seeks and finds satisfaction. I have a notion that if we could know the order in which the writer evolved his book, whether the man came first or the incidents, whether he fitted his scene to his characters or contrived characters to put on his scene, we should in most cases be able to say whether his book would be a good book or not a good book in the most essential point. When a lady said to Sir Walter Scott that she never knew what was going to happen on the next page of his books, Sir Walter is reported to have replied, "Nor I neither, madam." The story may well embody a truth; he may very likely not have known what was going to happen *to* his characters, but depend upon it Sir Walter knew very well what was happening and what was about to happen *in* them; he knew where he was going, though he might not have decided exactly what road to take.

Perceiving this radical fact, we find all contradiction between romance and the life we call real to vanish, and we must confess that the fault has been in our own ideas and not in the subject with which we are concerned. Romance becomes an expression of what are perhaps the most important, the most far-reaching, the most deeply seated instincts and impulses of humanity. It has no monopoly of this expression, but it is its privilege to render it in a singularly clear, distinct, and pure form; it can give to love an ideal object, to ambition a boundless field, to courage a high occasion; and these great emotions, revelling in their freedom, exhibit themselves in their glory. Thus in its most worthy forms, in the hands of its masters, it can not only delight men, but can touch them to the very heart. It shows them what they would be if they could, if time and fate and circumstances did not bind, what in a sense they all are, and what their acts would show them to be if an opportunity offered. So they dream and are the happier, and at least none the worse, for their dreams. It is the gift of the Romancer, in the measure of his ability, to see and reveal truths of the heart, and for a time to loose the fetters that a man's own lot rivets on him, to bid men forget what is round them, but not of them, about them, but not themselves. We say that a man "forgets himself" in an exciting romance. We mean, as we sometimes do in speaking, just the opposite of what we say. A man does not read a good romance to forget himself, but to forget what is not himself; and because he finds there something that recalls the self which the changes and chances and troubles of the world have almost made him forget, he is well pleased.

There are two points on which I wish to guard myself before I sit down, if your patience will kindly allow me. The first has reference to what I have said about the relative position of incidents and emotions. I must not be understood to mean anything in the least like what is sometimes said, half-seriously, half-jokingly—that "the plot doesn't matter." In my judgment the plot matters so much as to be the surest mark of the writer's ability, and incomparably the chief criterion of the merit of the book. But the word "plot" must be understood in its proper sense, in the sense that makes it the very core and kernel of the book, the story, the thing the writer tells the reader. Every novel consists of emotions and incidents; this is the rudimentary analysis of it in respect of matter, just as the division into theme and auxiliaries is the rudimentary analysis of it in respect of form (I am not, of course, insisting on my own precise terms, but on the obvious distinctions which I use them to express). The plot is not emotions, for emotions idle, in a vacuum, so to speak, will yield no story; neither is it incidents, for as we saw at the beginning, naked incidents, incidents without people and without emotions, will yield no story. The plot of a romance is emotions and incidents—emotions in action—and the merit of the plot lies first in choosing emotions of true romantic quality, and secondly in fitting those emotions with the most appropriate actions—those which will best exhibit the

emotions and most attract the reader to the engrossed study of them. It is almost impossible to say, and certainly not very useful to spend time in inquiring, whether the first task or the second is the more difficult: the successful accomplishment of both is necessary to the writing of a good romance, and the product which results from bringing the emotions into contact with the incidents is the plot. This product may or may not be in complete existence when the writer begins the story; it must be complete by the time he ends it. I do not mean that every incident which may be related in a novel is part of the plot, or every emotion which may be described either. We may revert to the formal division of theme and auxiliaries, and although it may not be practicable to draw a very definite line between what belongs to the plot and what does not in all cases, we may say that the plot lies in the theme and such of the auxiliaries as afford the most immediate and essential vehicle for the expression of the theme. Beyond these limits there may lie both many emotions and many incidents, all of which should no doubt, if we are to follow a rigid rule, have their particular service to perform in relation to the plot, but as to which in the practice of critics considerable latitude is allowed, provided that they are in themselves of an entertaining description, or contain true and life-like sketches of human nature. No man is denied a few digressions if he will make good use of the indulgence.

The second point is this. I may seem to have drifted into a eulogy where I meant only to render justice, and to have claimed for romantic novels a pre-eminence over other kinds. To make any such pretentions on their behalf is not my purpose, and would by no means represent my own opinion. The power and province of romance are limited; it cannot annex and does not seek to encroach upon sister-kingdoms. Concerned itself with strong and simple emotions, it is addressed to emotions of a similar nature; it is primarily an appeal to feeling, and to feeling of a direct, normal and straightforward description. It is not armed with the keenest weapons of analysis; it is not skilled to trace minute variations or to catch flitting shades; it is not at home with struggles and stirrings that find no outlet in action, are invisible to the world, and barely conscious in the heart which is their home; it prefers an environment where a man's individuality can have play, and has no pleasure in the sombre picture of a tyranny of circumstances that crushes the actor into a mere sufferer; its purpose is not to arraign the equity of institutions or to read the riddles of life. These subtle investigations, so attractive in their difficulty, so delicate and patient in their methods, with their results so fascinating to the alert intellect and the curious mind, it must leave to writings of another temper. Nor, again, is it the way of romance to bid you stand by, an amused spectator, while it exhibits to you scenes from the world's comedy, and bids you laugh at follies of which you are not guilty, or at passions from which you smilingly thank heaven you are free—or wonder you are not; it is not disinterested enough for that, and must have you

share the emotions which it displays before your eyes. It will make terms with humour, but it does not love ridicule. In spite of the deep truths with which romance deals, the romantic temper is, in a sense, innocent, unsophisticated, primitive; it throws itself into life rather than analyses it; it sympathises and shares, it does not stand aloof and smile. Intricacy baffles it; it retreats in fear from the bite of the acid of irony. It is conversant with great sorrows, yet in the end it is a cheerful thing. It trusts life, it loves life; even for its deepest woes there are the consolations of love or the hallowing pride of memory—for when romance kills, she kills becomingly. It does not ask whence we come and whither we go, it does not cry, "Vanity of Vanities!" But a temper like this, while it has its virtues, and possesses about it much that is attractive, has its obvious limitations and is subject to great disabilities. It is not a full expression of the human mind; it is not final, exhaustive, nor perhaps even particularly helpful in regard to the great problems which occupy the intellect; there are large fields of emotion which it leaves untouched, complications that it does not unravel, varieties that it cannot note, moods with which it cannot enter into sympathy and which it seems rather to delude than satisfy. So sometimes men and women turn away from it in a sort of impatience, and they are especially apt to do this when they are members of a society which is highly civilised, highly cultivated, and much interested in the puzzles and difficulties that beset the life of the community and the individual—a society that takes a critical and perhaps not a very hopeful view of itself, that has its intellect fully developed, its conscience very acute, and (perhaps I may add) its nervous system in a state of some irritation. Romance seems then rather a childish thing—yes, like a child laughing in the garden while a man lies dead in the house. Even if it were no more, yet let the child laugh: his laughter is a part of the truth about the world. But, as a matter of fact, this impatience may be understood and excused as a mood, but is not to be justified as a criticism; and those who are guilty of it fail in catholicity of judgment. Because romance cannot fill the place and discharge the function of other writings inspired by different tempers and employing different means, they are hasty to deny the value of its proper office and the importance of the position it holds as one of the many forms which must be assumed by that interpretation of human life which is the great occupation of all imaginative literature, and the title by which it commands the attention of human minds. They are all at the task—the careful chronicler, the keen analyst, the patient student, the smiling comedian, the indignant satirist, the theoriser, the visionary, and the wit. It is enough for the romancer to claim and take his place in the rank, being sure that, if he pursues his own task faithfully and performs it with ability, there are many who will find in him not the worst companion, and few to whom he will not (at some moments at least) seem to speak words both of gladness and of truth. For romance is, in the end, an assertion, constantly and confidently re-

peated, that, resistless as may seem the stream of tendencies, hard as the fetters of fate, tyrannous as the order of society, of nature, or even of the universe, yet there is still in men themselves an exuberant something which lives, and works, and does, and makes. Thus, after all acknowledgment made of its limitations, with the amplest recognition of the value and necessity to literature of other methods and other points of view, it remains a fine expression of the vitality of the human race, of the love of life and the fruitful joy in it, of the excellent vigour of the spirit of man.

[A. H. H.]

WEEKLY EVENING MEETING,

Friday, May 14, 1897.

William Crookes, Esq. F.R.S. Vice-President, in the Chair.

Professor Harold Dixon, M.A. F.R.S.

Explosion-Flames.

The lecturer gave a brief history of the researches made on the temperatures and pressures produced in explosion-flames, and exhibited photographs of various explosion-flames taken on a very rapidly moving film. The photographs showed the movements of the flame from the ignition point, and the effect of sound-waves reflected from the ends of the explosion-tube.

WEEKLY EVENING MEETING,

Friday May 28, 1897.

Ludwig Mond, Esq. Ph.D. F.R.S. Vice-President,
in the Chair.

Professeur Henri Moissan, Membre de l'Académie des Sciences, Paris.

Le Fluor.

Milords, Mesdames et Messieurs,—J'ai été heureux de répondre à votre appel, et je tiens tout d'abord à vous remercier de l'honneur que vous avez bien voulu me faire en me demandant cette conférence.

On connaissait depuis longtemps un minéral curieux auquel on a donné le nom de fluorine et que l'on rencontre dans la nature en gros cristaux cubiques, incolores ou teintés de vert ou de violet. Cette fluorine est un composé binaire formé d'un métal, le calcium uni à un autre corps simple qu'il avait été impossible d'isoler jusqu'ici et auquel on a donné le nom de fluor.

Ce fluorure de calcium a été comparé bien souvent au chlorure de sodium dont les chimistes connaissent parfaitement la composition. En effet, entre les fluorures et les chlorures, il y a de grandes et profondes analogies : le chlorure et le fluorure de potassium cristallisent tous deux dans le système cubique. Les propriétés principales des chlorures sont semblables à celles des fluorures. Ils fournissent le plus souvent des réactions parallèles ; traités par l'acide sulfurique ils produisent les uns et les autres des acides hydrogénés solubles dans l'eau et donnant à l'air d'abondantes fumées.

Outre le fluorure de calcium, on trouve encore, dans la nature, d'autres composés renfermant du fluor. On connait, par exemple, une combinaison complexe de phosphate de chaux et de fluorure de calcium à laquelle on a donné le nom d'apatite.

Ce minerai, qui se présente parfois en très jolis cristaux, a pu être obtenu synthétiquement dans les laboratoires, mais ce qui est plus important, Henri Sainte-Claire Deville a pu préparer une apatite chlorée, et ce nouveau composé se présente en cristaux identiques à ceux de l'apatite fluorée. On est donc en droit de dire que, dans ces combinaisons le chlore peut remplacer le fluor, s'y substituer. C'est là une analogie remarquable, un lien qui réunissait le chlore bien étudié, bien connu, à ce corps simple, non encore isolé, le fluor.

Ai-je besoin de vous citre d'autres exemples ? Ils ne nous man-

eront pas. On connaît la wagnérite, fluorée naturelle ; on peut parer le composé similaire chloré.

Ces analogies du chlore et du fluor se poursuivent plus loin.

Traitons du sel marin, du chlorure de sodium, par de l'acide sulfique. Vous voyez qu'il se produit aussitôt un abondant dégagement d'acide chlorhydrique gazeux.

Faisons de même pour le fluorure de sodium. Ajoutons dans un e de plomb de l'acide sulfurique à un fluorure alcalin. Nous rons des fumées intenses se produire. Dans l'un et l'autre cas, s aurons dégagé un corps gazeux à une température de + 20° ntigrade), fumant abondamment à l'air, incolore, possédant les actères d'un acide énergique, s'unissant à l'état anhydre avec mmoniaque, très soluble dans l'eau et s'y combinant avec une nde élévation de température.

Si nous donnons au fluorure de sodium, au composé binaire du r et du sodium, la formule NaFl, celle du corps acide produit par tion de l'acide sulfurique ne peut être que HFl. Les deux ctions sont identiques.

Le corps gazeux, acide, produit dans cette réaction, est donc une nbinaison de fluor et d'hydrogène, un corps analogue à l'acide orhydrique auquel nous donnerons le nom d'acide fluorhydrique.

Mais, dans les sciences naturelles l'analogie ne suffit pas ; la thode scientifique ne peut admettre que ce qui est rigoureusement nontré. Il fallait donc tout d'abord prouver que l'acide fluorhyque était un acide hydrogéné. Et ceci, messieurs, va nous reporter commencement de ce siècle. Vous savez combien fut grande l'innce de Lavoisier sur l'essor de la chimie en tant que science vérile. Vous savez combien ce grand esprit, par l'emploi continu de alance dans les réactions, fournit à la science que nous étudions une ueur mathématique. Frappé du rôle important de l'oxygène dans ombustion, il crut que cet élément était indispensable à la forma- des acides. Pour Lavoisier, tout acide était un corps oxygéné ; ide chlorhydrique fut donc, d'après les théories de Lavoisier, conéré comme renfermant de l'oxygène, et il en fut de même, par logie, pour l'acide fluorhydrique.

C'est à votre grande savant Humphry Davy que revient l'honneur oir démontré que l'acide fluorhydrique ne renfermait pas d'oxye. Mais permettez-moi, avant d'arriver aux belles recherches de vy, de vous rappeler l'historique de la découverte de l'acide fluorrique. Nous ne nous arrêterons pas aux recherches de Margraff ce sujet, publiées en 1768, mais nous n'oublierons pas que ce fut eele qui caractérisa l'acide fluorhydrique en 1771, sans arriver tefois à l'obtenir à l'état de pureté. En 1809, Gay-Lussac et énard reprirent l'étude de cette préparation et arrivèrent à prore un acide assez pur, très concentré, mais qui était loin d'être ydre. L'action de l'acide fluorhydrique sur la silice et les silicates alors parfaitement élucidée.

Reportons-nous maintenant vers l'année 1813, époque où Davy

reprend l'étude de l'acide fluorhydrique. Peu de temps auparavant, Ampère, dans deux lettres adressées à Humphry Davy, avait émis cette opinion que l'acide fluorhydrique pouvait être considéré comme formé par la combinaison de l'hydrogène avec un corps simple encore inconnu, le fluor, en un mot que c'était un acide non oxygéné.

Davy, qui partageait cette idée, chercha donc tout d'abord à démontrer que l'acide fluorhydrique ne renferme pas d'oxygène. Pour cela, il neutralise l'acide fluorhydrique par de l'ammoniaque et, en chauffant fortement ce sel dans un appareil en platine, il ne recueille dans la partie froide que le fluorhydrate d'ammoniaque sublimé sans aucune trace d'eau.

Répétons la même expérience, mais avec un acide oxygéné; prenons de l'acide sulfurique que nous neutraliserons par de l'ammoniaque; nous obtenons ainsi du sulfate d'ammoniaque. Si nous chauffons alors ce sel dans le même appareil en platine, il fond vers 140°, puis vers 180° il se décompose en ammoniaque et en bisulfate, enfin ce dernier sel se transforme par une nouvelle élévation de température en bisulfite d'ammoniaque volatil, en azote et en eau.

Ainsi, en chauffant fortement le sulfate d'ammoniaque, il y a eu formation d'eau. Et dans cette expérience de Davy, lorsque l'on se trouve en présence d'un acide oxygéné, la quantité d'eau recueillie est assez grande pour être admise d'une façon indiscutable. Le fluorhydrate d'ammoniaque, de même que le chlorhydrate, ne fournissant pas d'eau par sa décomposition, on était donc conduit à dire que l'acide fluorhydrique ne renfermait pas d'oxygène et qu'il était analogue à l'acide chlorhydrique. Or, on sait par démonstration expérimentale que l'acide chlorhydrique est formé de chlore et d'hydrogène; il est donc logique de penser que l'acide fluorhydrique est produit par la combinaison de l'hydrogène avec le fluor.

Cette expérience importante, faite par des mains exercées, ne parvint cependant pas à faire admettre d'une façon générale, l'existence des hydracides.

Les idées de Lavoisier sur le rôle de l'oxygène dans la formation des acides, idées qui avaient été combattues au début, étaient alors si bien admises que beaucoup d'esprits se refusaient à croire à l'existence d'acides hydrogénés. Ce ne fut qu'après les recherches mémorables de Gay-Lussac sur le cyanogène et sur l'acide cyanhydrique, qu'il fut démontré d'une façon indiscutable qu'il pouvait exister des acides énergiques ne renfermant pas trace d'oxygène.

D'ailleurs, quand nous avons à comparer les combinaisons acides formées par le chlore, par exemple, ou le soufre, avec l'hydrogène, nous avons là deux types de composés tout à fait différents.

Prenons un volume de chlore et un volume d'hydrogène; sous l'action de la lumière ou d'une étincelle d'induction, ils s'uniront pour former deux volumes de gaz acide chlorhydrique, composé ayant toutes les propriétés d'une acide très énergique.

Si nous combinons deux volumes d'hydrogène à un volume de vapeur de soufre, nous obtiendrons deux volumes de gaz hydrogène

sulfuré, possédant encore une réaction acide, il est vrai, mais incomparablement plus faible que celle de l'acide chlorhydrique.

Il est bien évident que, par ses réactions énergiques, par le dégagement de chaleur qu'il produit au contact de l'eau et des bases, l'acide fluorhydrique doit être comparé à l'acide chlorhydrique et non à l'acide sulfhydrique. Il se rapproche absolument de cet acide chlorhydrique formé d'un volume de chlore et d'un volume d'hydrogène unis sans condensation.

Permettez-moi maintenant de vous rappeler une expérience beaucoup plus récente de Gorre. Ce chimiste a chauffé du fluorure d'argent dans une atmosphère d'hydrogène. Il a vu, dans ces conditions, le volume gazeux doubler ; il semble donc bien que l'acide fluorhydrique soit formé d'un volume d'hydrogène uni à un volume de ce corps simple non encore isolé, le fluor. De plus, c'est bien ce même corps simple qui a quitté le fluorure d'argent pour s'unir à l'hydrogène et produire l'acide fluorhydrique dont nous venons de parler précédemment.

Ainsi, messieurs, sans préparer ce fluor, sans pouvoir le séparer des corps avec lesquels il est uni, la chimie était parvenue à étudier et à analyser un grand nombre de ses combinaisons. Le corps n'était pas isolé et cependant sa place était marquée dans nos classifications. Et c'est là ce qui nous démontre bien l'utilité d'une théorie scientifique : théorie qui sera regardée comme vraie pendant un certain temps, qui résumera les faits et permettra à l'esprit de nouvelles hypothèses, causes premières d'expériences, qui, peu à peu détruiront cette même théorie, pour la remplacer par une autre plus en harmonie avec les progrès de la science.

C'est ainsi que certaines propriétés du fluor étaient prévues avant même que son isolement ait été possible.

Voyons maintenant quels ont été les essais tentés, non seulement sur cet acide fluorhydrique, mais encore sur les fluorures, pour arriver à isoler le fluor.

Je vous parlais tout à l'heure des expériences de Davy, dans lesquelles il a démontré notamment que l'acide fluorhydrique ne renfermait pas d'oxygène. Outre ces expériences, Davy en a fait un grand nombre d'autres que je rappellerai en les résumant.

On peut d'une façon générale diviser les recherches entreprises sur le fluor en deux grandes classes :

1°. Expériences faites par voie électrolytique s'adressant soit à l'acide soit aux fluorures.

2°. Expériences faites par voie sèche. Dès le début de ces recherches, il était à prévoir que le fluor décomposerait l'eau quand on pourrait l'isoler ; par conséquent, toutes les tentatives qui ont été faites par la voie humide depuis les premiers travaux de Davy le furent sans aucune espèce de chance de succès.

Humphry Davy a fait beaucoup d'expériences électriques, et ces expériences il les a exécutées dans des appareils en platine ou en chlorure d'argent fondu et au moyen de la puissante pile de la Société royale.

Il a reconnu que l'acide fluorhydrique se décomposait tant qu'il contenait de l'eau et qu'ensuite le courant semblait passer avec beaucoup plus de difficulté. Il a essayé aussi de faire jaillir des étincelles dans l'acide concentré, et il a pu, dans quelques essais, obtenir par cette méthode une petite quantité de gaz. Mais l'acide, bien que refroidi, ne tardait pas à se réduire en vapeurs: le laboratoire devenait rapidement inhabitable. Davy fut même très malade pour s'être exposé à respirer les vapeurs d'acide fluorhydrique et il conseille aux chimistes de prendre de grandes précautions pour éviter l'action de cet acide sur la peau et sur les bronches. Vous savez, messieurs, que Gay-Lussac et Thénard avaient eu également beaucoup à souffrir de ces mêmes vapeurs acides.

Les autres expériences de Davy (je ne puis les citer toutes) ont été faites surtout en faisant réagir le chlore sur les fluorures. Elles présentaient des difficultés très grandes, car on ignorait à cette époque l'existence des fluorhydrates de fluorures et l'on ne savait point préparer la plupart des fluorures anhydres.

Ces recherches de Davy sont, comme on pouvait s'y attendre, de la plus haute importance, et une propriété remarquable du fluor a été mise en évidence par ce savant: dans les recherches où il avait été possible de produire une petite quantité de ce radical des fluorures, le platine ou l'or des vases dans lesquels se faisait la réaction était profondément attaqué. Il s'était formé dans ce cas des fluorures d'or ou de platine.

Davy a varié beaucoup les conditions de ces expériences. Il a répété l'action du chlore sur un fluorure métallique dans des vases de soufre, de charbon, d'or, de platine, etc.; il n'est jamais arrivé à un résultat satisfaisant.

Il est conduit ainsi à penser que le fluor possédera sans doute une activité chimique beaucoup plus grande que celle des composés connus.

Et en terminant son mémoire Humphry Davy indique que ces expériences pourraient peut-être réussir si elles étaient exécutées dans des vases en fluorine. Nous allons voir que cette idée va être reprise par différents expérimentateurs. La lecture du travail de Davy vous intéresse, vous captive au plus haut point. Je ne puis mieux comparer ce beau mémoire qu'à ces tableaux de maître auxquels le temps ajoute un nouveau charme. On ne se lasse jamais de les admirer et l'on y découvre sans cesse de nouveaux détails et de nouvelles beautés.

C'est en opérant dans des appareils en fluorure de calcium que les frères Knox essayèrent de décomposer le fluorure d'argent par le chlore. La principale objection à faire à leurs expériences repose sur ce fait que le fluorure d'argent employé n'était pas sec. Il est en effet très difficile de déshydrater complètement les fluorures de mercure et d'argent. De plus, nous verrons, par les recherches de Fremy, que l'action du chlore sur les fluorures tend plutôt à former des produits d'addition, des fluochlorures, qu'à chasser le fluor et à le mettre en liberté.

En 1848, Louyet en opérant aussi dans des appareils en fluorine,

étudia une réaction analogue : il fit réagir le chlore sur le fluorure de mercure. Les objections que l'on peut faire aux recherches des frères Knox s'appliquent aussi aux travaux de Louyet. Fremy a démontré que le fluorure de mercure préparé par le procédé de Louyet renfermait encore une notable quantité d'eau. Aussi les résultats obtenus étaient assez variables. Le gaz recueilli était un mélange d'air, de chlore et d'acide fluorhydrique, dont les propriétés se modifiaient suivant la durée de la préparation.

Les frères Knox se plaignirent beaucoup de l'action de l'acide fluorhydrique sur les voies respiratoires, et, à la suite de leurs travaux l'un d'eux rapporte qu'il a passé trois années à Gênes, et en est revenu encore très souffrant. Quant à Louyet, entraîné par ses recherches, il ne prit pas assez de précautions pour éviter l'action irritante des vapeurs d'acide fluorhydrique, et il paya de sa vie son dévouement à la science.

Ces recherches de Louyet amenèrent Fremy à reprendre vers 1850 cette question de l'isolement du fluor. Fremy étudia d'abord avec méthode les fluorures métalliques; il démontra l'existence de nombreux fluorhydrates de fluorures, indiqua leurs propriétés et leur composition. Puis, il fit réagir un grand nombre de corps gazeux sur ces différents fluorures; l'action du chlore, de l'oxygène fut étudiée avec soin. Enfin, toute son attention fut attirée sur l'électrolyse des fluorures métalliques.

La plupart de ces expériences était faite dans des vases de platine à des températures parfois très élevées. Lorsque, après cette étude général des fluorures, Fremy reprit l'action du chlore sur les fluorures de plomb, d'antimoine, de mercure et d'argent, il montra nettement la presque impossibilité d'obtenir à cette époque ces fluorures absolument secs. Aussi l'on comprend que, dans ces recherches électrolytiques, ce savant se soit adressé surtout au fluorure de calcium.

Ayant vu combien les fluorures retiennent l'eau avec avidité, il revient toujours à cette fluorine, qu'on trouve parfois dans la nature dans un grand état de pureté, et absolument anhydre. C'est ce fluorure de calcium maintenu liquide, grâce à une haute température, qu'il va électrolyser dans un vase de platine.

Dans ces conditions, le métal calcium se porte au pôle négatif, et l'on voit, autour de la tige de platine qui constitue l'électrode négative et qui se ronge avec rapidité, un bouillonnement indiquant la mise en liberté d'un nouveau corps gazeux.

Certainement, dans ces expériences, du fluor a été mis en liberté, mais, messieurs, représentez-vous cette électrolyse faite à la température du rouge vif. Combien l'expérience devient difficile dans ces conditions : comment recueillir le gaz ? comment en constater les propriétés ? Ce corps gazeux déplace l'iode des iodures ; mais, aussitôt que l'on tente quelques essais, le métal alcalin, mis en liberté, perce la paroi de platine ; tout est à recommencer, l'appareil est mis hors d'usage.

Loin de se décourager par les insuccès, Fremy apporte, au contraire, dans ces recherches, une persévérance incroyable. Il varie ses expériences, modifie ses appareils, et les difficultés ne font que l'encourager à poursuivre son étude.

Deux faits importants se dégagent tout d'abord de ses travaux: l'un qui est entré immédiatement dans le domaine de la science; l'autre qui semble avoir frappé beaucoup moins les esprits.

Le premier c'est la préparation de l'acide fluorhydrique anhydre, de l'acide fluorhydrique pur. Jusqu'aux recherches de Fremy, on avait ignoré l'existence de l'acide fluorhydrique vraiment privé d'eau. Ayant préparé et analysé le fluorhydrate de fluorure de potassium, Fremy s'en sert aussitôt pour obtenir l'acide fluorhydrique pur et anhydre.

Il prépare ainsi un corps gazeux à la température ordinaire qui se condense dans un mélange réfrigérant en un liquide incolore très avide d'eau. Voilà donc une réaction d'une grande importance. préparation de l'acide fluorhydrique pur.

Je tiens à vous faire remarquer en passant que le jour ou Humphry Davy a electrolysé l'acide fluorhydrique concentré, le liquide mauvais conducteur qu'il obtenait à la fin de son expérience était de l'acide fluorhydrique à peu près anhydre.

Le second fait, qui a passé je dirai presque inaperçu et qui m'a vivement intéressé, surtout à la fin de mes recherches, c'est que le fluor a la plus grande tendance à s'unir à presque tous les composés par voie d'addition.

En un mot, le fluor forme avec facilité des composés ternaires et quaternaires. Faisons réagir le chlore sur un fluorure; au lieu d'isoler le fluor, nous préparerons un fluochlorure. Employons l'oxygène, nous ferons un oxyfluorure. Cette propriété nous explique l'insuccès des essais de Louyet, des frères Knox et d'autres opérateurs. Même en agissant sur les fluorures secs, dans une atmosphère de chlore, de brome ou d'iode, nous aurons plutôt des composés ternaires que du fluor libre. Ce fait a été nettement mis en évidence par Fremy. Et le mémoire de ce savant comportait un si grand nombre d'expériences, qu'il semble avoir découragé les chimistes, arrêté l'essor de nouvelles tentatives. Depuis 1856, date de la publication du mémoire de M. Fremy, les recherches sur l'acide fluorhydrique et sur l'isolement du fluor sont peu nombreuses. La question paraît subir un temps d'arrêt. Cependant, en 1869, M. Gorre reprend avec méthode l'étude de l'acide fluorhydrique. Il part de l'acide fluorhydrique anhydre préparé par la méthode de Fremy; il détermine son point d'ébullition, sa tension de vapeur aux différentes températures, enfin ses principales propriétés. Son mémoire est d'une exactitude remarquable. Des nombreuses recherches de Gorre, nous ne retiendrons pour le moment que les suivantes, sur lesquelles je veux appeler votre attention.

Ce savant électrolyse dans un appareil spécial de l'acide fluorhydrique anhydre contenant une petite quantité de fluorure de platine,

de telle sorte qu'il puisse recueillir les gaz produits à chaque électrode ; il voit au pôle négatif se dégager de l'hydrogène en abondance, tandis que la tige qui terminait le pôle positif était rongée avec rapidité. Ce phénomène était identique à celui obtenu par Fremy dans l'électrolyse du fluorure de calcium. Gorre vérifie ensuite cette observation de Faraday, que l'acide fluorhydrique contenant de l'eau laisse passer le courant, mais que l'acide fluorhydrique absolument pur, bien anhydre, n'est nullement conducteur. Dans une de ses expériences, Gorre essaye d'électrolyser de l'acide fluorhydrique qui, par suite d'une impureté, était bon conducteur, et voulant éviter l'usure de l'électrode, il y substitue une baguette de charbon.

Ce charbon, il le prépare avec soin, en chauffant dans un courant d'hydrogène un bois dense, qui lui fournit une tige sonore, bonne conductrice de l'électricité. L'appareil étant monté, il commence l'expérience ; aussitôt une violente explosion se produit, les morceaux de charbon sont brisés et projetés aux extrémités du laboratoire. Gorre répète l'expérience plusieurs fois ; le résultat est toujours le même. Nous pouvons aujourd'hui donner l'explication de ce phénomène.

Le charbon qu'il préparait ainsi par distillation d'un bois très dur était rempli d'hydrogène. Vous savez tous, messieurs, combien les gaz se condensent avec facilité dans le charbon ; les belles expériences de Melsens l'ont établi d'une façon très nette. Lorsque l'on électrolysait ensuite de l'acide fluorhydrique conducteur, en plaçant au pôle positif un semblable charbon, il se dégageait du fluor qui s'unit à l'hydrogène, comme nous le verrons plus loin, en produisant une violente détonation. Dans cette expérience de Gorre une petite quantité de fluor avait été mise en liberté, et c'est à sa combinaison avec l'hydrogène occlus dans le charbon que l'explosion était due.

Et maintenant, messieurs, j'arrive aux expériences nouvelles dont j'ai à vous entretenir.

Je suis parti dans ces recherches d'une idée préconçue. Si l'on suppose pour un instant que le chlore n'ait pas encore été isolé, bien que nous sachions préparer les chlorures de phosphore et d'autres composés similaires, il est de toute évidence que l'on augmentera les chances que l'on peut avoir d'isoler cet élément en s'adressant aux composés que le chlore peut former avec les metalloïdes.

Il me semblait qu'on obtiendrait plutôt du chlore, en essayant de décomposer le pentachlorure de phosphore ou l'acide chlorhydrique qu'en s'adressant à l'électrolyse du chlorure de calcium ou d'un chlorure alcalin.

Ne doit-il pas en être de même pour le fluor ?

Enfin le fluor étant, d'après les recherches antérieures et particulièrement celles de Davy, un corps doué d'affinités très énergiques, on devait pour pouvoir recueillir cet élément, opérer à des températures aussi basses que possible.

Telles sont les idées générales qui nous ont amené à reprendre d'une façon systématique l'étude des combinaisons formés par le fluor et les métalloïdes.

Je me suis adressé tout d'abord au fluorure de silicium, et j'ai été frappé, dès ces premières recherches, de la grande stabilité de ce composé. Sauf les métaux alcalins, qui, au rouge sombre, le dédoublent avec facilité, peu de corps agissent sur le fluorure de silicium. Il est facile de se rendre compte de cette propriété si l'on remarque que sa formation est accompagnée d'un très grand dégagement de chaleur. M. Berthelot a démontré depuis longtemps que les corps composés sont d'autant plus stables qu'ils dégagent plus de chaleur au moment de leur production.

J'estimais donc, à tort ou à raison, avant même d'avoir isolé le fluor, que, si l'on parvenait jamais à préparer ce corps simple, il devait se combiner avec incandescence au silicium cristallisé. Et chaque fois que, dans ces longues recherches j'espérais avoir mis du fluor en liberté, je ne manquais pas d'essayer cette réaction ; on verra plus loin qu'elle m'a parfaitement réussi.

Après ces premières expériences sur le fluorure de silicium, j'ai entrepris des recherches sur les composés du fluor et du phosphore.

M. Thorpe a découvert le composé $PhFl^5$ un pentafluorure de phosphore ; j'ai préparé le composé $PhFl^3$ et j'ai porté toute mon attention sur les réactions qui permettaient d'essayer un dédoublement. J'ai fait cette expérience à laquelle avait songé Humphry Davy, de faire brûler le trifluorure de phosphore dans l'oxygène, et je me suis aperçu qu'il n'y avait pas eu formation d'acide phosphorique et mise en liberté du fluor, comme l'espérait le savant anglais, mais que le trifluorure et l'oxygène s'étaient unis pour donner un nouveau corps gazeux, l'oxyfluorure de phosphore.

N'était-ce pas là un nouvel exemple de cette facilité que possède le fluor de fournir des produits d'addition ?

J'ai tenté alors, mais inutilement, l'action de l'étincelle d'induction sur le trifluorure de phosphore. Cependant le pentafluorure de phosphore découvert par M. Thorpe a pu être dédoublé par de très fortes étincelles en trifluorure de phosphore et fluor.

Cette expérience était faite dans une éprouvette de verre sur la cuve à mercure ; vous pensez bien qu'immédiatement, il se produisait du fluorure de mercure et du fluorure de silicium. On ne pouvait pas espérer dans ces conditions conserver le fluor, même noyé dans un excès de pentafluorure. J'ai donc songé à une autre réaction.

On savait, depuis les recherches de Fremy, que le fluorure de platine, produit dans l'électrolyse des fluorures alcalins, se décomposait sous l'influence d'une température élevée. Ayant constaté que les fluorures de phosphore sont facilement absorbés à chaud par la mousse de platine, avec production finale de phosphure de platine, nous avions pensé que ce procédé de préparation du fluorure de platine permettrait d'isoler le fluor. En chauffant peu d'abord, l'absorption du fluorure de phosphore, par exemple, donnerait un mélange de phosphore et de fluorure de platine, et la quantité de ce dernier étant assez grande, une élévation de température pourrait en dégager le fluor. Ces expériences et d'autres analogues ont été tentées dans les

conditions les plus propres à en assurer le succès ; elles ont fourni des résultats intéressants, mais qui n'avaient pas une netteté suffisante pour résoudre la question de l'isolement du fluor.

En même temps que se poursuivaient les études précédentes, je préparais le trifluorure d'arsenic qui avait été obtenu par Dumas dans un grand état de pureté ; je déterminais ses constantes physiques ainsi que quelques propriétés nouvelles, et j'apportais tous mes soins à étudier l'action du courant électrique sur ce composé.

Le fluorure d'arsenic, corps liquide à la température ordinaire, composé binaire formé d'un corps solide, l'arsenic et d'un corps gazeux, le fluor, semblait se prêter dans d'excellentes conditions à des expériences d'électrolyse.

J'ai dû, à quatre reprises différents, interrompre ces recherches sur le fluorure d'arsenic, dont le maniement est plus dangereux que celui de l'acide fluorhydrique anhydre et dont les propriétés toxiques m'avaient mis dans l'impossibilité de continuer ces expériences.

Je suis arrivé cependant à électrolyser ce composé en employant le courant produit par 90 éléments Bunsen.

Dans ces conditions, le courant passe d'une façon continue ; l'arsenic se dépose à l'état pulvérulent au pôle négatif, et l'on voit se former sur l'électrode positive des bulles gazeuses qui montent dans le liquide mais sont absorbées presque aussitôt. Le fluor mis en liberté est repris de suite par le trifluorure d'arsenic $AsFl^3$ qui passe à l'état de pentafluorure $AsFl^5$. Cette expérience, poursuivie pendant longtemps, ne m'a pas donné le fluor ; mais elle m'a fourni de précieux renseignements sur l'électrolyse des composés fluorés liquides, et elle m'a conduit à la décomposition de l'acide fluorhydrique anhydre.

Pour arriver à l'électrolyse de l'acide fluorhydrique, j'avais fait faire un petit appareil que vous avez sous les yeux et qui est formé d'un tube en U en platine portant sur chaque branche un tube abducteur placé au-dessus du niveau du liquide.

Les deux ouvertures de ce tube en U devaient être fermées par des bouchons de liège imbibés au préalable de paraffine ainsi que nous l'avions fait dans toutes nos expériences sur l'électrolyse du fluorure d'arsenic.

Un fil de platine traversait chaque bouchon et était mis en communication avec une pile de cinquante éléments Bunsen.

Nous avons préparé tout d'abord de l'acide fluorhydrique pur et anhydre, et nous avons vu que ce liquide, ainsi que l'avait indiqué Faraday et ensuite Gorre, ne conduisait nullement le courant.

L'expérience a été variée de bien des façons, le résultat est toujours le même. Avec le courant fourni par 90 éléments Bunsen, la décomposition ne se produit que lorsqu'on s'adresse à un acide hydraté, et cette décomposition s'arrête aussitôt que toute l'eau a été séparée en hydrogène et oxygène. Il semble donc impossible d'obtenir, par ce procédé, le dédoublement de l'acide fluorhydrique en ses éléments : hydrogène et fluor.

Je me suis souvenu à ce moment, que, dans les études précédentes

sur le fluorure d'arsenic, j'avais essayé de rendre ce liquide bon conducteur, en l'additionnant d'une petite quantité de fluorure de manganèse ou de fluorhydrate de fluorure de potassium. Ce procédé fut appliqué à l'acide fluorhydrique, et c'est alors qu'après trois années de recherches, j'arrivai à la première expérience importante sur l'isolement du fluor.

L'acide fluorhydrique contenant du fluorhydrate de fluorure de potassium se décompose sous l'action du courant et, dans l'appareil que vous avez sous les yeux, on peut obtenir au pôle négatif un dégagement régulier de gaz hydrogène. Qu'obtient-on au pôle positif? Rien. Une légère augmentation de pression, voilà tout. Seulement, en démontant l'appareil, on remarque que le bouchon de liège du pôle positif a été brûlé, carbonisé, sur une profondeur d'un centimètre. Le bouchon de liège paraffiné du pôle négatif n'a pas été altéré. Il s'est donc dégagé au pôle positif un corps agissant sur le liège avec une activité toute différente de celle de l'acide fluorhydrique.

Je dois ajouter qu'afin de diminuer la tension de vapeur de l'acide fluorhydrique, nous avons refroidi ce liquide dans nos expériences au moyen du chlorure de méthyle, qui, par une rapide évaporation, nous produit un froid de -50° (centigrade).

Il a fallu modifier l'appareil et particulièrement la fermeture du tube en U. Les bouchons en fluorine à frottement doux ne m'ont pas donné de bons résultats. La gomme laque ou la gutta-percha dont on les entourait était rapidement attaqué par le corps gazeux produit au pôle positif. On dut alors recourir à une fermeture gazeuse, au moyen de pas de vis en platine, et voici après bien tâtonnements, comment l'expérience fut disposée.

Le tube en U en platine est fermé par des bouchons à vis. Chacun de ces bouchons est formé par un cylindre de spath-fluor, bien serti dans un cylindre creux de platine, dont l'extérieur porte le pas de vis. Chaque bouchon de fluorine laisse passer en son axe une tige carrée de platine. Ces tiges, plongeant par leur extrémité inférieure dans le liquide, servaient d'électrodes. Enfin, deux ajutages en platine soudés à chaque branche du tube, au-dessous des bouchons, par conséquent au-dessus du niveau du liquide, permettaient aux gaz dégagés par l'action du courant de s'échapper au dehors.

Pour obtenir l'acide fluorhydrique pur et anhydre on commence par préparer le fluorhydrate de fluorure de potassium en prenant toutes les précautions indiquées par Fremy. Lorsqu'on a obtenu ce sel pur, on le dessèche au bain-marie à 100°, et la capsule qui le contient est placée ensuite dans le vide en présence d'acide sulfurique concentré et de potasse fondue au creuset d'argent. L'acide et la potasse sont remplacés tous les matins pendant quinze jours et le vide est toujours maintenu dans les cloches à 1 centim. de mercure environ.

Il faut avoir soin pendant cette dessiccation, de pulvériser le sel de temps en temps dans un mortier de fer, afin de renouveler les surfaces; lorsque le fluorhydrate ne contient plus d'eau, il tombe en poussière

et peut alors servir à préparer l'acide fluorhydrique. Il est à remarquer que le fluorhydrate de fluorure de potassium bien préparé est beaucoup moins déliquescent que le fluorure.

Lorsque le fluorhydrate est bien sec, il est introduit rapidement dans un alambic en platine que l'on a séché en le portant au rouge peu de temps auparavant. On le maintient à une douce température pendant une heure ou une heure et demie de façon que la décomposition commence très lentement; on perd la première portion d'acide fluorhydrique formé qui entraîne avec elle les petites traces d'eau pouvant rester dans le sel. Le récipient de platine est alors adapté à la cornue et l'on chauffe plus fortement, tout en conduisant la décomposition du fluorhydrate avec une certaine lenteur. On entoure ensuite ce récipient d'un mélange de glace et de sel, et à partir de ce moment, tout l'acide fluorhydrique est condensé et fournit un liquide limpide, bouillant à 19°·5, très hygroscopique et produisant, comme l'on sait, d'abondantes fumées en présence de l'humidité de l'air.

Pendant cette opération, le tube en U en platine, desséché avec le plus grand soin, a été fixé au moyen d'un bouchon dans un vase de verre cylindrique et entouré de chlorure de méthyle. Jusqu'au moment de l'introduction de l'acide fluorhydrique, les tubes abducteurs sont reliés à des éprouvettes desséchantes contenant de la potasse fondue. Pour faire pénétrer l'acide fluorhydrique dans ce petit appareil, on peut l'absorber par l'un des tubes latéraux dans le récipient même où il s'est condensé.

Lorsqu'on a fait pénétrer, à l'avance, un volume determiné d'acide fluorhydrique liquide dans le petit appareil en platine, refroidi par le chlorure de méthyle en ébullition tranquille, à la température de −23°, on fait passer dans les électrodes le courant produit par 25 éléments Bunsen, grand modèle, montés en série. Un ampèremètre, placé dans le circuit, permet de se rendre compte de l'intensité du courant.

Afin de rendre l'acide conducteur, nous y avons ajouté, avant l'expérience, une petite quantité de fluorhydrate de fluorure de potassium séché et fondu; environ 2 grammes pour 10 centimètres cubes d'acide. Dans ce cas, la décomposition se produit d'une façon continue, et l'on obtient, au pôle négatif, un gaz brûlant avec une flamme incolore et présentant tous les caractères de l'hydrogène; au pôle positif, un gaz incolore d'une odeur pénétrante très désagréable, se rapprochant de celle de l'acide hypochloreux, et irritant rapidement la muqueuse de la gorge et les yeux. Nous faisons en ce moment l'expérience sous vos yeux. Le nouveau corps gazeux est doué de propriétés très énergiques: vous voyez le soufre s'enflammer à son contact.

Le phosphore prend feu et fournit un mélange d'oxyfluorure et de fluorure de phosphore. L'iode s'y combine avec une flamme pâle en perdant sa couleur. L'arsenic et l'antimoine en poudre se combinent au fluor avec incandescence.

Le silicium cristallisé, froid, brûle de suite au contact de ce gaz

avec beaucoup d'éclat. Parfois il se produit des étincelles; il se forme du fluorure de silicium qui a été recueilli sur le mercure et nettement caractérisé.

Le bore pur brûle également en se transformant en fluorure de bore. Le carbone amorphe devient incandescent au contact du fluor. Pour faire ces différentes expériences, il suffit de placer, comme vous le voyez, les corps solides dans un petit tube de verre et de les approcher de l'extrémité du tube de platine par lequel se dégage le fluor. On peut aussi répéter ces expériences en mettant de petits fragments des corps solides à étudier sur le couvercle d'un creuset de platine maintenu auprès de l'ouverture du tube abducteur.

Ce gaz décompose l'eau à froid en fournissant de l'acide fluorhydrique et de l'ozone; il enflamme le sulfure de carbone et, recueilli dans une capsule de platine remplie de tétrachlorure de carbone, il fournit un dégagement continu de chlore.

Le chlorure de potassium fondu est attaqué à froid, avec dégagement de chlore. En présence du mercure, l'absorption est complète avec formation de protofluorure de mercure de couleur jaune clair. Le potassium et le sodium deviennent incandescents et fournissent des fluorures. D'une façon générale, les métaux sont attaqués avec beaucoup moins d'énergie que les métalloïdes. Cela tient, pensons-nous, à ce que la petite quantité de fluorure métallique formé empêche l'attaque d'être profonde. Le fer et le manganèse en poudre brûlent en fournissant des étincelles.

Les corps organiques sont violemment attaqués. Un morceau de liège, placé auprès de l'extrémité du tube de platine par lequel le gaz se dégage, se carbonise aussitôt et s'enflamme. L'alcool, l'éther, la benzine, l'essence de térébenthine, le pétrole prennent feu à son contact.

En opérant dans de bonnes conditions on peut obtenir à chaque pôle un rendement de 2 litres à 4 litres de gaz par heure.

Lorsque l'expérience a duré plusieurs heures et que la quantité d'acide fluorhydrique liquide restant au fond du tube n'est plus suffisante pour séparer les deux gaz, ils se recombinent à froid dans l'appareil en platine, avec une violente détonation.

Nous nous sommes assurés par des expériences directes, faites au moyen d'ozone saturé d'acide fluorhydrique, qu'un semblable mélange ne produit aucune des réactions décrites précédemment. Il en est de même de l'acide fluorhydrique gazeux. Nous ajouterons que l'acide fluorhydrique employé ainsi que le fluorhydrate de fluorure étaient absolument exempts de chlore. Enfin, on ne peut pas objecter que le nouveau gaz produit soit un perfluorure d'hydrogène; car en présence de fer chauffé au rouge maintenu dans un tube de platine, il est absorbé entièrement sans dégagement d'hydrogène.

Enfin, dans des recherches plus récentes je me suis assuré qu'il est possible de faire ces expériences dans un appareil de cuivre tel que celui que vous avez devant vous.

Par l'électrolyse de l'acide fluorhydrique rendu conducteur au

moyen de fluorhydrate de fluorure de potassium, on obtient donc au pôle négatif de l'hydrogène et au pôle positif un dégagement continu d'un corps gazeux présentant des propriétés nouvelles, doué d'affinités très énergiques : ce corps gazeux est le fluor.

Nous avons pu en déterminer la densité, la couleur, le spectre, étudier son action sur les corps simples et composés.

Maintenant que l'on connait les principales propriétés du fluor, maintenant que cet élément a pu être isolé, je suis convaincu que l'on trouvera, malgré l'énergie de ses réactions, de nouvelles méthodes de préparation.

Il est à croire que l'on arrivera à préparer le fluor par un procédé chimique fournissant de meilleurs rendements que le procédé électrolytique.

Le fluor aura-t-il jamais des applications?

Il est bien difficile de répondre à cette question. D'ailleurs, je puis le dire en toute sincérité, je n'y pensais guère au moment ou j'ai entrepris ces recherches, et je crois que tous les chimistes qui ont tenté ces expériences avant moi n'y pensaient pas davantage.

Une recherche scientifique est une recherche de la vérité, et ce n'est qu'après cette première découverte que les idées d'application peuvent se produire avec utilité.

Il est évident que lorsqu'on voit les grandes transformations industrielles qui se font aujourd'hui sous nos yeux, on ne peut se prononcer sur cette question. Après la préparation de l'acier Bessemer, la fabrication du manganèse au haut fourneau, la production de l'alizarine de synthèse, le chimiste hésite toujours à nier la vitalité industrielle d'une réaction de laboratoire.

Quand on pense à la valeur qu'avaient certains métaux tels que le potassium et le sodium, lorsque Davy les préparait par électrolyse ; quand on se rappelle que, par le procédé de Gay-Lussac et Thénard, ils revenaient à quelques milliers de francs le kilogramme, et qu'aujourd'hui par les méthodes électrolytiques ils ne coûtent plus que 5 francs, on n'ose plus dire qu'une réaction chimique ne saurait avoir d'applications industrielles.

Seulement, messieurs, et c'est par là que je termine, il est curieux de voir combien il faut d'efforts continus, de vues différentes, pour arriver à résoudre une de ces questions scientifiques ; je devrais dire plutôt pour faire progresser une de ces questions scientifiques, car en réalité un sujet n'est jamais fermé. Il reste toujours ouvert pour nos successeurs : nous ne faisons qu'ajouter un anneau à une chaine sans fin.

L'avancement de la science est lent ; il ne se produit qu'à force de travail et de ténacité. Et lorsqu'on est arrivé à un résultat, ne doit-on pas par reconnaissance se reporter aux efforts de ceux qui vous ont precédés, de ceux qui ont lutté et peiné avant vous? N'est-ce pas en effet un devoir de rappeler les difficultés qu'ils ont vaincues, les vues qui les ont dirigés et comment des hommes, différents de pays et d'idées, de position, et de caractère, mus seulement par l'amour de

la science, se sont légués sans se connaître la question inachevée; afin qu'un dernier venu pût recueillir les recherches de ses devanciers et y ajouter à son tour, sa part d'intelligence et de travail? Collaboration intellectuelle entièrement consacrée à la recherche de la vérité et qui se poursuit ainsi de siècle en siècle.

Ce patrimoine scientifique que nous cherchons toujours à étendre est une partie de la fortune de l'humanité; nous devons garder un souvenir reconnaissant à tous ceux qui lui ont donné la chaleur de leur cœur et le meilleur de leur esprit.

[H. M.]

WEEKLY EVENING MEETING,

Friday, June 4, 1897.

SIR FREDERICK BRAMWELL, Bart. D.C.L. LL.D. F.R.S.
Honorary Secretary and Vice-President, in the Chair.

W. H. PREECE, Esq. C.B. F.R.S. M. Inst. C.E.

Signalling through Space without Wires.

IENCE has conferred one great benefit on mankind. It has supied us with a new sense. We can now see the invisible, hear the audible, and feel the intangible. We know that the universe is lled with a homogeneous continuous elastic medium which transmits at, light, electricity and other forms of energy from one point of ace to another without loss. The discovery of the real existence this "ether" is one of the great scientific events of the Victorian a. Its character and mechanism are not yet known by us. All tempts to "invent" a perfect ether have proved beyond the mental wers of the highest intellects. We can only say with Lord Salisury that the ether is the nominative case to the verb "to undulate." 'e must be content with a knowledge of the fact that it was created the beginning for the transmission of energy in all its forms, at it transmits these energies in definite waves and with a known locity, that it is perfect of its kind, but that it still remains as scrutable as gravity or life itself.

Any disturbance of the ether must originate with some disturbce of matter. An explosion, cyclone or vibratory motion may cur in the photosphere of the sun. A disturbance or wave is imessed on the ether. It is propagated in straight lines through ace. It falls on Jupiter, Venus, the Earth and every other planet it with in its course, and any machine, human or mechanical, pable of responding to its undulations indicates its presence. Thus e eye supplies the sensation of light, the skin is sensitive to heat, e galvanometer indicates electricity, the magnetometer indicates sturbances in the earth's magnetic field. One of the greatest ientific achievements of our generation is the magnificent generaliion of Clerk-Maxwell that all these disturbances are of precisely e same kind, and that they differ only in degree. Light is an ctromagnetic phenomenon, and electricity in its progress through ace follows the laws of optics. Hertz proved this experimentally, d few of us who heard it will forget the admirable lecture on

"The Work of Hertz" given in this hall by Prof. Oliver Lodge three years ago.*

By the kindness of Prof. Silvanus Thompson I am able to illustrate wave transmission by a very beautiful apparatus devised by him. At one end we have the *transmitter* or oscillator, which is a heavy suspended mass to which a blow or impulse is given, and which, in consequence, vibrates a given number of times per minute. At the other end is the *receiver*, or resonator, timed to vibrate to the same period. Connecting the two together is a row of leaden balls suspended so that each ball gives a portion of its energy at each oscillation to the next in the series. Each ball vibrates at right angles to or athwart the line of propagation of the wave, and as they vibrate in different phases you will see that a wave is transmitted from the transmitter to the receiver. The receiver takes up these vibrations and responds in sympathy with the transmitter. Here we have a visible illustration of that which is absolutely invisible. The wave you see differs from a wave of light or of electricity only in its length or in its frequency. Electric waves vary from units per second in long submarine cables to millions per second when excited by Hertz's method. Light-waves vary per second between 400 billions in the red to 800 billions in the violet, and electric waves differ from them in no other respect. They are reflected, refracted and polarised, they are subject to interference, and they move through the ether in straight lines with the same velocity, viz. 186,400 miles per second—a number easily recalled when we remember that it was in the year 1864 that Maxwell made his famous discovery of the identity of light and electric waves.

Electric waves, however, differ from light waves in this, that we have also to regard the direction at right angles to the line of propagation of the wave. The model gives an illustration of that which happens along a *line of electric force*, the other line of motion I speak of is a circle around the point of disturbance, and these lines are called *lines of magnetic force*.† The animal eye is tuned to one series of waves, the "electric eye," as Lord Kelvin called Hertz's resonator, to another. If electric waves could be reduced in length to the forty-thousandth of an inch we should see them as colours.

One more definition, and our ground is cleared. When electricity is found stored up in a potential state in the molecules of a dielectric like air, glass or gutta-percha, the molecules are strained, it is called a *charge*, and it establishes in its neighbourhood *an electric field*. When it is active, or in its kinetic state in a circuit, it is called a *current*. It is found in both states, kinetic and potential, when a current is maintained in a conductor. The surrounding

* This is published in an enlarged and useful form by 'The Electrician' Printing and Publishing Company.—W. H. P.

† *Vide* Fig. 4, p. 474.

neighbourhood is then found in a state of stress forming what is called a *magnetic field.*

In the first case the charges can be made to rise and fall, and to surge to and fro with rhythmic regularity, exciting *electric waves* along each line of electric force at very high frequencies, and in the second case the currents can rise or alternate in direction with the same regularity—but with very different frequencies—and originate *electromagnetic waves* whose wave fronts are propagated in the same direction.

The first is the method of Hertz, which has recently been turned to practical account by Mr. Marconi, and the second is the method which I have been applying, and which for historical reasons I will describe to you first.

In 1884 messages sent through insulated wires buried in iron pipes in the streets of London were read upon telephone circuits erected on poles above the housetops, 80 feet away. Ordinary telegraph circuits were found in 1885 to produce disturbances 2000 feet away. Distinct speech by telephone was carried on through one quarter of a mile, a distance that was increased to 1¼ mile at a later date. Careful experiments were made in 1886 and 1887 to prove that these effects were due to pure electromagnetic waves, and were entirely free from any earth-conduction. In 1892 distinct messages were sent across a portion of the Bristol Channel between Penarth and Flat Holm, a distance of 3·3 miles.

Early in 1895 the cable between Oban and the Isle of Mull broke down, and as no ship was available for repairing and restoring communication, communication was established by utilising parallel wires on each side of the channel and transmitting signals across this space by these electromagnetic waves.

The apparatus (Fig. 1) connected to each wire consists of—

(*a*) A rheotome or make and break wheel, causing about 260 undulations per second in the primary wire.

(*b*) An ordinary battery of about 100 Leclanché cells, of the so-called dry and portable form.

(*c*) A Morse telegraph key.

(*d*) A telephone to act as receiver.

(*e*) A switch to start and stop the rheotome.

Good signals depend more on the rapid rise and fall of the primary current than on the amount of energy thrown into vibration. Leclanché cells give as good signals at 3·3 miles distant as 2½ H.P. transformed into alternating currents by an alternator, owing to the smooth sinusoidal curves of the latter. 260 vibrations per second give a pleasant note to the ear, easily read when broken up by the key into dots and dashes.

In my electromagnetic system two parallel circuits are established, one on each side of a channel or bank of a river, each circuit becoming successively the primary and secondary of an induction system, according to the direction in which the signals are being

sent. Strong alternating or vibrating currents of electricity are transmitted in the first circuit so as to form signals, letters and words in Morse character. The effects of the rise and fall of these currents are transmitted as electromagnetic waves through the intervening space, and if the secondary circuit is so situated as to be washed by these ethereal waves, their energy is transformed into secondary currents in the second circuit which can be made to affect a telephone and thus to reproduce the signals. Of course their intensity is much reduced, but still their presence has been detected though five miles of clear space have separated the two circuits.

Such effects have been known scientifically in the laboratory since the days of Faraday and of Henry, but it is only within the

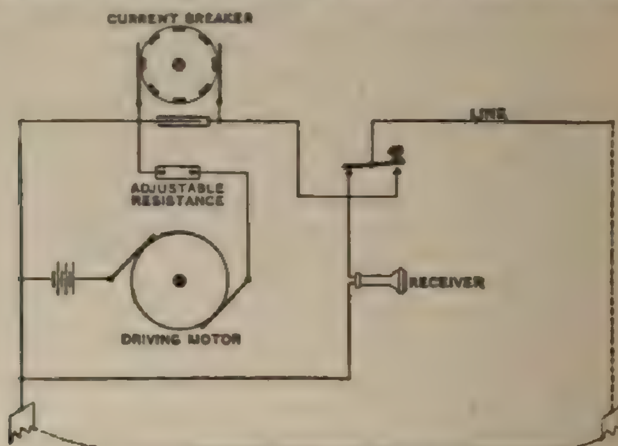


FIG. 1.—Diagram of connections of Mr. Preece's system.

last few years that I have been able to utilise them practically through considerable distances. This has been rendered possible through the introduction of the telephone.

Last year (August, 1896) an effort was made to establish communication with the North Sandhead (Goodwin) lightship. The apparatus used was designed and manufactured by Messrs. Evershed and Vignoles, and a most ingenious relay to establish a call was invented by Mr. Evershed. One extremity of the cable was coiled in a ring on the bottom of the sea, embracing the whole area over which the lightship swept while swinging to the tide, and the other end was connected with the shore. The ship was surrounded above the water line with another coil. The two coils were separated by a mean distance of about 200 fathoms, but communication was found to be impracticable. The screening effect of the sea water and the effect of the iron hull of the ship absorbed practically all the energy

of the currents in the coiled cable, and the effects on board, though perceptible, were very trifling—too minute for signalling. Previous experiments had failed to show the extremely rapid rate at which energy is absorbed with the depth or thickness of sea water. The energy is absorbed in forming eddy currents. There is no difficulty whatever in signalling through 15 fathoms. Speech by telephone has been maintained through 6 fathoms. Although this experiment has failed through water, it is thoroughly practical through air to considerable distances where it is possible to erect wires of similar length to the distance to be crossed on each side of the channel. It is not always possible, however, to do this, nor to get the requisite height to secure the best effect. It is impossible on a lightship and on rock lighthouses. There are many small islands—Sark, for example—where it cannot be done.

In July last Mr. Marconi brought to England a new plan. My plan is based entirely on utilising electromagnetic waves of very low frequency. It depends essentially on the rise and fall of *currents* in the primary wire. Mr. Marconi utilises electric or Hertzian waves of very high frequency, and they depend upon the rise and fall of electric force in a sphere or spheres. He has invented a new relay which, for sensitiveness and delicacy, exceeds all known electrical apparatus.

The peculiarity of Mr. Marconi's system is that, apart from the ordinary connecting wires of the apparatus, conductors of very moderate length only are needed, and even these can be dispensed with if reflectors are used.

The Transmitter.—His transmitter is Prof. Righi's form of Hertz's radiator (Fig. 2).

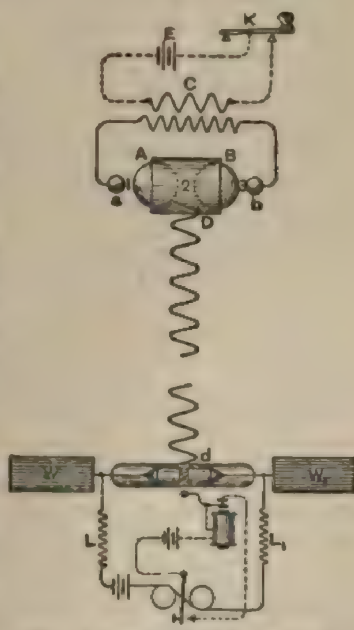


Fig. 2.—Diagram of the Marconi apparatus.

Two spheres of *solid* brass, 4 inches in diameter (A and B), are fixed in an oil-tight case D of insulating material, so that a hemisphere of each is exposed, the other hemisphere being immersed in a bath of vaseline oil. The use of oil has several advantages. It maintains the surfaces of the spheres electrically clean, avoiding the frequent polishing required by Hertz's exposed balls. It impresses on the waves excited by these spheres a uniform and constant form.

It tends to reduce the wave lengths—Righi's waves are measured in centimetres, while Hertz's were measured in metres. For these reasons the distance at which effects are produced is increased. Mr. Marconi uses generally waves of about 120 centimetres long. Two small spheres, *a* and *b*, are fixed close to the large spheres, and connected each to one end of the secondary circuit of the "induction coil" C, the primary circuit of which is excited by a battery E, thrown in and out of circuit by the Morse key K. Now, whenever the key K is depressed sparks pass between 1, 2 and 3, and since the system A B contains capacity and electric inertia, oscillations are set up in it of extreme rapidity. The line of propagation is D *d*, and the frequency of oscillation is probably about 250 millions per second.

The distance at which effects are produced with such rapid oscillations depends chiefly on the energy in the discharge that passes. A 6-inch spark coil has sufficed through 1, 2, 3, up to four miles, but for greater distances we have used a more powerful coil—one emitting sparks 20 inches long. It may also be pointed out that this distance increases with the diameter of the spheres A and B, and it is nearly doubled by making the spheres solid instead of hollow.

The Receiver.—Marconi's relay (Fig. 2) consists of a small glass tube four centimetres long, into which two silver pole-pieces are tightly fitted, separated from each other by about half a millimetre —a thin space which is filled up by a mixture of fine nickel and silver filings, mixed with a trace of mercury. The tube is exhausted to a vacuum of 4 mm., and sealed. It forms part of a circuit containing a local cell and a sensitive telegraph relay. In its normal condition the metallic powder is virtually an insulator. The particles lie higgledy-piggledy, anyhow in disorder. They lightly touch each other in an irregular method, but when electric waves fall upon them they are "polarised," order is installed. They are marshalled in serried ranks, they are subject to pressure—in fact, as Prof. Oliver Lodge expresses it, they "cohere"—electrical contact ensues and a current passes. The resistance of such a space falls from infinity to about five ohms. The electric resistance of Marconi's relay—that is, the resistance of the thin disc of loose powder—is practically infinite when it is in its normal or disordered condition. It is, then, in fact, an insulator. This resistance drops sometimes to five ohms, when the absorption of the electric waves by it is intense. It therefore becomes a conductor. It may be, as suggested by Prof. Lodge, that we have in the measurement of the variable resistance of this instrument a means of determining the intensity of the energy falling upon it. This variation is being investigated both as regards the magnitude of the energy and the frequency of the incident waves. Now such electrical effects are well known. In 1866 Mr. S. A. Varley introduced a lightning protector constructed like the above tube, but made of boxwood and containing powdered carbon. It was fixed as a shunt to the instrument to be protected. It acted well, but it was subject to this coherence, which rendered

the cure more troublesome than the disease, and its use had to be abandoned. The same action is very common in granulated carbon microphones like Hunning's, and shaking has to be resorted to to decohere the carbon particles to their normal state. Mons. E. Branly (1890) showed the effect with copper, aluminium and iron filings. Prof. Oliver Lodge, who has done more than any one else in England to illustrate and popularise the work of Hertz and his followers, has given the name "coherer" to this form of apparatus. Marconi "decoheres" by making the local current very rapidly vibrate a small hammer head against the glass tube, which it does effectually, and in

Fig. 3.—Map of locality where the experiments were carried out.

doing so makes such a sound that reading Morse characters is easy. The same current that decoheres can also record Morse signals on paper by ink. The exhausted tube has two wings which, by their size, tune the receiver to the transmitter by varying the capacity of the apparatus.* Choking coils prevent the energy escaping. The analogy to Prof. Silvanus Thompson's wave apparatus is evident. Oscillations set up in the transmitter fall upon the receiver tuned in

* The period of vibration of a circuit is given by the equation $T = 2\pi\sqrt{KL}$, so that we have simply to vary either the capacity K or the so-called "self-induction" L to tune the receiver to any frequency. It is simpler to vary K.

sympathy with it, coherence follows, currents are excited and signals made.

In open clear spaces within sight of each other nothing more is wanted, but when obstacles intervene and great distances are in question height is needed—tall masts, kites and balloons have been used. Excellent signals have been transmitted between Penarth and Brean Down, near Weston-super-Mare, across the Bristol Channel, a distance of nearly nine miles (Fig. 3). [The system was here shown in operation.]

Mirrors also assist and intensify the effects. They were used in the earlier experiments, but they have been laid aside for the present, for they are not only expensive to make, but they occupy much time in manufacture.

It is curious that hills and apparent obstructions fail to obstruct. The reason is probably the fact that the lines of force escape these hills. When the ether is entangled in matter of different degrees of inductivity the lines are curved as in fact they are in light. Fig. 4

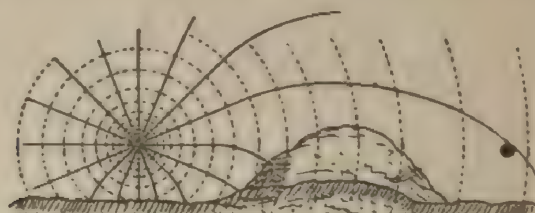


FIG. 4.—Diagram illustrating the way in which hills are bridged by the electric waves.

shows how a hill is virtually bridged over by these lines, and consequently some electric waves fall on the relay. Weather seems to have no influence: rain, fogs, snow and wind avail nothing.

The wings shown in Fig. 2 may be removed. One pole can be connected with earth, and the other extended up to the top of the mast, or fastened to a balloon by means of a wire. The wire and balloon or kite covered with tin foil becomes the wing. In this case one pole of the transmitter must also be connected with earth. This is shown by Fig. 5.

There are some apparent anomalies that have developed themselves during the experiments. Mr. Marconi finds that his relay acts even when it is placed in a perfectly closed metallic box. This is the fact that has given rise to the rumour that he can blow up an ironclad ship. This might be true if he could plant his properly tuned receiver in the magazine of an enemy's ship. Many other funny things could be done if this were possible. I remember in my childhood that Capt. Warner blew up a ship at a great distance off

Brighton. How this was done was never known, for his secret died shortly afterwards with him. It certainly was not by means of Marconi's relay.

The distance to which signals have been sent is remarkable. On Salisbury Plain Mr. Marconi covered a distance of four miles. In the Bristol Channel this has been extended to over eight miles, and we have by no means reached the limit. It is interesting to read the surmises of others. Half a mile was the wildest dream.*

It is easy to transmit many messages in any direction at the same time. It is only necessary to tune the transmitters and receivers to the same frequency or "note." I could show this here, but we are

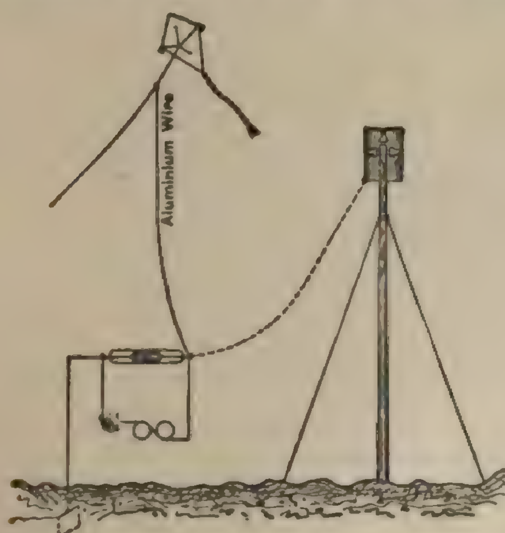


FIG. 5.—Diagram of Marconi connections when using pole or kite.

bothered by reflection from the walls. This does not happen in open space. Tuning is very easy. It is simply necessary to vary the capacity of the receiver, and this is done by increasing the length of the wings W in Fig. 2. The proper length is found experimentally close to the transmitter. It is practically impossible to do so far away.

* "Unfortunately at present we cannot detect the electromagnetic waves more than 100 feet from their source."—Trowbridge, 1897, 'What is Electricity,' page 256.

"I mention 10 yards because that was one of the first out of door experiments, but I should think that something more like half a mile was nearer the limit of sensibility. However, this is a rash statement not at present verified."—Oliver Lodge, 1894, 'The Work of Hertz,' page 18.

It has been said that Mr. Marconi has done nothing new. He has not discovered any new rays; his transmitter is comparatively old; his receiver is based on Branly's coherer. Columbus did not invent the egg, but he showed how to make it stand on its end, and Marconi has produced from known means a new electric eye more delicate than any known electrical instrument, and a new system of telegraphy that will reach places hitherto inaccessible. There are a great many practical points connected with this system that require to be threshed out in a practical manner before it can be placed on the market, but enough has been done to prove its value, and to show that for shipping and lighthouse purposes it will be a great and valuable acquisition.

[W. H. P.]

WEEKLY EVENING MEETING,

Friday, June 11, 1897.

r Frederick Abel, Bart. K.C.B. D.C.L. LL.D. F.R.S. Vice-President, in the Chair.

William Crookes, Esq. F.R.S. *M.R.I.*

Diamonds.

s but the other day I saw London in a blaze of illumination rate Her Majesty's happy accession to the throne. As in a s the whole Empire will be celebrating the Diamond Jubilee Queen, who will then have reigned over her multitudinous for sixty years, what more suitable topic can I bring before in than that of Diamonds! One often hears the question Why *Diamond* Jubilee?" I suppose it is a symbol intended a faint notion of the pure brilliancy and durability of the reign; and in thus associating Her Majesty with the precious d, to convey an idea of those noble qualities public and private ave earned for her the love, fealty and reverence of her sub-

m the earliest times the diamond has occupied men's minds. een a perennial puzzle—one of the riddles of creation. The pher Steffans is accredited with the dictum that, "Diamond is which has arrived at self-consciousness!" and an eminent st has parodied this metaphysical definition, saying: "Quartz nd which has become insane!"

fessor Maskelyne, in a lecture "On Diamonds," thirty-seven go,* in this very theatre, said, "The diamond is a substance transcends all others in certain properties to which it is l for its usefulness in the arts and its beauty as an ornament. n the one hand, it is the hardest substance found in nature or d by art. Its reflecting power and refractive energy, on the and, exceed those of all other colourless bodies, while it yields in the perfection of its pellucidity"—but he was constrained "The formation of the diamond is an unsolved problem."

ntly the subject has attracted many men of science. The ment of electricity, with the introduction of the electric furs facilitated research, and I think I am justified in saying that iamond problem is not actually solved, it is certainly no longer le.

* 'Chemical News,' vol. i. p. 208.

In the early part of last year, accompanied by my wife, I visited some of our Colonies in South Africa, and spent a considerable time in the neighbourhood of the famous Diamond Mines of Kimberley, where I had an exceptionally good opportunity of studying the peculiar geological formation, and of noting interesting facts connected with the occurrence of the precious stone which forms the subject of this evening's lecture.

Although the experiments I wish to bring before you are chiefly connected with the physical and chemical properties of diamonds, and of the light that recent researches throws upon their probable formation, it will possibly act as a kind of compensation for the dryness of some of the theoretical points if with the help of a few photographs* taken on the spot, I bring before your very eyes the general character of the famous mines and their surroundings.

The most famous diamond mines are Kimberley, De Beers, Dutoitspan, Bulfontein and Wesselton. They are situated in latitude 28° 43′ South, and longitude 24° 46′ East. Kimberley town is 4042 feet above sea-level. Other mines in the district, as yet unimportant, are worked for diamonds. Kimberley is practically in the centre of the present diamond-producing area. Besides these mines, there are in the Orange Free State, about 60 miles from the Kimberley diamond region, two others of some importance known as Jagersfontein and Coffeefontein.

Before describing the present mode of diamond extraction followed in the leading mines, I will commence with the so-called "River Washings," where, in their original simplicity, can be seen the methods of work and the simple machinery long since discarded in the large centres (Fig. 1). These drifts or "river-washings" present an interesting phase of diamond industry. The work is carried out in the crude fashion of early diamond discovery, every man working on his own little claim, assisted by a few natives, and employing primitive machinery. The chief centre of the river washings is at Klipdam No. 2, about 30 miles to the north-west of Kimberley. The road to Klipdam No. 2 involves a journey of about a dozen miles in one of the old African coaches now becoming obsolete through the spread of railways. Road there is none—only a track across the veldt made by countless teams of oxen and mules.

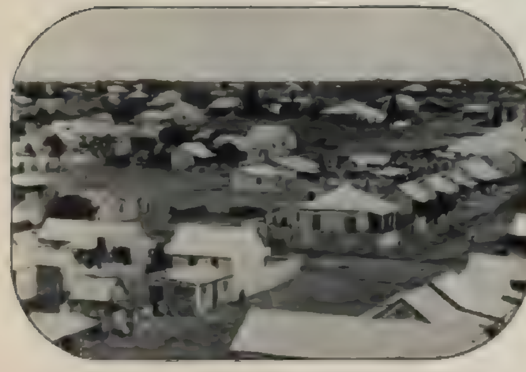
Diamonds from the "river washings" are of all kinds, as if every mine in the neighbourhood contributed. The samples are much rolled and etched, and contain a fair proportion of stones of very good quality, as if only the better and larger stones had survived the ordeal of knocking about.

Diamonds from the drift fetch about 40 per cent. more than those

* Of the photographs illustrating this lecture, Nos. 4 and 7 are from plans lent by Mr. Gardner Williams, and Nos. 3, 5, 6, 8, 9, 12, 13 and 18 are copies of photographs purchased at Kimberley. The remaining twenty were photographed by myself.

1.—Alluvial Diamond Washing

2.—Market Square, Kimberley



3.—Suburbs of Kimberley.

4.—Plan of the Diamond Mines

n Kimberley: taking the yield of the Kimberley and De Beers es as worth, all round, large and small, 26*s.* 6*d.* a carat, the drift nonds are worth 40*s.*

The town of Kimberley is a remarkable instance of rapid growth g. 2). It has an excellent club and one of the best public aries in South Africa. Parts of the town, affectionately called e camp" by the older inhabitants, are still in the galvanised iron tin shanty" stage (Fig. 3), and the general appearance is unlovely l depressing. Reunert reckons that over a million trees have been led to supply timber for the mines, and the whole country within adius of 100 miles has been denuded of wood, with most injurious ects to the climate. The extreme dryness of the air, and the ence of trees to break the force of the wind and temper the heat the sun, probably account for the dust storms so frequent in nmer. The temperature in the day frequently rises to 100° in the de, but in so dry a climate this is not unpleasant, and I felt less pressed than I did in London the previous September. Moreover, Kimberley, owing to the high altitude, the nights are always l.

The five noted diamond mines are all contained in a circle miles in diameter (Fig. 4). The mines are irregularly shaped and or oval pipes, extending vertically downwards to an unknown pth, retaining about the same diameter throughout. They are d to be volcanic necks (Fig. 5), filled from below with a hetero-eous mixture of fragments of the surrounding rocks, and of ler rocks such as granite, mingled and cemented with a bluish oured hard clayey mass, in which famous blue the diamonds are lden.

The breccia filling the pipes, usually called "blue ground," is a llection of fragments of shale, eruptive rocks, boulders, and crystals many kinds of minerals.

The Kimberley mine for the first 70 or 80 feet is filled with what called "yellow ground," and below that with "blue ground." is superposed yellow on blue is common to all the mines. The e is the unaltered ground, and owes its colour chiefly to the ence of lower oxides of iron. When atmospheric influences have ess to the iron it becomes peroxidised, and the ground assumes a llow colour. The thickness of yellow earth in the mines is there-e a measure of the depth of penetration of air and moisture. The our does not affect the yield of diamonds.

The diamantiferous clay or blue ground shows no signs of passing ough great heat, as the fragments in the breccia are not fused at edges. The eruptive force was probably steam or water-gas, ing under great pressure but at no high temperature. According Mr. Dunn, in the Kimberley mine, at a depth of 120 feet, several all fresh-water shells were discovered in what appeared to be disturbed material.

Let me cite a description of a visit to Kimberley in 1872, by

Mr. Paterson, taken from a paper read to the Geologists' Association, which gives a graphic picture of the early days of the Kimberley mine:—

"The New Rush diggings (as the Kimberley Mine was first called) are all going forward in an oval space enclosed around by the trap dyke, of which the larger diameter is about 1000 feet, while the shorter is not more than 700 feet in length. Here all the claims of 31 feet square each are marked out with roadways about 12 feet in width, occurring every 60 feet. Upon these roadways, beside a short pole fixed into the roadway, sits the owner of the claim with watchful eye upon the Kaffir diggers below, who fill, and hoist by means of a pulley fixed to the pole above, bucketful after bucketful of the picked marl stuff in which the diamonds occur."

Soon came the difficulty how to continue working the host of separate claims without infringements. A system of rope haulage was then adopted. This mode of haulage continued in vogue during the whole of 1873, and if the appearance of the mine was less picturesque than when roadways existed, it was, by moonlight particularly, a weird and beautiful sight.

But the mine was now threatened in two other quarters. The removal of the blue ground undermined the support from the walls of the pipe, and frequent falls of reef occurred, not only burying valuable claims but endangering the lives of workers below (Fig. 6). Moreover, as the workings deepened, water made its appearance, necessitating pumping.

It soon became evident that open workings were doomed, and by degrees the present system of underground working was devised.

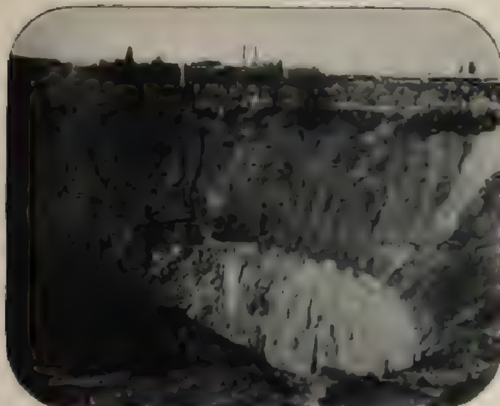
During this time of perplexity, individual miners who might have managed one or two claims near the surface could not continue work in the face of harassing difficulties and heavy expenses. Thus the claims gradually changed hands until the mine became the property first of a comparatively small number of capitalists, then of a smaller number of limited liability companies, until the whole of the mines have practically become the property of the "De Beers Consolidated Mines, Limited."

The areas of the mines are:—

Kimberley	33 acres.
De Beers	22 "
Dutoitspan	45 "
Bulfontein	36 "

The contents of the several pipes are not absolutely identical. The diamonds from each pipe differ in character, showing that the upflow was not simultaneous from one large reservoir below but was the result of several independent eruptions. Even in the same mine there are visible traces of more than one eruption.

The blue ground varies in its yield of diamonds in different mines.



5.—Kimberley Mine—Volcanic Neck.

6.—Kimberley Mine in 1872.

7.—Section of Kimberley Mine.

8.—De Beers Mine.—Underground Workings.

9.—De Beers Mine.—Underground Workings.

10.—The Depositing Floors.

but is pretty constant in the same mine. In 1890, the yield per load of blue ground was—

From the	Kimberley mine	from 1·25 to 1·5	carat.
„	De Beers mine	„ 1·20 „ 1·33	„
„	Dutoitspan mine	„ 0·17 „ 0·5	„
„	Bulfontein mine	„ 0·5 „ 0·33	„

In the face of constant developments I can only describe the system in use at the time of my visit. Shafts are sunk in the solid rock at a sufficient distance from the pipe to be safe against reef movements in the open mine (Fig. 7). Tunnels are driven from this shaft at different levels, about 120 feet apart, to cross the mine from west to east. These tunnels are connected by two others running north and south, one near the west side of the mine and one midway between it and the east margin of the mine. From the east and west tunnels offsets are driven to the surrounding rock. When near the rock, the offsets widen into galleries, these in turn being stoped on the sides until they meet, and upwards until they break through the blue ground. The fallen reef with which the upper part of the mine is filled sinks and partially fills the open space. The workmen then stand on the fallen reef and drill the blue ground overhead, and as the roof is blasted back the *débris* follows. When stoping between two tunnels the blue is stoped up to the *débris* about midway between the two tunnels. The upper levels are worked back in advance of the lower levels, and the works assume the shape of irregular terraces. The main levels are from 90 to 120 feet apart, with intermediate levels every 30 feet. Hoisting is done from only one level at a time through the same shaft. By this ingenious method of mining every portion of blue ground is excavated and raised to the surface, the rubbish on the top gradually sinking and taking its place.

The scene below ground in the labyrinth of galleries is bewildering in its complexity, and very unlike the popular notion of a diamond mine (Figs. 8, 9). All below is dirt, mud, grime; half naked men, black as ebony, muscular as athletes, dripping with perspiration, are seen in every direction, hammering, picking, shovelling, wheeling the trucks to and fro, keeping up a weird chant which rises in force and rhythm when a titanic task calls for excessive muscular strain. The whole scene is more suggestive of a coal mine than a diamond mine, and all this mighty organisation, this strenuous expenditure of energy, this costly machinery, this ceaseless toil of skilled and black labour, goes on day and night, just to win a few stones wherewith to deck my lady's finger!

Owing to the refractory character of blue ground fresh from the mines, it has to be exposed to atmospheric influences before it will pulverise under the action of water and mechanical treatment. It is brought to the surface and spread on the floors (Fig. 10). Soon the heat of the sun and moisture produce a wonderful effect. Boulders, hard as ordinary sandstone when fresh from the mine, commence to

crumble. At this stage the treatment of the diamonds assumes more the nature of farming than mining. To assist pulverisation by exposing the larger pieces to atmospheric influences, the ground is frequently harrowed and occasionally watered. The length of time necessary for crumbling the ground preparatory to washing, depends on the season of the year and the amount of rain. The longer the ground remains exposed the better it is for washing. When the process is complete the softened friable blue clay is again loaded into trucks and taken to the washing machinery, where it is agitated with water and forced through a series of revolving cylinders perforated with holes about an inch in diameter; incorrigible lumps that will not pass the cylinders are again subjected either to the weathering process or passed between crushing rollers.

The fine ground which has passed through the holes in the cylinder, together with a plentiful current of water, flows into the washing pans (Fig. 11). These pans are of iron, 14 feet in diameter, furnished with ten arms each having six or seven teeth. The teeth are set to form a spiral, so that when the arms revolve the teeth carry the heavy deposit to the outer rim of the pan, while the lighter material passes towards the centre and is carried from the pan by the flow of water. The heavy deposit contains the diamonds. It remains on the bottom of the pan and near its outer rim. This deposit is drawn off every twelve hours by means of a broad slot in the bottom of the pan. The average quantity of blue ground passed through each pan is from 400 to 450 loads in ten hours. The deposit left in each pan after putting through the above number of loads amounts to three or four loads, which go to the pulsator for further concentration.

The pulsator (Fig. 12) is an ingeniously designed, somewhat complicated machine for dealing with the diamantiferous gravel already reduced one hundred times from the blue ground; the pulsator still further concentrating it till the stones can be picked out by hand. The value of the diamonds in a load of original blue ground is about 30*s.*, the gravel sent to the pulsator from the pans, reduced a hundredfold, is worth 150*l.* a load.

The sorting room in the pulsator house is long, narrow and well lighted. Here the rich gravel is brought in wet, a sieveful at a time, and is dumped in a heap on tables covered with iron plates. The tables at one end take the coarsest lumps, next comes the gravel which passed the ⅜-inch holes, then the next in order, and so on. The first sorting, where the danger of robbery is greatest, is done by thoroughly trustworthy white men. Sweeping the heap of gravel to the right, the sorter scrapes a little of it to the centre of the table by means of a flat piece of sheet zinc (Fig. 13). With this tool he rapidly surveys the grains, seizes the diamonds, and puts them into a little tin box in front of him. The stuff is then swept off to the left, and another lot taken, and so on, till the sieveful of gravel is exhausted and another brought in.

11.—De Beers Washing and Concentrating Machinery

12.—The Pulsator.

13.—Sorting Gravel for Diamonds.

14.—De Beers Diamond Office.—Valuators' Table.

The diamond has a peculiar lustre, impossible to mistake. On the sorting table the stones look like clear pieces of gum arabic, but with an intrinsic lustre which makes a conspicuous shine among the other stones.

Watching the white men in the sorting room is an experience but tame compared to the excitement of taking a sorter's place at the big diamond table and disinterring from the gravel diamonds usually described as the finest and biggest found for many a day. The interest, however, abates when the amateur sorter is told that the jewels may not be carried away as mementos!

Sometimes as many as 8000 carats of diamonds are separated in one day, representing about 10,000*l.* in value.

Diamonds occur in all shades, from deep yellow to pure white and jet black, from deep brown to light cinnamon; they are also green, blue, pink, yellow, orange and opaque.

From the pulsator sorting room the stones are taken to the Diamond Office to be cleaned in acids and sorted into classes by the valuators, according to colour and purity. It is a sight for Aladdin to see the valuators at work in the strong-room of the De Beers Company at Kimberley (Fig. 14). The tables are literally heaped with stones won from the rough blue ground—stones of all sizes, purified, flashing and of inestimable price; stones that will be coveted by men and women all the world over; and last, but not least, stones that are probably destined to largely influence the development and history of a whole huge continent.

When the diamantiferous gravel has been washed down to a point at which the stones can be picked out by hand, a good plan for separating them is by their specific gravities. The following table gives the specific gravities of the minerals found on the sorting tables. I have also included the specific gravities of two useful liquids.

This table shows that if I throw the whole mixture of minerals into methylene iodide the hornblende and all above that mineral will rise to the surface; while the diamond and all minerals below will sink to the bottom. If I take these heavy minerals, and throw them into thallium lead acetate, they will all sink except the diamond, which floats and can be skimmed off.

	Specific Gravity.		Specific Gravity.
Hard graphite	2·5	Thallium lead acetate	3·6
Quartzite and granite	2·6	Garnet	3·7
Beryl	2·7	Corundum	3·9
Mica	2·8	Zircon	4·4
Hornblende	3·0	Barytes	4·5
Methylene iodide	3·3	Chrome and titanic iron ore	4·7
Diamond	3·5	Magnetite	5·0

In illustration, I have arranged an experiment. In front of the lantern is a cell containing a dense liquid; when I throw into it several minerals of different specific gravities, some sink whilst

others swim, and these swimmers can easily be skimmed from the surface.

With gems like diamonds, where infinite riches are concentrated in so small a bulk, it is not surprising that safeguards against robbery are elaborate. The Illicit Diamond Buying (I.D.B.) laws are stringent, and the searching, rendered easy by the "compounding" of the natives, is of a drastic character. In fact, it is very difficult for a native employé to steal diamonds; even were he to succeed, it would be almost impossible to dispose of them, as a potential buyer would prefer to secure the safe reward for detecting a theft rather than run the serious risk of doing convict work on the Cape Town Breakwater for a couple of years. Before the passing of the "Diamond Trade Act" the value of stolen diamonds reached nearly one million sterling per annum.

One great safeguard against robbery is the "compound" system of looking after the natives (Fig. 15). A "compound" is a large square, about 20 acres in extent, surrounded by rows of one-story buildings of corrugated iron. These are divided into rooms each holding about twenty natives. A high iron fence is erected around the compound, 10 feet from the buildings. Within the enclosure is a store where the necessaries of life are supplied to the natives at a reduced price, and wood and water free of charge. In the middle is a large swimming-bath with fresh water running through it. The rest of the space is devoted to games, dances, concerts, and any other amusement the native mind can desire. In case of accident or illness there is a well-appointed hospital where the sick are tended. Medical supervision, nurses and food are supplied free by the Company.

As a rule the better class of natives—the Zulus, Matabeles, Basutos, Bechuanas—when well treated, are honest and loyal.

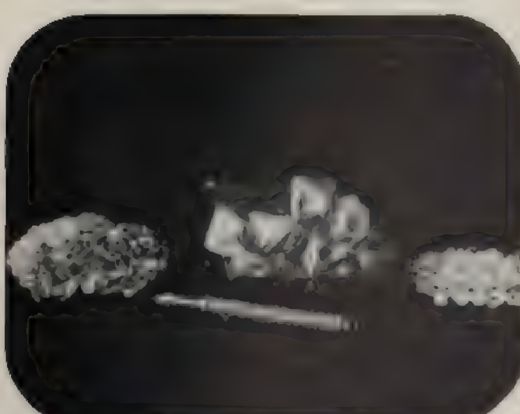
In the compound are to be seen representatives of nearly all the picked types of African tribes (Fig. 16). Each tribe keeps to itself, and to go round the buildings skirting the compound is an admirable object lesson in ethnology. At one point is a group of Zulus; next we come to Fingoes; then Basutos; beyond come Matabele (Fig. 17), Bechuanas, Pondos, Swazis, and other less-known tribes, each forming a distinct group, or wandering around making friendly calls. We went one afternoon to the De Beers compound when most of the natives were assembled, and having a camera with me I was naturally glad to get as many photographs as I could. I have to thank Captain Dallas, Mr. Moses, and Mr. Mandy, the Superintendents of the respective compounds, who speak all the dialects fluently, for their kindness in showing us round and improvising dances and concerts (Fig. 18), for the benefit of my camera.

The clothing in the compound is diverse and original (Fig. 19). Some of the men are great dandies, whilst others think that in so hot a climate a bright coloured pocket-handkerchief or "a pair of spectacles and a smile" is as great a compliance with the requirements of civilisation as can be expected.

15.—De Beers Compound.

16.—De Beers Compound.

19.—De Beers Compound.



20—Groups of Diamond Crystals

So distinctive are the characters in diamonds from each mine that an experienced buyer at once tells the locality of any particular parcel of stones. De Beers and Kimberley mines are distinguished by large yellowish crystals. Dutoitspan yields many coloured stones, while Bulfontein—half a mile off—produces small white stones, occasionally speckled and flawed, but rarely coloured. Diamonds from the Wesselton mine are nearly all irregular in shape; a perfect crystal is rare, and most of the stones are white, few yellow. Diamonds from the Leicester mine have a frosted, etched appearance; they are white, the crystallisation irregular ("cross-grained"), and they are very hard. The newly discovered "Newlands" mines in Griqualand West are remarkable for the whiteness of their diamonds and for their many perfect octahedral crystals. Jagersfontein stones in the Orange Free State, take the prize for purity of colour and brilliancy, and they show that so-called "steely" lustre characteristic of old Indian gems. Stones from Jagersfontein are worth nearly double those from Kimberley and De Beers.

Monster diamonds are not so uncommon as is generally supposed. Diamonds weighing over an ounce (151·5 carats) are not infrequent at Kimberley, and there would be no difficulty in getting together a hundred of them. Not long ago, in one parcel of stones at the office of Wernher, Beit and Co., I saw eight perfect crystals, each over an ounce, and one that weighed two ounces (Fig. 20). The largest known diamond—a true mountain of light—weighs 970 carats, over half a pound. It was found four years ago at Jagersfontein. It is perfection in colour, but has a small black spot in the centre. Diamonds smaller than a small fraction of a grain elude the sorters and are lost. A microscopic examination of blue ground from Kimberley, after treatment with appropriate solvents, shows the presence of microscopic diamonds, white, coloured and black, also of boart and carbonado.

From two to three million carats of diamonds are turned out of the Kimberley mines in a year, and as five million carats go to the ton, this represents half a ton of diamonds. To the end of 1892, ten tons of diamonds had come from these mines, valued at 60,000,000*l.* sterling. This mass of blazing diamonds could be accommodated in a box five feet square and six feet high.

The diamond is a luxury for which there is only a limited demand. From 4 to 4½ millions sterling is as much as is spent annually in diamonds; if production is not regulated by demand, there will be over-production, and the trade will suffer. By regulating the output, since the consolidation in 1888 the directors have succeeded in maintaining prices.

Outside companies and individuals collect diamonds to the value of about a million annually.

Intermediate between soft carbon and diamond come the graphites. The name graphite is given to a variety of carbon, generally crystalline, which in an oxidising mixture of chlorate of potassium and

nitric acid forms graphitic acid easy to recognise. Graphites are of varying densities, from 2·0 to 3·0, and generally of crystalline aspect. Graphite and diamond pass insensibly into one another. Hard graphite and soft diamond are near the same specific gravity. The difference appears to be one of pressure at the time of formation.

Some forms of graphite exhibit a remarkable property, by which it is possible to ascertain approximately the temperature at which they were formed, or to which they have subsequently been exposed. Graphites are divided into "sprouting" and "non-sprouting." When obtained by simple elevation of temperature in the arc or the electric furnace they do not sprout; but when they are formed by dissolving carbon in a metal at a high temperature and then allowing the graphite to separate out on cooling, the sprouting variety is formed. One of the best varieties is that which can be separated from platinum in ebullition in a carbon crucible. The phenomenon of sprouting is easily shown. I place a few grains in a test-tube and heat it to about 170° C., when, as you see, it increases enormously in bulk and fills the tube with a light form of amorphous carbon.

The resistance of a graphite to oxidising agents is greater the higher the temperature to which it has previously been exposed. Graphites which are easily attacked by a mixture of fuming nitric acid and potassium chlorate are rendered more resistant by strong heat in the electric furnace.

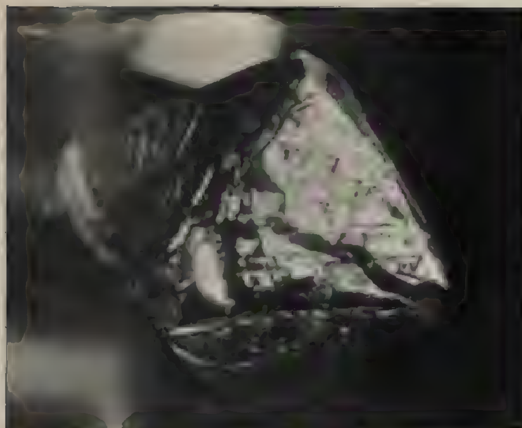
I will now briefly survey the chief chemical and physical characteristics of the diamond, showing you by the way a few experiments that bear upon the subject.

When heated in air or oxygen to a temperature varying from 760° to 875° C. according to its hardness, the diamond burns with production of carbonic acid. It leaves an extremely light ash, sometimes retaining the shape of the crystal, consisting of iron, lime, magnesia, silica, and titanium. In boart and carbonado the amount of ash sometimes rises to 4 per cent., but in clear crystallised diamonds it is seldom higher than 0·05 per cent. By far the largest constituent of the ash is iron.

The following table shows the temperatures of combustion in oxygen of different kinds of carbon:—

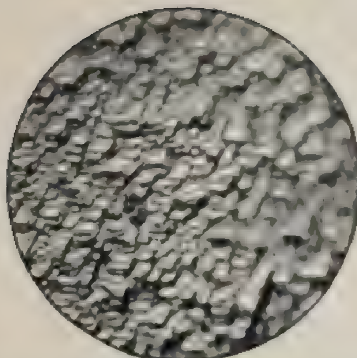
	°C.
Condensed vapour of carbon	650
Carbon from sugar, heated in an electrical furnace	660
Artificial graphites, generally	660
Graphite from ordinary cast iron	670
Carbon from blue ground, of an ochrey colour	690
" " very hard and black	710
Diamond, soft Brazilian	760
" hard Kimberley	780
Boart from Brazil	790
" from Kimberley	790
" very hard, impossible to cut	900

At the risk of repeating an experiment shown so well at this

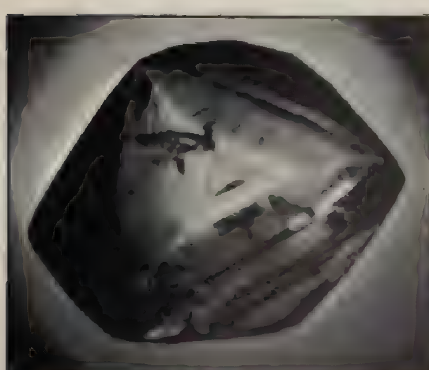


21.—Crystal of Diamond, showing Triangular Markings.

22.—Triangular Markings on a Crystal of Diamond (× 100).



23.—Markings developed on smooth surface of Diamond by combustion.



24.—Crystal of Diamond showing curved edges.

le by Professor Dewar, I will heat a diamond to a high tempera-
in the oxyhydrogen blowpipe and then suddenly throw it in
essel of liquid oxygen. Notice the brilliant light of its combus-
. I want you more especially to observe the white opaque
osit forming in the liquid oxygen. This deposit is solid carbonic
l produced by the combustion of the carbon. I will lead it
ough baryta water, and you will see a white precipitate of barium
bonate. With a little more care than is possible in a lecture I
ld perform this experiment quantitatively, leading the carbonic
l and oxygen, as they assume the gaseous state, through baryta
er, weighing the carbonate so formed, and showing that one
mme of diamond would yield 3·666 grammes of carbonic acid—
theoretical proportion for pure carbon.

Some crystals of diamonds have their surfaces beautifully marked
h equilateral triangles, interlaced and of varying sizes (Fig. 21).
der the microscope these markings appear as shallow depressions
rply cut out of the surrounding surface (Fig. 22), and these
ressions were supposed by Gustav Rose to indicate the probability
t the diamonds at some previous time had been exposed to
ipient combustion. Rose also noted that striations appeared on
surfaces of diamonds burnt before the blowpipe. This experi-
nt I have repeated on a clear smooth diamond, and have satisfied
self that during combustion in the field of a microscope, before
blowpipe, the surface becomes etched with markings very
ferent in character from those naturally inscribed on crystals.
e artificial striæ are cubical and closer massed, looking as if
diamond during combustion had been dissected into rectangular
tes (Fig. 23), while the markings natural to crystals appear as if
duced by the crystallising force as they were being built up.

I exhibit on a diagram a form of graphite from the Kimberley
e ground (reproduced from M. Moissan's work) which in its
stalline appearance strangely resembles the surface of a diamond
ose internal structure has been partially dissected and barred by
mbustion. It looks as if this piece of graphite was ready to
arate out of its solvent as diamond, but owing to some insufficient
tor it retained its graphitic form.

The specific gravity of the diamond is from 3·514 to 3·518.
r comparison, I give in tabular form the specific gravities of the
ferent varieties of carbon :—

Amorphous carbon	1·45 to 1·70
Graphite	2·11 „ 3·0
Hard gas coke	2·356
Boart	3·47 „ 3·49
Carbonado	3·50
Diamond	3·514 „ 3·518

The diamond belongs to the isometric system of crystallography.
frequently occurs with curved faces and edges (Fig. 24). Twin

crystals (macles) are not uncommon. Having no double refraction it should not act on polarised light. But, as is well known, if a transparent body which does not so act is submitted to strain of an irregular character it becomes doubly refracting, and in the polariscope reveals the existence of the strain by brilliant colours arranged in a more or less defined pattern according to the state of tension in which the crystal exists. Under polarised light I have examined many hundred diamond crystals, and with few exceptions all show the presence of internal tension. On rotating the polariser, the black cross, which is most frequently seen, revolves round a particular point in the inside of the crystal, and on examining this point with a high power, we see sometimes a slight flaw, more rarely a minute cavity. The cavity is filled with gas at an enormous pressure, and the strain is set up in the stone by the effort of the gas to escape.

It is not uncommon for a diamond to explode soon after it reaches the surface, and some have been known to burst in the pockets of the miners or when held in the warm hand. Large crystals are more liable to burst than smaller pieces. Valuable stones have been destroyed in this way, and it is whispered that cunning dealers are not averse to allowing responsible clients to handle or carry in their warm pockets large crystals fresh from the mine. By way of safeguard against explosion, some dealers imbed large diamonds in raw potato to insure safe transit to England.

I will project some diamonds on the screen by means of the polarising microscope, and you will see by the colours how great is the strain to which some of them are exposed.

In the substance of many diamonds we find enclosed black uncrystallised particles of graphite. There also occur what may be considered intermediate forms between the well-crystallised diamond and graphite. These are "boart" and "carbonado." Boart is an imperfectly crystallised diamond, having no clear portions, and therefore useless for gems. Boart is frequently found in spherical globules, and may be of all colours. It is so hard that it is used in rock-drilling, and when crushed it is employed for cutting and polishing other stones. Carbonado is the Brazilian term for a still less perfectly crystallised form of carbon. It is equally hard, and occurs in porous masses, and in massive black pebbles, sometimes weighing a couple or more ounces.

Diamonds vary considerably in hardness, and even different parts of the same crystal are decidedly different in their resistance to cutting and grinding. The famous Koh-i-noor, when cut into its present form, showed a notable variation in hardness. In cutting one of the facets near a yellow flaw, the crystal became harder and harder the further it was cut into, until, after working the mill for six hours at the usual speed of 2400 revolutions a minute, little impression was made. The speed was accordingly increased to more

than 3000, when the work slowly proceeded. Other portions of the stone were found to be comparatively soft, and became harder as the outside was cut away.

Beautifully white diamonds have been found at Inverel, New South Wales, and from the rich yield of the mine and the white colour of the stones, great things were expected. A parcel of many hundred carats came to England, when it was found they were so hard as to be practically unworkable as gems, and I believe they were ultimately sold for rock-boring purposes.

I will illustrate the intense hardness of the diamond by an experiment. I place a diamond on the flattened apex of a conical block of steel, and on the diamond I bring down a second cone of steel. With the electric lantern I will project an image of the diamond and steel faces on the screen, and force them together by hydraulic power. You see I can squeeze the stone right into the steel blocks without injuring it in the slightest degree.

But it is not the hardness of the diamond so much as its optical qualities that make it so highly prized. It is one of the most refracting substances in nature, and it also has the highest reflecting properties. In the cutting of diamonds advantage is taken of these qualities. When cut as a brilliant the facets on the lower side are inclined so that light falls on them at an angle of 24° 13′, at which angle all the incident light is totally reflected. A well cut diamond should appear opaque by transmitted light except at a small spot in the middle where the table and culet are opposite. All the light falling on the front of the stone is reflected from the facets, and the light passing into the diamond is reflected from the interior surfaces and refracted into colours when it passes out into the air, giving rise to the lightnings and coruscations for which the diamond is supreme above all other gems.

I hold some of Mr. Streeter's magnificent diamonds in the electric light, and by transmitted light you will see they are black, while by reflected light they fill the room with radiance and colour.

The following table gives the refractive indices of diamonds and other bodies:—

Refractive Indices for the D Line.

Chromate of lead ..	2·50–2·97	Beryl	1·60
Diamond	2·47–2·75	Emerald	1·59
Phosphorus	2·22	Flint glass	1·58
Sulphur	2·12	Quartz	1·55
Ruby	1·78	Canada balsam	1·53
Thallium glass ..	1·75	Crown glass	1·53
Iceland spar	1·65	Fluor-spar	1·44
Topaz	1·61	Ice	1·31

According to Dr. Gladstone, the specific refractive energy—

$$\frac{\mu - 1}{d},$$

will be for the D line 0·404, and the refraction equivalent,—

$$P\frac{\mu - 1}{d},$$

will be 4·82.

After exposure for some time to the sun many diamonds glow in a dark room. Some diamonds are fluorescent, appearing milky in sunlight. In a vacuum, exposed to a high-tension current of electricity, diamonds phosphoresce of different colours, most South African diamonds shining with a bluish light. Diamonds from other localities emit bright blue, apricot, pale blue, red, yellowish-green, orange, and pale green light. The most phosphorescent diamonds are those which are fluorescent in the sun. One beautiful green diamond in my collection, when phosphorescing in a good vacuum, gives almost as much light as a candle, and you can easily read by its rays. The light is pale green, tending to white.

I will now draw your attention to a strange property of the diamond, which at first sight might seem to argue against the great permanence and unalterability of this stone. It has been ascertained that the cause of phosphorescence is in some way connected with the hammering of the gaseous molecules, violently driven from the negative pole, on to the surface of the body under examination, and so great is the energy of the bombardment, that impinging on a piece of platinum or even iridium, the metal will actually melt. When the diamond is thus bombarded in a radiant matter tube the result is startling. It not only phosphoresces but assumes a brown colour, and when the action is long continued becomes almost black.

I will project a diamond on the screen and bombard it with radiant matter before your eyes. I do not like to anticipate a failure, but here I am entirely at the mercy of my diamond. I cannot rehearse this experiment beforehand, and it may happen that the diamond I have selected will not blacken in reasonable time. Some visibly darken in a few minutes, while others, more leisurely in their ways, require an hour.

This blackening is only superficial, but no ordinary means of cleaning will remove the discoloration. Ordinary oxidising reagents have little or no effect in restoring the colour. The black stain on the diamond is due to a form of graphite which is very resistant to oxidation. It is not necessary to expose the diamond in a vacuum to electrical excitement in order to produce this change.

I have already signified that there are various degrees of refractoriness to chemical reagents among the different forms of graphite. Some dissolve in strong nitric acid; other forms of graphite require a mixture of highly concentrated nitric acid and potassium chlorate to attack them, and even with this intensely powerful agent some graphites resist longer than others. M. Moissan has shown that the power of resistance to nitric acid and potassium chlorate is in proportion to the temperature at which the graphite

was formed, and with tolerable certainty we can estimate this temperature by the resistance of the specimen of graphite to this reagent.

The superficial dark coating on a diamond after exposure to molecular bombardment I have proved to be graphite,* and M. Moissan † has shown that this graphite, on account of its great resistance to oxidising reagents, cannot have been formed at a lower temperature than 3600° C.

It is therefore manifest that the bombarding molecules, carrying with them an electric charge, and striking the diamond with enormous velocity, raise the superficial layer to the temperature of the electric arc, and turn it into graphite, whilst the mass of diamond and its conductivity to heat are sufficient to keep down the general temperature to such a point that the tube appears scarcely more than warm to the touch.

A similar action occurs with silver, the superficial layers of which can be raised to a red heat without the whole mass becoming more than warm.‡

This conversion of diamond into graphite is, I believe, a pure effect of heat. In 1880 § Professor Dewar in this theatre placed a crystal of diamond in a carbon tube through which a current of hydrogen was maintained. The tube was heated from the outside by an electric arc, and in a few minutes the diamond was converted into graphite. I will now show you that a clear crystal of diamond, heated in the electric arc (temperature 3600° C.), is converted into graphite, and this graphite is most refractory.

The diamond is remarkable in another respect. It is extremely transparent to the Röntgen rays, whereas highly refracting glass, used in imitation diamonds, is almost perfectly opaque to the rays (Fig. 25). I exposed over a photographic plate to the X rays for a few seconds the large Delhi diamond, of a fine pink colour, weighing 31½ carats, a black diamond weighing 23 carats, together with an imitation in glass of the pink diamond lent me by Mr. Streeter; also a flat triangular crystal of diamond of pure water, and a piece of glass of the same shape and size. On development, the impression where the diamond obscured the rays was found to be strong, showing that most rays passed through, while the glass was practically opaque. By this means imitation diamonds and some other false gems can readily be detected and distinguished from the true gems. It would take a good observer to distinguish my pure triangular diamond from the adjacent glass imitation.

Speculations as to the probable origin of the diamond have been greatly forwarded by patient research, and particularly by improved

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* 'Chemical News,' vol. lxxiv. p. 39, July 1896.
† 'Comptes Rendus,' cxxiv. p. 653.
‡ Proc. Roy. Soc. vol. l. p. 99, June 1891.
§ 'Proceedings of the Royal Institution,' Jan. 16, 1880.

means of obtaining high temperatures. Thanks to the success of Professor Moissan, whose name will always be associated with the artificial production of diamonds, we are able to-day to manufacture diamonds in our laboratories—minutely microscopic, it is true—all the same veritable diamonds, with crystalline form and appearance, colour, hardness, and action on light the same as the natural gem.

Until recent years carbon was considered absolutely non-volatile and infusible; but the enormous temperatures at the disposal of experimentalists—by the introduction of electricity—show that, instead of breaking rules, carbon obeys the same laws that govern other bodies. It volatilises at the ordinary pressure at a temperature of about 3600° C., and passes from the solid to the gaseous state without liquefying. It has been found that other bodies which volatilise without liquefying at the ordinary pressure will easily liquefy if pressure is added to temperature. Thus, arsenic liquefies under the action of heat if the pressure is increased; it naturally follows that if along with the requisite temperature sufficient pressure is applied, liquefaction of carbon will be likely to take place, when on cooling it will crystallise. But carbon at high temperatures is a most energetic chemical agent, and if it can get hold of oxygen from the atmosphere or any compound containing it, it will oxidise and fly off in the form of carbonic acid. Heat and pressure therefore are of no avail unless the carbon can be kept inert.

It has long been known that iron when melted dissolves carbon, and on cooling liberates it in the form of graphite. Moissan discovered that several other metals have similar properties, especially silver; but iron is the best solvent for carbon. The quantity of carbon entering into solution increases with the temperature, and on cooling in ordinary circumstances it is largely deposited as crystalline graphite.

Professor Dewar has made a calculation as to the Critical Pressure of carbon—that is, the lowest pressure at which carbon can be got to assume the liquid state at its critical temperature, that is the highest temperature at which liquefaction is possible. He starts from the vaporising or boiling point of carbon, which, from the experiments of Violle and others on the electric arc, is about 3600° C., or 3874° Absolute. The critical point of a substance on the average is 1·5 times its absolute boiling point. Therefore the critical point of carbon is 5811° Ab., or, say, 5800° Ab. But the absolute critical temperature divided by the critical pressure is for elements never less than 2·5. Then—

$$\frac{5800^\circ \text{ A.}}{\text{PCr}} = 2\cdot 5, \text{ or PCr} = \frac{5800^\circ \text{ A.}}{2\cdot 5}, \text{ or 2320 atmospheres.}$$

The result is that the critical pressure of carbon is about 2300 atmospheres, or say 15 tons on the square inch. The highest critical pressure recorded is that of water, amounting to 195 atmospheres,

and the lowest that of hydrogen, about 20 atmospheres. In other words, the critical pressure of water is ten times that of hydrogen, and the critical pressure of carbon is ten times that of water.

Now 15 tons on the square inch is not a difficult pressure to obtain in a closed vessel. In their researches on the gases from fired gunpowder and cordite, Sir Frederick Abel and Sir Andrew Noble obtained in closed steel cylinders pressures as great as 95 tons to the square inch, and temperatures as high as 4000° C. Here, then, if the observations are correct, we have sufficient temperature and enough pressure to liquefy carbon; and if the temperature could only be allowed to act for a sufficient time on the carbon there is little doubt that the artificial formation of diamonds would soon pass from the microscopic stage to a scale more likely to satisfy the requirements of science, industry and personal decoration.

I now proceed to manufacture a diamond before your eyes—don't think I yet have a talisman that will make me rich beyond the dreams of avarice! Hitherto the results have been very microscopic and are chiefly of scientific interest in showing us Nature's workshop, and how we may ultimately hope to vie with her in the manufacture of diamonds. Unfortunately the operations of separating the diamond from the iron and other bodies with which it is associated are somewhat prolonged—nearly a fortnight being required to detach it from the iron, graphite and other matters in which it is embedded. I can, however, show the different stages of the operations, and project on the screen diamonds made in this manner.

In Paris recently I saw the operation carried out by M. Moissan, the discoverer of this method of making carbon separate out in the transparent crystalline form, and I can show you the operations straight as it were from the inventor's laboratory. I am also indebted to the Directors of the Notting Hill Electric Lighting Co., and to the general manager, Mr. Schultz, for enabling me to perform several operations at their central station, where currents of 600 ampères and 100 volts were placed at my disposal.

The first necessity is to select pure iron—free from sulphur, silicon, phosphorus, &c.—and to pack it in a carbon crucible with pure charcoal from sugar. Half a pound of this iron is then put into the body of the electric furnace and a powerful arc formed close above it between carbon poles, utilising a current of 700 ampères at 40 volts pressure. The iron rapidly melts and saturates itself with carbon. After a few minutes' heating to a temperature above 4000° C.—a temperature at which the lime of the furnace melts like wax and volatilises in clouds—the current is stopped, and the dazzling fiery crucible is plunged beneath the surface of cold water, where it is held till it sinks below a red heat. As is well known, iron increases in volume at the moment of passing from the liquid to the solid state. The sudden cooling solidifies the outer layer of iron and holds the inner molten mass in a tight grip. The expansion of the inner liquid on solidifying produces an enormous pressure,

and under the stress of this pressure the dissolved carbon separates out in a transparent, dense, crystalline form—in fact, as diamond.

Now commences the tedious part of the process. The metallic ingot is attacked with hot nitro-hydrochloric acid until no more iron is dissolved. The bulky residue consists chiefly of graphite, together with translucent flakes of a chestnut-coloured carbon, black opaque carbon of a density of from 3·0 to 3·5, and hard as diamonds—black diamonds or carbonado, in fact—and a small portion of transparent colourless diamonds showing crystalline structure. Besides these, there may be carbide of silicon and corundum, arising from impurities in the materials employed.

The residue is first heated for some hours with strong sulphuric acid at the boiling point, with the cautious addition of powdered nitre. It is then well washed and allowed for two days to soak in strong hydrofluoric acid in the cold, then in boiling acid. After this treatment the soft graphite will disappear, and most, if not all, of the silicon compounds will be destroyed. Hot sulphuric acid is again applied to destroy the fluorides, and the residue, well washed, is repeatedly attacked with a mixture of the strongest nitric acid and powdered potassium chlorate, kept warm, but to avoid explosions not above 60° C. This ceremony must be repeated six or eight times, when all the hard graphite will gradually be dissolved, and little else left but graphitic oxide, diamond and the harder carbonado and boart. The residue is fused for an hour in fluorhydrate of fluoride of potassium, then boiled out in water, and again heated in sulphuric acid. The well-washed grains which resist this energetic treatment are dried, carefully deposited on a slide, and examined under the microscope. Along with numerous pieces of black diamond are seen transparent colourless pieces, some amorphous, others with a crystalline appearance, as I have attempted to reproduce in drawings. Although many fragments of crystals occur, it is remarkable that I have never seen a complete crystal. All appear broken up, as if on being liberated from the intense pressure under which they were formed they burst asunder. I have direct evidence of this phenomenon. A very fine piece of artificial diamond, carefully mounted by me on a microscopic slide, exploded during the night and covered my slide with fragments. This bursting paroxysm is not unknown at the Kimberley mines.

On the screen I will project fragments of artificial diamond (Figs. 26, 27), some lent me by Professor Roberts-Austen, others of my own make; while on the wall you will see drawings of diamonds copied from M. Moissan's book on the Electric Furnace. Unfortunately these specimens are all microscopic. The largest artificial diamond, so far, is less than one millimetre across.

Laboratory diamonds burn in the air before the blowpipe to carbonic acid; and in lustre, crystalline form, optical properties, density and hardness they are identical with the natural stone.

Many circumstances point to the conclusion that the diamond

hemist and the diamond of the mine are strangely akin as t. It is conclusively proved that the diamond has not been n *situ* in the blue ground. The diamond genesis must have ace at great depths under enormous pressure. The explosion diamonds on coming to the surface shows extreme tension. amonds are found in fragments and splinters than in perfect ; and it is noteworthy that although many of these splinters gments are derived from the breaking up of a large crystal, no instance have pieces been found which could be fitted . Does not this fact point to the conclusion that the blue is not their true matrix? Nature does not make fragments als. As the edges of the crystals are still sharp and ed, the *locus* of formation cannot have been very distant e present sites. There were probably many sites of crystal- differing in place and time, or we should not see such ive characters in the gems from different mines, nor indeed iamonds from different parts of the same mine.

the great diamond pipes originally came into existence is ult to understand, in the light of the foregoing facts. They y were not burst through in the ordinary manner of volcanic ; the surrounding and enclosing walls show no signs of action, and are not shattered nor broken even when t the "blue ground." These pipes after they were pierced led from below, and the diamonds formed at some previous oo remote to imagine were erupted with a mud volcano, with all kinds of *débris* eroded from the adjacent rocks. ection of flow is seen in the upturned edges of some of the f shale in the walls, although I was unable at great depths any upturning in most parts of the walls of the De Beers

me again refer you to the picture of the section through the ley mine. There are many such pipes in the immediate urhood. It may be that each volcanic pipe is the vent for special laboratory—a laboratory buried at vastly greater than we have reached or are likely to reach—where the ture is comparable with that of the electric furnace, where sure is fiercer than in our puny laboratories and the melting- igher, where no oxygen is present, and where masses of aturated iron have taken centuries, perhaps thousands of cool to the solidifying point. Such being the conditions der is, not that diamonds are found as big as one's fist, but y are not found as big as one's head. The chemist arduously tures infinitesimal diamonds, valueless as ornamental gems; ure, with unlimited temperature, inconceivable pressure and material, to say nothing of measureless time, produces stint the dazzling, radiant, beautiful crystals I am enabled you to-night.

ferric origin of the diamond is corroborated in many ways.

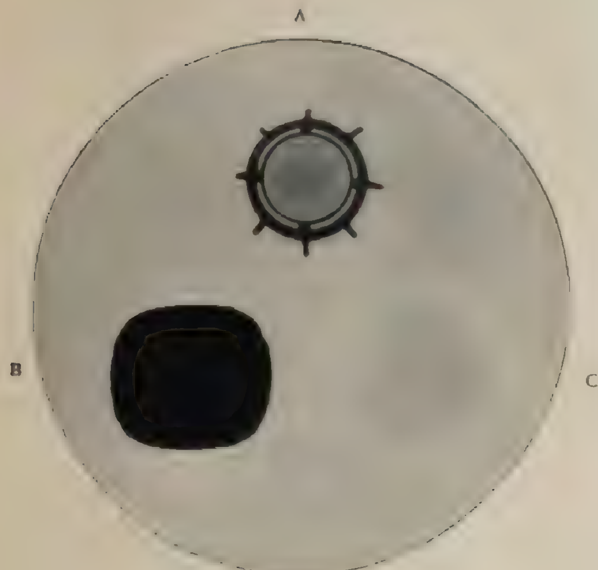
The country round Kimberley is remarkable for its ferruginous character, and iron-saturated soil is popularly regarded as one of the indications of the near presence of diamonds. Certain artificial diamonds present the appearance of an elongated drop. From Kimberley I have with me diamonds which have exactly the appearance of drops of liquid separated in a pasty condition and crystallised on cooling (Fig. 28). At Kimberley and in other parts of the world, diamonds have been found with little appearance of crystallisation, but with rounded forms similar to those which a liquid might assume if kept in the midst of another liquid with which it would not mix. Other drops of liquid carbon retained above their melting-point for sufficient time would coalesce with adjacent drops, and on slow cooling would separate in the form of large perfect crystals. Two drops, joining after incipient crystallisation, would assume the not uncommon form of interpenetrating twin crystals. Illustrations of these forms from Kimberley are here to-night. Other modified circumstances would produce diamonds presenting a confused mass of boarty crystals, rounded and amorphous masses, or a hard black form of carbonado.

Again, diamond crystals are almost invariably perfect on all sides. They show no irregular side or face by which they were attached to a support, as do artificial crystals of chemical salts; another proof that the diamond must have crystallised from a dense liquid.

When raised the diamond is in a state of enormous strain, as I have already shown by means of polarised light. Some diamonds exhibit cavities which the same test proves to contain gas at considerable pressure.

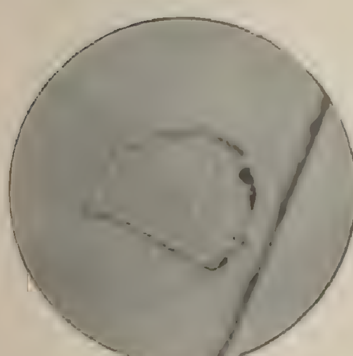
The ash left after burning a diamond invariably contains iron as its chief constituent; and the most common colours of diamonds, when not perfectly pellucid, show various shades of brown and yellow, from the palest "off colour" to almost black. These variations accord with the theory that the diamond has separated from molten iron, and also explain how it happens that stones from different mines, and even from different parts of the same mine, differ from each other. Along with carbon, molten iron dissolves other bodies which possess tinctorial powers. One batch of iron may contain an impurity colouring the stones blue, another lot would tend towards the formation of pink stones, another of green, and so on. Traces of cobalt, nickel, chromium and manganese, metals present in the blue ground, might produce all these colours.

A hypothesis, however, is of little value if it only elucidates one-half of a problem. Let us see how far we can follow out the ferric hypothesis to explain the volcanic pipes. In the first place we must remember these so-called volcanic vents are admittedly not filled with the eruptive rocks, scoriaceous fragments, &c., constituting the ordinary contents of volcanic ducts. At Kimberley the pipes are filled with geological plum pudding of heterogeneous character-agreeing, however, in one particular. The appearance of shale and

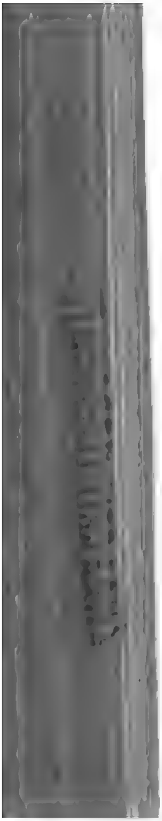


25.—Diamonds in Rontgen Rays.

A.—Black Diamond (In Gold Frame)
B.—Glass Imitation Diamond.
C.—Pink Delhi Diamond.



26.—Artificial Diamond, from Molten Iron.



27.—Artificial Diamond from Molten Iron.

28.—Diamond Crystal in the form of a Drop.

gments of other rocks shows that the mélange has suffered no at heat in its present condition, and that it has been erupted from at depths by the agency of water vapour or some similar gas. w is this to be accounted for?

It must be borne in mind I start with the reasonable supposition t at a sufficient depth * there were masses of molten iron at a at pressure and high temperature, holding carbon in solution, dy to crystallise out on cooling. In illustration I may cite the sses of erupted iron in Greenland. Far back in time the cooling n above caused cracks in superjacent strata through which water † nd its way. On reaching the iron the water would be converted o gas, and this gas would rapidly disintegrate and erode the annels through which it passed, grooving a passage more and re vertical in the endeavour to find the quickest vent to the rface. But steam in the presence of molten or even red-hot iron pidly attacks it, oxidises the metal and liberates large volumes of drogen gas, together with less quantities of hydrocarbons ‡ of kinds—liquid, gaseous and solid. Erosion commenced by steam uld be continued by the other gases, and it would be no difficult k for pipes, large as any found in South Africa, to be scored out this manner. Sir Andrew Noble has shown that when the screw pper of his steel cylinders in which gunpowder explodes under essure is not absolutely perfect, gas finds its way out with a rush overpowering as to score a wide channel in the metal; some of se stoppers and vents are on the table. To illustrate my argu- nt Sir Andrew Noble has been kind enough to try a special periment. Through a cylinder of granite is drilled a hole 0·2 inch meter, the size of a small vent. This is made the stopper of an plosion chamber, in which a quantity of cordite is fired, the es escaping through the granite vent. The pressure is about 00 atmospheres, and the whole time of escape is less than half a ond. Notice the erosion produced by the escaping gases and by heat of friction, which have scored out a channel over half an h diameter and melted the granite along their course. If steel and nite are thus vulnerable at comparatively moderate gaseous pres- e, is it not easy to imagine the destructive upburst of hydrogen and er gas grooving for itself a channel in the diabase and quartzite, ing fragments from resisting rocks, covering the country h débris, and finally at the subsidence of the great rush, filling self-made pipe with a water-borne magma in which rocks,

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* The requisite pressure of fifteen tons on the square inch would exist not y miles beneath the surface of the earth.

† There are abundant signs that a considerable portion of this part of Africa once under water, and a fresh-water shell has been found in apparently isturbed blue ground at Kimberley.

‡ The water sunk in wells close to the Kimberley mine is sometimes impreg- ed with paraffin, and Sir H. Roscoe extracted a solid hydrocarbon from the e ground."

minerals, iron oxide, shale, petroleum and diamonds are churned together in a veritable witch's cauldron? As the heat abated the water vapour would gradually give place to hot water, which, forced through the magma, would change some of the mineral fragments into the now existing forms.

Each outbreak would form a dome-shaped hill, but the eroding agency of water and ice would plane these eminences until all traces of the original pipes were lost.

Actions such as I have described need not have taken place simultaneously. As there must have been many molten masses of iron with variable contents of carbon, different kinds of colouring matter, solidifying with varying degrees of rapidity, and coming in contact with water at intervals throughout long periods of geological time—so must there have been many outbursts and upheavals, giving rise to pipes containing diamonds. And these diamonds, by sparseness of distribution, crystalline character, difference of tint, purity of colour, varying hardness, brittleness and state of tension, would have impressed upon them, engraved by natural forces, the story of their origin—a story which future generations of scientific men may be able to interpret with greater precision than we can to-day.

Who knows but that at unknown depths in the earth's metallic core beneath the present pipes there are still masses of iron not yet disintegrated and oxidised by aqueous vapour—masses containing diamonds, unbroken and in greater profusion than they exist in the present blue ground, inasmuch as they are enclosed in the matrix itself, undiluted by the numerous rock constituents which compose the bulk of the blue ground?

If this be the case a careful magnetic survey of the country round about Kimberley might prove of immense interest, scientific and practical. Observations, at carefully selected stations, of the three magnetic elements—the horizontal component of direction, the vertical component of direction and the magnetic intensity—would soon show whether any large masses of iron exist within a certain distance of the surface. It has been calculated that a mass of iron 500 feet in diameter could be detected were it ten miles below the surface. A magnetic survey might also reveal other valuable diamantiferous pipes, which owing to the absence of surface indications would otherwise remain hidden.

There is another diamond theory which appeals to the fancy. It is said that the diamond is a direct gift from Heaven, conveyed to earth in meteoric showers. The suggestion, I believe, was first broached by A. Meydenbauer,* who says:—"The diamond can only be of cosmic origin, having fallen as a meteorite at later periods of the earth's formation. The available localities of the diamond contain the residues of not very compact meteoric masses which may,

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* 'Chemical News,' vol. lxi. p. 209, 1890.

perhaps, have fallen in historic ages, and which have penetrated more or less deeply, according to the more or less resistant character of the surface where they fell. Their remains are crumbling away on exposure to the air and sun, and the rain has long ago washed away all prominent masses. The enclosed diamonds have remained scattered in the river beds, while the fine light matrix has been swept away."

According to this hypothesis, the so-called volcanic pipes are simply holes bored in the solid earth by the impact of monstrous meteors—the larger masses boring the holes, while the smaller masses, disintegrating in their fall, distributed diamonds broadcast. Bizarre as such a theory may appear, I am bound to say there are many circumstances which show that the notion of the Heavens raining diamonds is not impossible.

In 1846 a meteorite fell in Hungary (the "Ava meteorite") which was found to contain graphite in the cubic crystalline system. G. Rose thought this cubic graphite was produced by the transformation of a diamond. Long after this prediction was verified by Weinschenk, who found transparent crystals in the Ava meteorite. Mr. Fletcher has found in two meteoric irons—one from Youndegin, East Australia, and one from Crosby's Creek, United States—crystals absolutely similar to those in the Ava meteorite.

In 1886 a meteoric falling in Russia contained, besides other constituents, about 1 per cent. of carbon in light grey grains, having the hardness of diamond, and burning in oxygen to carbonic acid.

Daubrée says the resemblance is manifest between the diamantiferous earth of South Africa and the Ava meteorite, of which the stony substance consists almost entirely of peridot. Peridot being the inseparable companion of meteoric iron, the presence of diamonds in the meteorites of Ava, of Youndegin, and of Crosby's Creek, bring them close to the terrestrial diamantiferous rocks.

Hudleston maintains that the bronzite of the Kimberley blue ground is in a condition much resembling the bronzite grains of meteorites; whilst Maskelyne says that the bronzite crystals of Dutoitspan resemble closely those of the bronzite of the meteor of Breitenbach, but are less rich in crystallographic planes.

But the most striking confirmation of the meteoric theory comes from Arizona. Here, on a broad open plain, over an area about five miles diameter, were scattered one or two thousand masses of metallic iron, the fragments varying in weight from half a ton to a fraction of an ounce. There is little doubt these masses formed part of a meteoric shower, although no record exists as to when the fall took place. Curiously enough, near the centre, where most of the meteorites have been found, is a crater with raised edges three-quarters of a mile in diameter, and about 600 feet deep, bearing exactly the appearance which would be produced had a mighty mass of iron or falling star struck the ground, scattered in all directions, and buried itself deep under the surface. Altogether ten tons of

this iron have already been collected, and specimens of the Canyon Diablo meteorite are in most collectors' cabinets.

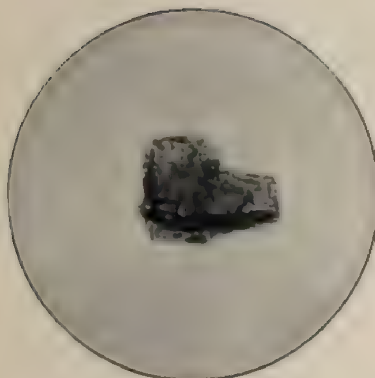
An ardent mineralogist, the late Dr. Foote, in cutting a section of this meteorite, found the tools were injured by something vastly harder than metallic iron, and an emery-wheel used in grinding the iron had been ruined. He examined the specimen chemically, and soon after announced to the scientific world that the Canyon Diablo meteorite contained black and transparent diamonds. This startling discovery was afterwards verified by Professors Friedel and Moissan, who found that the Canyon Diablo meteorite contained the three varieties of carbon—diamond (transparent and black), graphite and amorphous carbon. Since this revelation, the search for diamonds in meteorites has occupied the attention of chemists all over the world.

I am enabled to show you photographs of true diamonds I myself have extracted from pieces of the Canyon Diablo meteorite (Figs. 29, 30), five pounds of which I have dissolved in acids for this purpose —an act of vandalism in the cause of science for which I hope mineralogists will forgive me. A very fine slab of the meteorite, weighing about seven pounds, which has escaped the solvent, is on the table before you.

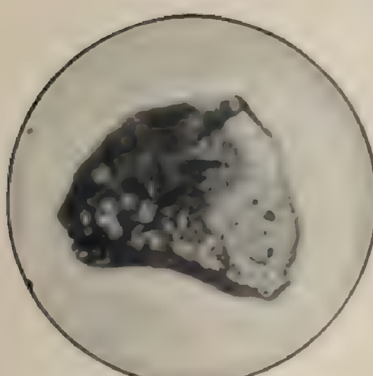
Here, then, we have absolute proof of the truth of the meteoric theory. Under atmospheric influences the iron would rapidly oxidise and rust away, colouring the adjacent soil with red oxide of iron. The meteoric diamonds would be unaffected, and would be left on the surface of the soil to be found by explorers when oxidation had removed the last proof of their celestial origin. That there are still lumps of iron left at Arizona is merely due to the extreme dryness of the climate and the comparatively short time that the iron has been on our planet. We are here witnesses to the course of an event which may have happened in geologic times anywhere on the earth's surface.

Although in Arizona diamonds have fallen from above, confounding all our usual notions, this descent of precious stones seems what is called a freak of Nature rather than a normal occurrence. To the modern student of science there is no great difference between the composition of our earth and that of extra-terrestrial masses. The mineral peridot is a constant extra-terrestrial visitor, present in most meteorites. And yet no one doubts that peridot is also a true constituent of rocks formed on this earth. The spectroscope reveals that the elementary composition of the stars and the earth are pretty much the same; so does the examination of meteorites. Indeed, not only are the selfsame elements present in meteorites but they are combined in the same way to form the same minerals as in the crust of the earth.

This identity between terrestrial and extra-terrestrial rocks recalls the masses of nickeliferous iron of Ovifak. Accompanied with graphite they form part of the colossal eruptions which have covered



29.—Diamond from Canyon Diablo Meteorite.



30.—Diamond from Canyon Diablo Meteorite.

GENERAL MONTHLY MEETING.

Monday, June 14, 1897.

SIR JAMES CRICHTON-BROWNE, M.D. LL.D. F.R.S. Treasurer and Vice-President, in the Chair.

The following Vice-Presidents for the ensuing year were announced:—

Sir Frederick Abel, Bart. K.C.B. D.C.L. LL.D. F.R.S.
The Right Hon. A. J. Balfour, M.P. D.C.L. LL.D. F.R.S.
William Crookes, Esq. F.R.S.
Edward Frankland, Esq. D.C.L. LL.D. F.R.S.
Ludwig Mond, Esq. Ph.D. F.R.S.
Basil Woodd Smith, Esq. F.R.A.S. F.S.A.
Sir James Crichton-Browne, M.D. LL.D. F.R.S. *Treasurer.*
Sir Frederick Bramwell, Bart. D.C.L. LL.D. F.R.S. *Hon. Secretary.*

Tempest Anderson, M.D. B.Sc.
Samuel Pope, Esq. Q.C.
Major Clifford Probyn,

were elected Members of the Royal Institution.

The following Address to the Queen was read, and it was moved from the Chair, seconded by the Honorary Secretary, and carried by acclamation, all present standing,

"That this Address be approved and authorised to be signed by His Grace the President on behalf of the Members:—

TO HER MOST GRACIOUS MAJESTY THE QUEEN,
Patron of the Royal Institution of Great Britain.

MAY IT PLEASE YOUR MAJESTY,

We, the President and Members of the Royal Institution of Great Britain, in general meeting assembled, desire humbly to congratulate your Majesty on the completion of the Sixtieth Year of your glorious and beneficent reign, and with profound thankfulness to acknowledge the blessings which we, in common with all classes of your subjects, have enjoyed under your rule, and more especially, the freedom and encouragement given to those pursuits with which we, as a corporation, are concerned.

Science, Arts, and Manufactures, which it is the object of our institution to promote, have found in the serenity, which your just and gentle government has conferred upon the country, the conditions most favourable to their growth, while the ethical principles, which ought ever to sustain and direct these, have been quickened by the virtues which have adorned your throne. The extension of education, and particularly of that technical education, the national importance of which your late illustrious and ever lamented Consort was the first to recognise, has favoured the diffusion of natural knowledge, which again has multiplied useful mechanical inventions, and conduced to new applications of the mineral and other productions of the country.

We venture to believe that the investigations carried on in the laboratories of our Institution during the last sixty years, by its eminent Professors, have resulted in discoveries which will make your reign memorable in the annals of science, and we confidently anticipate that the addition recently made to the resources of the Institution, by the generosity of one of your subjects, will greatly enhance its public usefulness in the future.

We fervently hope and pray that your Majesty will still, for many years to come, reign over us and the vast and varied Empire that is happily united under your Sceptre, and that to-day, with one voice, offers you its grateful homage; and we look to a continuance of the gracious patronage, which you and the Royal Family have so long bestowed on our Institution, as the best guarantee of its prosperity and success."

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FOR

The Lords of the Admiralty—Report of the Astronomer Royal to the Board of Visitors. fol. 1897.

Report of Her Majesty's Astronomer at the Cape of Good Hope for 1897. 4to. 1897.

Independent Day-Numbers for 1897, as used at the Royal Observatory, Cape of Good Hope. 8vo. 1897.

The Secretary of State for India—Progress Report of the Archæological Survey of Western India for Sept. 1895 to April 1896. fol. 1896.

Palæontologia Indica. Ser. XVI. Baluchistan and N.W. Frontier. Vol. I. The Jurassic Fauna. Part 1, The Fauna of the Killawuys of Mazar Drik. By F. Noetling. fol. 1895.

Memoirs, Vols. XXV. XXVI. 8vo. 1895–96.

Accademia dei Lincei, Reale, Roma—Atti, Serie Quinta: Rendiconti. Classe di Scienze Morali, Vol. VI. Fasc. 2. Classe di Scienze Fisiche, etc. 1° Semestre, Vol. VI. Fasc. 8–10. 8vo. 1897.

American Academy of Arts and Sciences—Proceedings, Vol. XXXII. Nos. 5–9. 8vo. 1897.

Armstrong, Lord, C.B. F.R.S. M.R.I. (the Author)—Electric Movement in Air and Water, with Theoretical Inferences. fol. 1897.

Asiatic Society of Bengal—Proceedings, 1896. Nos. 6–10. 8vo. 1896–97.

Journal, Vol. LXV. Part 1, Nos. 3, 4; Part 2, Nos. 3, 4; Part 3, No. 1. 8vo. 1896–97.

Astronomical Society, Royal—Monthly Notices, Vol. LVII. No. 6. 8vo. 1897.

Bankers, Institute of—Journal, Vol. XVIII. Part 5. 8vo. 1897.

Batavia Magnetical and Meteorological Observatory—Observations, Vol. XVIII. (1895). 4to. 1896.

Rainfall in the East Indian Archipelago (1895). 8vo. 1896.

Berlin, Koniglich Preussische Akademie der Wissenschaften—Sitzungsberichte, 1897. Nos. 1–25 8vo.

Boston, U.S.A. Public Library—Monthly Bulletin of Books added to the Library, Vol. II. No. 5. 8vo. 1897.

Boston Society of Natural History—Proceedings, Vol. XXVII. No. 14. 8vo. 1897.

Botanic Society, Royal—Quarterly Record, No. 69. 8vo. 1897.

British Architects, Royal Institute of—Journal, 1896–97, Nos. 13, 14. 8vo.

British Astronomical Association—Journal, Vol. VII. No. 6. 8vo. 1897.

British Institute of Public Health—Journal of State Medicine, Vol. V. No. 1. 8vo. 1897.

British Museum Trustees—Subject Index of Modern Works added to the Library of the British Museum in the years 1885–90 and 1891–95. Compiled by G. K. Fortescue. 2 vols. 8vo. 1891–97.

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Brymner, Douglas, Esq. LL.D. F.R.S.C. (the Archivist)—Report on Canadian Archives for 1896. 8vo. 1897.

California, University of—Report of Work of the Agricultural Experiment Stations of the University of California for 1894-95. 8vo. 1896.

Agricultural Experiment Station Bulletins.

Report of the Viticultural Work of the Agricultural Experiment Station, 1887-93. 8vo. 1896.

Notes on Children's Drawings. Edited by G. E. Brown. (Univ. of Cal. Studies, Vol. II. No. 2.) 8vo. 1897.

Geological Bulletins, Vol. I. Nos. 12-14. 8vo. 1896.

Biennial Report of the President of the University, 1894-96. 8vo. 1896.

Register of the University, 1895-96. 8vo.

Quicksilver Condensation at New Almaden, Cal. By S. B. Christy. 8vo. 1885.

On the Correlation of Elementary Studies. By G. H. Hourson. 8vo. 1896.

Grape Sugar Syrup. 8vo. 1893.

The White Wine Problem. 8vo. 1895.

The Vine in Southern California. 8vo. 1892.

The Vineyards in Alamede County. 8vo. 1893.

The Vineyards of Southern California. 8vo. 1888.

Annual Report of the Viticultural Commissioners. 1887. 8vo. 1888.

Study of Human Foods and Practical Dietetics. By M. E. Jaffa. 8vo. 1896.

Canadian Institute—Proceedings, New Series, Vol. I. Part 1, No. 1. 8vo. 1897.

Canning, Hon. A. S. G. (the Author)—History in Fact and Fiction. A Literary Sketch. 8vo. 1897.

Carruthers, Rev. G. T. (the Author)—The Origin of the Celestial Laws and Motions. 8vo. 1897.

Chemical Industry, Society of—Journal, Vol. XVI. No. 4. 8vo. 1897.

Chemical Society—Journal for May, 1897. 8vo.

Proceedings, Nos. 179, 180. 8vo. 1897.

Chicago Academy of Sciences—Twenty-ninth Annual Report for 1896. 8vo 1897.

The Lichen Flora of Chicago and Vicinity. By W. W. Calkins. (Bulletin of Geological and Natural History Survey, No. 1.) 8vo. 1896.

Chicago Field Columbian Museum—Contribution (2) to the Coastal and Plain Flora of Yucatan. By C. F. Millspaugh. (Botanical Series, Vol. I. No. 3.) 8vo. 1896.

Catalogue of a Collection of Birds obtained by the Expedition into Somali-land. By D. G. Elliot. (Ornithological Series, Vol. I. No. 2.) 8vo. 1897.

Cracovie, l'Académie des Sciences—Bulletin International, 1897, No. 3. 8vo.

Crawford and Balcarres, The Right Hon. the Earl of, K.T. F.R.S. M.R.I.—Bibliotheca Lindesiana. First Revision. Hand List of Proclamations, Vol. II. George I-William IV. 1714-1837. (Privately Printed at the Aberdeen University Press.) fol. 1897.

Dissett, M. R. Esq. (the Author)—The Explanation of the Origin of Solar and Stellar Light, and the Minor Phenomena connected therewith. 8vo. 1897.

Edinburgh, Royal College of Physicians—Reports from the Laboratory, Vol. VI. 8vo. 1897.

Editors—American Journal of Science for May, 1897. 8vo.

Analyst for May, 1897. 8vo.

Anthony's Photographic Bulletin for May, 1897. 8vo.

Aeronautical Journal for April, 1897. 8vo.

Astrophysical Journal for May, 1897. 8vo.

Athenæum for May, 1897. 4to.

Author for May, 1897.

Bimetallist for May, 1897.

Brewers' Journal for May, 1897. 8vo.

Chemical News for May, 1897. 4to.

Chemist and Druggist for May, 1897. 8vo

Education for May, 1897. 8vo.

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' *Information Office*—Combined Circulars for Canada, the Australian
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F. E. Esq.—La Resurreccion de Jesu-Christo, Nuestro Señor. 8vo.
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Index to Vols. I.-L. of the Quarterly Journal, Part 2, La-Z. 8vo.

Institute—Imperial Institute Journal for May, 1897.
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1897.
Otto Hesse's Gesammelte Werke. Herausgegeben von d. Math. Phys.
d. k. Bayerischen Akad. d. Wissenschaften. 4to. 1897.

Navy League—Navy League Journal for May, 1897. 4to.
New York Academy of Sciences—Transactions, Vol. XV. 1895-96. 8vo. 1896.
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North of England Institute of Mining and Mechanical Engineers—Transactions, Vol. XLVI. Part 3. 8vo. 1897.
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Odontological Society of Great Britain—Transactions, Vol. XXIX. No. 7. 8vo. 1897.
Onnes, Prof. Dr. H. Kamerlingh—Communications from the Physical Laboratory at the University of Leiden, No. 25. 8vo. 1897.
Paris, Société Française de Physique—Bulletin, Nos. 95-97. 8vo. 1897.
Séances, 1895, Fasc. 1, 2. 8vo. 1895.
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Royal Engineers, Corps of—Professional Papers, Foreign Translation Series, Vol. I. Nos. 4, 5. 8vo. 1897.
Professional Papers of the Corps of Royal Engineers, Vol. XXII. 8vo. 1896.
Royal Society of Edinburgh—Proceedings, Vol. XXI. No. 4. 8vo. 1897.
Royal Society of London—Philosophical Transactions, Vol. CLXXXVIII. B. Nos. 144, 145; Vol. CLXXXIX. A. Nos. 193-196. 4to. 1897.
Proceedings, Nos. 371-373. 8vo. 1897.
Russell, The Hon. F. A. Rollo, F.R.Met.Soc. M.R.I. (the Author)—The Atmosphere in relation to Human Life and Health. (Smithsonian Miscellaneous Collections, No. 1072, Hodgkins Prize Essay.) Washington. 8vo. 1896.
Selborne Society—Nature Notes for May, 1897. 8vo.
Smith, Miss Owen—Fallacies of Race Theories as applied to National Characteristics. By W. D. Babington. 8vo. 1895.
Smithsonian Institution—Atmospheric Actinometry and the Actinic Constitution of the Atmosphere. (Hodgkins Prize Essay, Smith. Cont. to Knowledge, No. 1034.) 4to. 1896.
Virginia Cartography: A Bibliographical Description. By P. L. Phillips. (Smith. Misc. Coll. 1039.) 8vo. 1896.
The Atmosphere in relation to Human Life and Health. By F. A. R. Russell. (Smith. Misc. Coll. 1072.) 8vo. 1896.
Constants of Nature, Part 5. By F. W. Clarke. (Smith. Misc. Coll. 1075.)
Air and Life. By H. De Varginy. (Hodgkins Prize Essay, Smith. Misc. Coll. 1071.) 8vo. 1896.
Mountain Observatories in America and Europe. By E. S. Holden. (Smith. Misc. Coll. 1035.) 8vo. 1896.
Smithsonian Physical Tables. By T. Gray. (Smith. Misc. Coll. 1038.) 8vo. 1896.
The Air of Towns. By Dr. J. B. Cohen. (Hodgkins Prize Essay, Smith. Misc. Coll. 1073.) 8vo. 1896.
Equipment and Work of an Aero-Physical Observatory. By A. McAdie. (Hodgkins Prize Essay, Smith. Misc. Coll. 1077.) 8vo. 1897.
Society of Arts—Journal for May, 1897. 8vo.
St. Bartholomew's Hospital—Reports, Vol. XXXII. 8vo. 1897.
Sunday Lecture Society—The Sunday Bill of 1895. By A. V. F. Wild. 8vo. 1897.
Tacchini, Prof. Hon. Mem. R.I. (the Author)—Memorie della Società degli Spettroscopisti Italiani, Vol. XXVI. Disp. 1, 2. 4to. 1897.
United States Department of Agriculture—Experiment Station Bulletin, No. 40. 8vo. 1897.

nited States Department of Interior (Census Office)—Report on Crime, Pauperism and Benevolence in the U.S. at the Eleventh Census, 1890, Part 1, Analysis. 4to. 1896.

Report on Insurance Business at Eleventh Census, 1890, Part 2, Life Insurance. 4to. 1895.

Report on Vital and Social Statistics at Eleventh Census, Part 2, Vital Statistics; Part 4, Statistics of Deaths. 4to. 1895–96.

Report on the Insane, Feeble-minded, Deaf and Dumb and Blind at the Eleventh Census. 4to. 1895.

Report on Farms and Homes, Proprietorship and Indebtedness at the Eleventh Census 4to. 1896.

nited Service Institution, Royal—Journal for May, 1897. 8vo.

nited States Patent Office—Official Gazette, Vol. LXXVIII. Nos. 8-13; Vol. LXXXIX. Nos. 1, 2. 8vo. 1897.

Alphabetical List of Patentees and Inventions to Sept. 1896. 8vo.

niversity College—Supplement to the Catalogue (1879) of the General Library and South Library of University College. 8vo. 1897.

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ienna, Geological Institute, Imperial—Jahrbuch, Band XLVI. Heft 2. 8vo. 1897.

illiams and Norgate, Messrs. (the Publishers)—Problems of Nature: Researches and Discoveries of Gustav Jaeger. Edited and Translated by H. G. Schlichter. 8vo. 1897.

orkshire Philosophical Society—Annual Report for 1896. 8vo. 1897.

oung & Co. Messrs. D. (the Publishers)—The Inventor's Companion. 8vo. 1897.

oological Society of London—Report of the Council for 1896. 8vo. 1897.

urich, Naturforschende Gesellschaft—Vierteljahrsschrift, 1897, Heft 1. 8vo.

GENERAL MONTHLY MEETING,

Monday, July 5, 1897.

SIR JAMES CRICHTON-BROWNE, M.D. LL.D. F.R.S. Treasurer and Vice-President, in the Chair.

Hugh Harper Baird, Esq.
Ivon Braby, Esq.
James Mackenzie Davidson, Esq. M.B. C.M.
Arthur Croft Hill, Esq. B.A.
James Y. Johnson, Esq.
Leo Kamm, Esq.
Michael Edmund Stephens, Esq.
The Rev. Henry Wace, D.D.
Julius Wernher, Esq.
Henry Wilde, Esq. F.R.S.

were elected Members of the Royal Institution.

The Special Thanks of the Members were returned for the following Donation to the Fund for the Promotion of Experimental Research at Low Temperatures:—

Sir Andrew Noble, K.C.B. £100

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

Accademia dei Lincei, Reale, Roma—Classe di Scienze Fisiche, Matematiche e Naturali. Atti, Serie Quinta: Rendiconti. 1° Semestre, Vol. VI. Fasc. 11. Classe di Scienze Morali, &c. Serie Quinta, Vol. VI. Fasc. 3, 4. 8vo. 1897.
Agricultural Society of England, Royal—Journal, 3rd Series, Vol. VIII. Part 2. 8vo. 1897.
American Academy of Arts and Sciences—Proceedings, New Series, Vol. XXII. Nos. 10, 12. 8vo. 1897.
Astronomical Society, Royal—Monthly Notices, Vol. LVII. No. 7. 8vo. 1897.
Bankers, Institute of—Journal, Vol. XVIII. Part 6. 8vo. 1897.
Bech, M. M. (the Author)—Étude expérimentale sur l'Electro-Magnétisme renversant toutes les idées actuellement admises sur cette science. 8vo. 1897.
Boston Public Library—Monthly Bulletin, Vol. II. No. 6. 8vo. 1897.
British Architects, Royal Institute of—Journal, 3rd Series, Vol. IV. Nos. 15, 16. 4to. 1897.
British Astronomical Association—Journal, Vol. VII. Nos. 7, 8. 8vo. 1897.
Cambridge Philosophical Society—Proceedings, Vol. IX. Part 5. 8vo. 1897.
Camera Club—Journal for May-June, 1897. 8vo.
Cape of Good Hope, The Surveyor-General of the Colony of the—Report on Colonel Morris's Geodetic Survey of South Africa. By D. Gill. fol. 1896.
Chemical Industry, Society of—Journal, Vol. XVI. No. 5. 8vo. 1897.
Chemical Society—Journal for June, 1897. 8vo.
Proceedings, Nos. 173-178, 181. 8vo. 1897.

American Journal of Science for June, 1897. 8vo.
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l of Physical Chemistry for June, 1897.
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for June, 1897. 4to.
Book List for June, 1897. 8vo.
Church Magazine for June, 1897. 8vo.
Cimento for May, 1897. 8vo.
graphic News for June, 1897. 8vo.
Health Engineer for June, 1897. 8vo.
Siftings for June, 1897.
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al Agriculturist for June, 1897.
list for June, 1897. 4to.
Engineers, Institution of—Journal. Vol. XXV. No. 129. 8vo. 1897.
s' Information Office—Combined Circulars for Canada, The Australasian South African Colonies, Nos. 1-3. 8vo. 1897.
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Biblioteca Nazionale Centrale—Bolletino, No. 275. 8vo. 1897.
Institute—Journal for June, 1897. 8vo.
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New Guinea, Country and People. By Sir W. Macgregor. (Extra lication.) 8vo. 1897.
Persian Irak. By General A. Houtum-Schindler. (Extra Publica-) 8vo. 1897.
Institute—Imperial Institute Journal for June, 1897.
pkins University—American Journal of Philology, Vol. XVIII. No. 1. 1897.
an Chemical Journal, Vol. XIX. No. 6 (June). 8vo. 1897.
sity Circulars, No. 130. 8vo. 1897.

Knox, H. T. C. Esq. M.R.I.—The Navy League Guide to the Naval Review of 1897. 8vo. 1897.
Meteorological Society, Royal—Quarterly Journal for April, 1897. 8vo.
Meteorological Record, No. 63. 8vo. 1897.
Hints to Meteorological Observers. Fourth edition. 8vo. 1897.
Microscopical Society, Royal—Journal, 1897, Part 3. 8vo.
Middlesex Hospital—Reports for 1895. 8vo. 1896.
Navy League—Navy League Journal for June, 1897. 8vo.
Paris, Société Française de Physique—Séances, 1896, Fasc. 4.
Bulletin, Nos. 98, 99. 8vo. 1897.
Perry-Coste, F. H. Esq. (the Author)—An Extraordinary Case of Colour Blindness. 8vo. 1897.
Pharmaceutical Society of Great Britain—Journal for June, 1897. 8vo.
Philadelphia, Academy of Natural Sciences—Proceedings, 1897, Part 1. 8vo.
Philadelphia, Geographical Society of—Map of the Arctic Regions (with most recent Explorations). By A. Heilprin. fol. 1897.
Photographic Society, Royal—Photographic Journal for June, 1897. 8vo.
Physical Society of London—Proceedings, Vol. XV. Part 6. 8vo. 1897.
Rome, Ministry of Public Works—Giornale del Genio Civile, 1897, Fasc. 3. 8vo.
Rose & Co. Messrs. W.—Jubilee Souvenir of the Fire Service. A History of the Fire Service and its Organisations. fol. 1897.
Royal Cornwall Polytechnic Society—Sixty-fourth Annual Report, 1896. 8vo.
Royal Society of London—Proceedings, No. 374. 8vo. 1897.
Saxon Society of Sciences, Royal—
Mathematisch-Physische Classe—
Berichte, 1897, Nos. 1, 2. 8vo. 1897.
Selborne Society—Nature Notes for June, 1897. 8vo.
Society of Arts—Journal for June, 1897. 8vo.
St. Petersburg, Académie Impériale des Sciences—Bulletin, Ve Série, Tome VI No. 3. 8vo. 1897.
Tacchini, Prof. P. Hon.Mem.R.I. (the Author)—Memorie della Società degli Spettroscopisti Italiani, Vol. XXVI. Disp. 3. fol. 1897.
Thornton, James Howard, Esq. C.B.—Memories of Seven Campaigns (India, China, Egypt, the Soudan). 8vo. 1895.
United Service Institution, Royal—Journal for June. 8vo. 1897.
United States Department of Agriculture—Experiment Station Record, Vol. VIII. No. 7. 8vo. 1897.
Experiment Station Bulletin, No. 38. 8vo. 1897.
Cotton Culture in Egypt. By G. P. Foaden. (Experiment Station Bulletin, No. 42.) 8vo. 1897.
Some Common Birds in their relation to Agriculture. By F. E. L. Beal. (Farmers' Bulletin, No. 54.) 8vo. 1897.
United States Patent Office—Official Gazette, Vol. LXXIX. Nos. 3–6. 8vo. 1897.
Upsal, Royal Society of Sciences—Nova Acta, 3rd Ser. Vol. XVII. Fasc. 1. 4to. 1896.
Verein zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, 1897, Heft 5. 4to.
Vienna, Imperial Geological Institute—Verhandlungen, 1897, Nos. 6–8. 8vo.
Welch, J. Cuthbert, Esq. F.C.S. (the Compiler)—General Index of the Proceedings of the Society of Public Analysts. Vol I. and to The Analyst, Vols. I.–XX. (1877–96). 8vo. 1897.
Zoological Society of London—Proceedings, 1897, Part 1. 8vo. 1897.

GENERAL MONTHLY MEETING,

Monday, November 1, 1897.

SIR JAMES CRICHTON-BROWNE, M.D. LL.D. F.R.S. Treasurer and Vice-President, in the Chair.

John W. Woodall, Esq. M.A. D.L. J.P.

was elected a Member of the Royal Institution.

The following letter was read :—

WHITEHALL, 2*nd July*, 1897.

MY LORD DUKE,—I have had the honour to lay before The Queen the loyal and dutiful address of the Royal Institution of Great Britain on the occasion of Her Majesty attaining the sixtieth year of her reign, and I have to inform Your Grace that Her Majesty was pleased to receive the same very graciously.

I have the honour to be,
My Lord Duke,
Your Grace's obedient servant,
(Signed) M. W. RIDLEY.

His Grace The Duke of Northumberland, K.G., &c.

The Special Thanks of the Members were returned to Mr. A. J. Hipkins for his valuable present of the Collection of Tuning Forks made by the late Dr. Alexander J. Ellis, F.R.S. *M.R.I.*

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz. :—

FOR

The Governor-General of India—Geological Survey of India: Records, Vol. XXX. Parts 2, 3. 8vo. 1897.
The Lords of the Admiralty—Results of the Spectroscopic and Photographic Observations made at the Royal Observatory, Greenwich, in 1894. 4to. 1897.
Greenwich Observations for 1894. 4to. 1897.
The British Museum (Natural History)—Catalogue of the Fossil Cephalopoda, Part 3. By A. H. Ford and G. C. Crick. 8vo. 1897.
Catalogue of the African Plants collected by Dr. F. Welwitsch in 1853-61. Dicotyledons, Part 1. By W. P. Hiern. 8vo. 1896.
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GENERAL MONTHLY MEETING,

Monday, December 6, 1897.

Sir James Crichton-Browne, M.D. LL.D. F.R.S. Treasurer and Vice-President, in the Chair.

The Hon. Herbert Mills Birdwood, C.S.I. M.A. LL.D.
Major John Leslie, B.A.
Captain Henry George Lyons, R.E. F.G.S.
Cecil Powney, Esq.

were elected Members of the Royal Institution.

The Special Thanks of the Members were returned to Professor Dewar, LL.D. F.R.S. for his present of a Portrait of Mr. Benjamin Vincent, Honorary Librarian of the Royal Institution.

The following Lecture arrangements were announced:—

Professor Oliver Lodge, D.Sc. LL.D. F.R.S. Professor of Physics in University College, Liverpool. Six Lectures (adapted to a Juvenile Auditory) on The Principles of the Electric Telegraph. On Dec. 28 (*Tuesday*), Dec. 30, 1897; Jan. 1, 4, 6, 8, 1898.

Professor E. Ray Lankester, M.A. LL.D. F.R.S. Eleven Lectures on The Simplest Living Things. On *Tuesdays*, Jan. 18, 25, Feb. 1, 8, 15, 22, March 1, 8, 15, 22, 29.

Professor Dewar, M.A. LL.D. F.R.S. *M.R.I.* Fullerian Professor of Chemistry R.I. Three Lectures on The Halogen Group of Elements. On *Thursdays*, Jan. 20, 27, Feb. 3.

Jean Paul Richter, Esq. Ph.D. *M.R.I.* Three Lectures on Some Italian Pictures at the National Gallery. On *Thursdays*, Feb. 10, 17, 24.

Professor J. A. Fleming, M.A. D.Sc. F.R.S. *M.R.I.* Professor of Electrical Engineering in University College, London. Five Lectures on Recent Researches in Magnetism and Diamagnetism. On *Thursdays*, March 3, 10, 17, 24, 31.

Professor Patrick Geddes, F.R.S.E. Professor of Botany, University College, Dundee. Three Lectures on Cyprus. On *Saturdays*, Jan. 22, 29, Feb. 5.

William Henry Hadow, Esq. M.A. B.Mus. Fellow of Worcester College, Oxford. Three Lectures on The Structure of Instrumental Music (with Musical Illustrations). On *Saturdays*, Feb. 12, 19, 26.

Professor Walter Raleigh, M.A. Three Lectures on English Letter-Writers. On *Saturdays*, March 5, 12, 19.

Lionel Cust, Esq. M.A. F.S.A. Director of the National Portrait Gallery. Two Lectures on Portraits as Historical Documents; Portraits as Monuments. On *Saturdays*, March 26, April 2.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FOR

Accademia dei Lincei, Reale, Roma—Classe di Scienze Fisiche, Matematiche e Naturali. Atti, Serie Quinta: Rendiconti. 2° Semestre, Vol. VI. Fasc. 8, 9. Classe di Scienze Morali, &c. Serie Quinta, Vol. VI. Fasc. 7, 8. 8vo. 1897.

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WEEKLY EVENING MEETING,

Friday, May 21, 1897.

Sir Edward Frankland, K.C.B. D.C.L. LL.D. F.R.S.
Vice-President, in the Chair.

The Right Hon. Lord Kelvin, G.C.V.O. D.C.L. LL.D. F.R.S. *M.R.I.*

Contact Electricity of Metals.

§ 1. Without preface two 95 years old experiments of Volta's were, one of them shown, and the other described. The apparatus used consists of: (*a*) a Volta-condenser of two varnished brass plates, of which the lower plate is insulated in connection with the gold leaves of a gold leaf electroscope, and the upper plate is connected by a flexible wire with the sole plate of the instrument; (*b*) two circular discs, one of copper and the other of zinc, each polished and unvarnished. I hold one in my right hand by a varnished glass stem attached to it, while on my left hand I hold the other, which is kept metallically connected with the sole plate of the electroscope by a thin flexible wire.

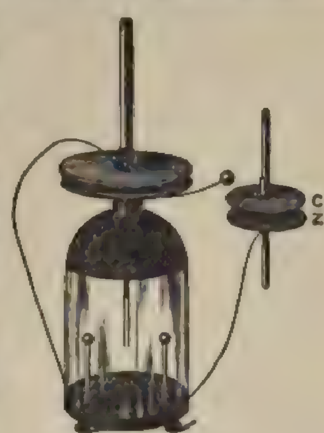

Fig. 1.

To commence the experiment I place one disc resting on the other, and lift the two till the upper touches a brass knob connected by a stiff metal wire with the lower plate of the Volta condenser. I break this contact and then lift the upper plate of the condenser; you see no divergence of the gold leaves. This proves that no disturbing electric influence sufficient to show any perceptible effect on our gold leaf electroscope is present. Now I repeat what I did, with only this change—I hold the lower disc with the upper disc resting on it two or three centimetres below the knob. I then with my right hand lift the upper plate of the Volta-condenser; you see a very slight divergence between the shadows of the gold leaves on the screen. I can just see it by looking direct at the leaves from a distance of about half a metre. Still holding the lower plate firmly in my left hand in the same position, and holding the upper plate by the top of its glass stem in my right, at first resting on the lower plate I lift it and

let it down very rapidly a hundred times, so as to produce one hundred cycles of operation—break contact between discs, make and break contact between upper disc and knob, make contact between discs. Lastly, I lift the upper plate of the condenser; you see now a great divergence of the gold leaves, many of you can see it direct on the leaves, while all of you can see it by their shadows on the screen. Now, keeping the upper plate of the condenser still unmoved, I bring a stick of rubbed sealing-wax into the neighbourhood of the electroscope; you see the divergence of the leaves is increased. I remove the sealing-wax and the divergence diminishes to what it was before. This proves that the gold leaves diverge in virtue of resinous electricity upon them, and therefore that the insulated plate of the condenser received resinous electricity from the copper disc. If now I interchange the two discs so that the upper is zinc and the lower copper, and repeat the experiment, you see that the rubbed sealing-wax diminishes the divergence as it is brought from a distance into the neighbourhood, and that a glass rod rubbed with silk increases the divergence. Hence we conclude that in the separation of two discs of copper and zinc the copper carries away resinous electricity and the zinc vitreous electricity.

§ 2. Experiment 2.—The same apparatus as in Experiment 1, except that the polished zinc and copper discs have their opposed faces varnished with shellac, and are provided with wires soldered to them for making metallic connection between them when the upper rests on the lower, as shown in Fig. 2. All operations are the same as in Experiment 1, but now with this addition—when the upper disc rests on the lower, make and break metallic contact by hand as shown in the diagram. The results are the same as those of Experiment 1, except that the quantity of electrification given to the gold leaves by a single cycle of operations is generally greater than in Experiment 1, for this reason: In Experiment 1 at the instant of breaking contact between the zinc and copper there is generally some degree of inclination between the two discs, while at the corresponding instant of Experiment 2 they are parallel and only separated by the insulating coats of varnish. If great care is taken to keep the discs as nearly as possible parallel at the instant of separation, the effect of a single separation may be made greater in Experiment 1 than in Experiment 2 (see § 3 below).

§ 3. An instructive variation of Experiment 1 may be made by giving a large inclination, 5°, or 10°, or 20°, of the upper plate to the lower, while still in contact and at the instant of separation. By operating thus the experiment may be made to fail so nearly completely that no divergence of the leaves will be observed even after one hundred cycles.

§ 4. These two experiments, with the variation described in § 3, put it beyond all doubt that Volta's electromotive force of contact between two dissimilar metals is a true discovery. It seems to have been made by him about the year 1801; at all events he exhibited

xperiments, proving it in that year to a Commission of the ch Institute (Academy of Sciences). It is quite marvellous that ndamental experiment (§ 1 above), simple, easy and sure as it s not generally shown in courses of lectures on electricity to nts, and has not been even mentioned or referred to in any ish text-book later than 1845, or at all events not in any one

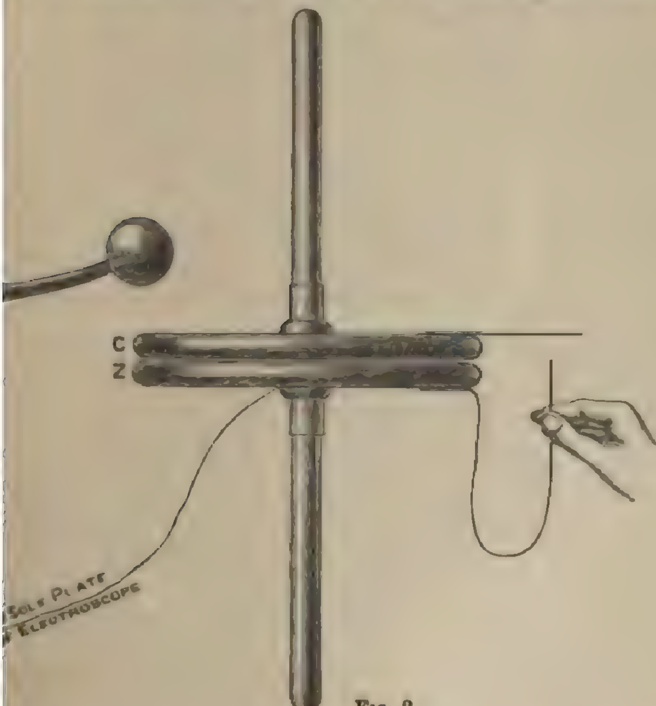

Fig. 2.

large number in which I have looked for it, except in the 'Ele-ary Treatise on Electricity and Magnetism,' founded on Joubert's ité Élémentaire d'Électricité,' by Foster and Atkinson, 1896 96). The only other places in which I have seen it described e English language are Roget's article in the 'Encyclopædia opolitana' referred to above; Tait's 'Recent Advances in Physical nce,' 1876; and Professor Oliver Lodge's most valuable, interest-nd useful account of all that had been done for knowledge of ct electricity from its discovery by Volta till 1884, in his Report

Fully and clearly described in Roget's article on "Galvanism," in the clopædia Metropolitana,' vol. iv. edition 1845, p. 210.

to the British Assocation of that year, 'On the Seat of the Electromotive Forces in the Voltaic Cell.'

§ 5. The reason for this unmerited neglect of a great discovery regarding properties of matter is that it was overshadowed by an earlier and greater discovery of its author, by which he was led to the invention of the voltaic pile and crown of cups, or voltaic battery, or, as it is sometimes called, the galvanic battery. Knowing, as we now know, both Volta's discoveries, we may describe the earlier most shortly by saying that the simple experiment (§ 1 above), demonstrating the later discovery, is liable to fail if a drop of water is placed on the lower of the two polished plates. It fails if (see Fig. 4 below) the last connection between the zinc and copper, when the upper disc is lifted, is by water. It would not fail (see Fig. 6 below) nor be sensibly altered from what is found with the dry polished metals, if the upper disc is slightly tilted in the lifting, so as to break the water arc before the separation between the metals, and secure that the last connection is contact of dry metals. To show this to you more readily than by a Volta condenser with gold leaf electroscope, I shall now use instead my quadrant electrometer without condenser.

(1) Holding the copper disc connected with the metal case of the electrometer in one hand, with my other hand I hold by a glass handle the zinc disc, which you see is connected by a fine wire with the insulated quadrants of the electrometer. I first place the zinc resting on the copper, both being polished and dry. You now see the spot of light at the point marked O on the scale, which I call the metallic zero. I now lift the zinc disc two or three millimetres from resting on the copper, and you see the spot of light travelling largely to the right, which proves that vitreous electricity has passed from the zinc disc through the connecting wire to the insulated quadrants of the electrometer. I lower the zinc disc down to rest again on the copper disc; you see the spot of light again comes back to the metallic zero.

(2) I now raise the zinc disc, and with a little piece of wet wood (or a quill pen) place a little mound of water on the copper disc, as shown in Fig. 3. I bring down the zinc disc to touch the top of the

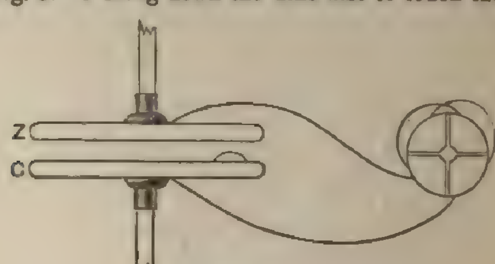

FIG. 3.

e mound of water, keeping it parallel to the copper disc so that e is no metallic contact between them (Fig. 4); you see that the

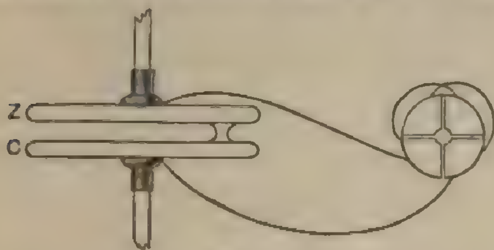

Fig. 4.

t of light moves to the left and settles at a point marked E (which ll the electrolytic zero of our circumstances), a few scale divisions he left of the metallic zero. This motion and settlement is the plest modern exhibition of Volta's greatest discovery.

(3) Now that the spot of light has settled, I lift the zinc disc illimetre till the water column is broken, and then two or three timetres farther (Fig. 5); the spot of light does not move, it

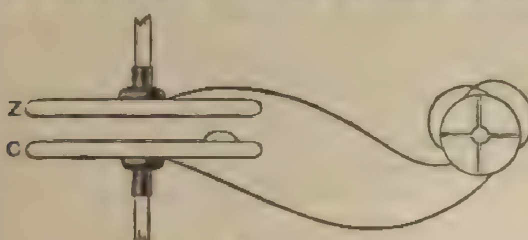

Fig. 5.

ains at E. I lower the zinc disc again: still no motion of the t of light, not even when the zinc again touches the little mound water.

(4) Now I tilt the zinc disc slightly till it makes a dry metallic tact with the copper, as shown in Fig. 6; while the water arc still

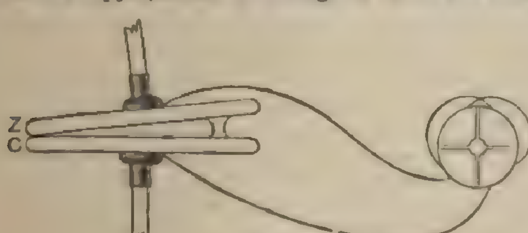

Fig. 6.

remains unbroken. You see the spot of light, at the instant of metallic contact, suddenly leaves E and moves to the right, and settles quickly at the metallic zero after a few vibrations through diminishing range.

(5) Lastly, I break the metallic contact, and hold the zinc disc again parallel to the copper (Fig. 4) with the water connection still remaining unbroken between them; the spot of light shows no sudden motion; it creeps to the left till, in half a minute or three-quarters of a minute, it reaches its previous steady position on the left. This is the now well-known phenomenon (never known to Volta) of the recovery of a voltaic cell from electrolytic polarisation after a metallic short-circuit.

§ 6. The succession of experiments described in § 5, interpreted according to elementary electrostatic law, proves the following conclusions:—

(1) When the dry and polished discs of zinc and copper are metallically connected and held parallel, their opposed faces are oppositely electrified, the zinc with vitreous electricity, and the copper with resinous electricity, in quantities varying inversely as the distance between them when this is small in comparison with the diameter of each.

(2) The opposed polished faces are non-electrified when polished portions of the zinc and copper surfaces are connected by water, and when there is no metallic connection between them. Or, if not absolutely free from electrification, they may be found slightly electrified, zinc resinously or vitreously, and copper vitreously or resinously, according to differences in respect to cleanness, polish, or scratching or burnishing, as explained in § 16 below; and according to polarisational or other difference in the wetted portions of the surfaces.

If instead of pure water we take a weak solution of common salt, or carbonate of soda, or sulphate of zinc or ammonia, we find results but little affected by the differences of the liquids.

§ 7. But if the polished surface of either the copper or the zinc is oxidised, or turnished in any way, notably different results are found when the experiments of § 5 are repeated with the disc or discs thus altered.

For example, hold the copper disc, with its polished side up, over a slab of hot iron, or a spirit lamp, or a Bunsen burner, till you see a perceptible change of colour, due to oxidation of the previously polished face. Then allow the copper to cool, and repolish a small area near one edge; place a little mound of water upon this area, and operate as in § 5 (2), (3). The water connection between polished zinc and polished copper brings the spot of light to the same electrolytic zero E as before. But now, when we lift the zinc disc and break the water connection, the spot of light moves to the right, instead of remaining steady as it does when both the dry opposed surfaces are polished. If

next we tarnish the zinc disc by heat, as we did for the copper disc, and repeat the experiment with wholly polished copper, and with the zinc disc oxidised where dry, and polished only where wet by the water connection, we find still the same electrolytic zero E; but now the spot of light moves to the left when we lift the zinc disc and break the water connection.

§ 8. The experiments of § 7, interpreted in connection with those of § 5, prove that there are dry contact voltaic actions between metallic copper and oxide of copper in contact with it, and between metallic zinc and oxide of zinc in contact with it; according to which, dry oxide of copper is resinous to copper in contact with it, and dry oxide of zinc is resinous to zinc in contact with it, just as copper is resinous to zinc in contact with it. We may verify this conclusion by another interesting experiment. Taking, for instance, the oxidised copper plate, with a little area polished for contacts; put a little mound of copper, instead of the mound of water, on this area for contact with the upper plate; and for the upper plate take polished copper instead of polished zinc. If we operate now as in § 7, the spot of light settles at the metallic zero O when the metallic contact is made, instead of at the electrolytic zero E, as it did when we had water connection between zinc and copper. But now, just as in § 7, the spot of light moves to the right when the contact is broken and the upper plate lifted, which proves that vitreous electricity flows into the electrometer from the upper plate, when its distance from the lower plate is increased after breaking the metallic contact. We conclude that when the two plates were parallel, and very near one another, and when there was metallic connection between them, vitreous and resinous electricities were induced upon the opposed surfaces of metallic copper and oxidised copper respectively. This statement, which we know from § 7 to be also true for zinc compared with oxidised zinc, is probably also true for every oxidisable metal compared with any one of its possible oxides. It is true, as we shall see later (appended paper of 1880–81; also Erskine Murray's paper referred to in § 15), even for platinum in its ordinary condition in our atmosphere of 21 per cent. oxygen and 79 per cent. nitrogen, voltaically tested in comparison with platinum which has been recently kept for several minutes or several hours in an atmosphere of pure oxygen, or even in an atmosphere of 95 per cent. oxygen and 5 per cent. nitrogen.

§ 9. Hitherto we have had no means of measuring the amount of the Volta-contact electric force between dry metals, except observation of the degrees of deflection of the gold leaves of an electroscope, or of the spot of light of the quadrant electrometer consequent upon operations performed upon different pairs of metals, with dimensions and distances of motion exactly the same, and comparison of these deflections with the steady deflection from the metallic zero given by polished zinc and copper connected conductively with one another by water, and connected metallically with the two electrodes of an

electroscope or electrometer. Kohlrausch, in 1851,* devised an apparatus for carrying out this kind of investigation systematically, and with a good approach to accuracy, by aid of a Dellman's electrometer and a Daniell's cell, as more definite and constant than a zinc-water-copper cell. This method of Kohlrausch's for measuring the Volta electromotive forces between dry metals, "has been employed with modifications by Hankel, by Gerland, by Clifton, by Ayrton and Perry, by von Zahn, and by most other experimenters on the subject."† About thirty-seven years ago, in repetitions of Volta's fundamental experiment proving contact electricity by electroscopic phenomena resulting from change of distance between parallel plates of zinc and copper, I found a null method for measuring electromotive forces due to metallic contact between dissimilar metals, in terms of the electromotive force of a Daniell's cell, which is represented diagrammatically in Fig. 7, and in perspective in Fig. 8. The two discs are protected against disturbing influences by a metal sheath. The lower disc is permanently insulated in a fixed position, and is kept connected with the insulated pair of quadrants of a quadrant electrometer. The upper disc is supported by a metal stem passing through a collar in the top of the sheath, so that it is kept always parallel to the lower disc and metallically connected to the sheath, while it can be lifted a few centimetres at pleasure from an adjustable lowest position in which its lower face is about half a millimetre or a millimetre above the upper face of the lower disc. A portion of the wire connecting the lower plate to the insulated quadrants of the electrometer is of polished platinum, and contact between this and a platinum-tipped wire connected to the slider of a potential divider is made and broken at pleasure. For certainty of obtaining good results it is necessary that these contacts should be between clean and dry polished metals, because if the last connection on breaking contact is through semi-moist dust, or oxide, or "dirt" (defined by Lord Palmerston to be matter in a wrong place), or if it is anything other than metallic, vitiating disturbance is produced.

§ 10. To make an experiment, first test the insulation with the upper plate held up in its highest position, and after that with it let down to its lowest position, in each case proceeding thus: Holding by hand the wire connected to the slider, run the slider to zero, make contact at P, observe on the screen the position of the spot of light from the electrometer mirror for the metallic zero, and then run the slider slowly to the top of its scale and break contact; the spot of light should remain steady, or at all events should not lose more than a very small percentage of its distance from metallic zero, in half a

* 'Poggendorff Annalen,' vols. lxxv. p. 88; lxxxii. pp. 1 and 45; and lxxxviii. p. 465, 1851 and 1853.

† Prof. O. J. Lodge, 'On the Seat of the Electromotive Forces in the Voltaic Cell,' Brit. Ass. Report, 1884, pp. 464–529.

minute. Repeat the test with the cell reversed. If the test is satisfactory with the upper plate high, the insulation of the insulated quadrants in the electrometer and of the lower disc in the Volta-condenser is proved good. If after that the test is not satisfactory with the upper disc at its lowest, we infer that there are vitiating shreds between the two plates, and we must do what we can to remove them; or, if necessary, we must alter the screw-stop at the top so as to increase the shortest distance between the plates sufficiently to prevent bridges of shred or dust between them, and so to give good insulation. The smaller we make the shortest distance with perfect

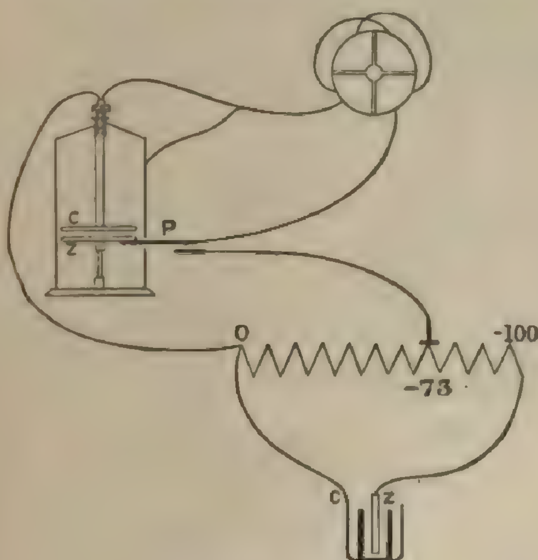

FIG. 7.

enough insulation, the more sensitive is the apparatus for the measurement of contact electricity performed as follows.

§ 11. Run the slider to zero; make and keep made the contact at P till the spot of light settles at the metallic zero; break contact at P, and lift the upper plate slightly. (If you lift it too far, the spot of light may fly out of range.) If the spot of light moves in the direction showing positive electricity on the insulated quadrants (as it does if the lower plate is zinc and the upper copper), connect the cell to make the slider negative (as shown in Fig. 7). Repeat the experiment with the slider at different points on the scale, until you find that, with contact P broken, lifting the upper plate causes no motion of the spot of light. If the compensating action with the slider at the top

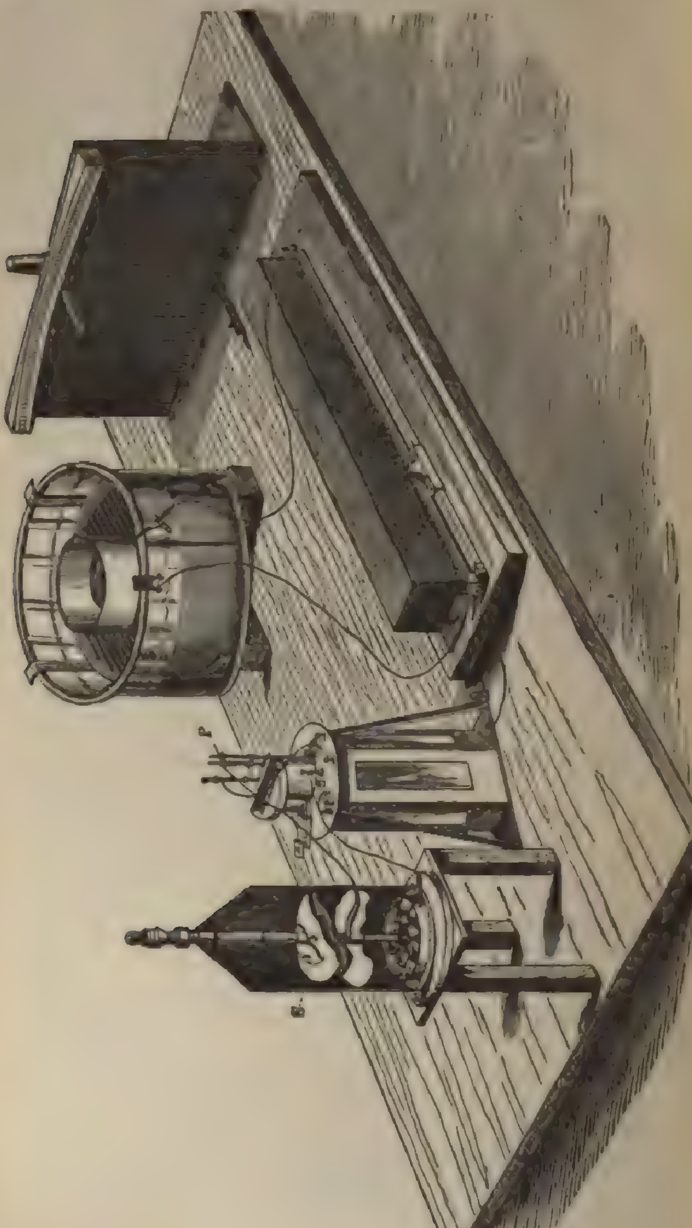

Fig. 8.

of the range is insufficient, add a second cell; if it is still insufficient, add a third cell; if still insufficient, add a fourth.*

§ 12. By this method I made an extended series of experiments in the years 1859–61, as stated in a short paper communicated to Section A of the British Association at its Swansea meeting in August 1880, which with additions published in 'Nature' for April 14, 1881, is appended to the present article.

§ 13. Quite independently,† Mr. H. Pellat found the same method, and made admirable use of it in a series of experiments described in theses presented to the Faculty of Sciences in Paris in 1881,‡ of which the results, accurate to a degree of minuteness unknown in previously published researches on the electrical effects of dry contacts between metals, constitute in many respects the most important and most interesting extension of our knowledge of contact electricity since the times of Volta and Pfaff. One of his results (I shall have to speak of others later) was that Pfaff was right in 1829 § when he described experiments in which he found no difference in the Volta-contact-electromotive force between zinc and copper, whether tested in dry or damp air, oxygen, nitrogen, hydrogen, carburetted hydrogen, or carbonic acid, so long as no visible chemical action occurred; and that De la Rive was not right when he "asserted that there was no Volta effect in the slightly rarefied air then known as vacuum." ‖ Pfaff experimented with varnished plates; Pellat arrived at the same conclusion with polished unvarnished plates of zinc and copper. He found slight variations of the Volta electromotive force due to the nature of the gas surrounding the plates, and to differences of its pressure, of which he says: "Ces variations sont très faibles, par rapport à la différence de potentiel totale. . . . Ces variations dans la différence de potentiel sont toujours en retard sur les changements de pression. Elles ne paraissent donc pas dépendre directement de celle-ci, mais bien des modifications qui en résultent dans la nature

* The only case hitherto tested by any experimenter, so far as known to me, in which more than two Daniell cells would be required for the compensation, is bright metallic sodium, guarded against oxide by glass, in Mr. Erskine Murray's experiments (§ 18 below), showing volta-difference of 3·56 volts from his standard gold plate. For direct test this would require four Daniell cells on the potential divider. The greatest volta-difference of potentials observed by Pellat was 1·08 volts, for which a Daniell's cell would rather more than suffice. About 1862 I found considerably more than the electromotive force of a single Daniell's element required to compensate the Volta electromotive force between polished zinc and copper oxidised by heat to a dark purple or slate colour.

† Ann. de Chimie et de Physique, vol. xxiv. 1881, p. 20, footnote.

‡ 'Thèses présentées à la Faculté des Sciences de Paris, pour obtenir le Grade de Docteur-ès-Sciences Physiques,' par M. H. Pellat, Professeur de Physique au Lycée Louis le Grand, No. 461, juin 22, 1881. See also 'Journal de Physique,' 1881, xvi. p. 68, and May 1880, 'Différence de potentiel des couches électriques qui recouvrent deux métaux en contact.'

§ Ann. de Chim., 2 series, vol. xli. p. 236.

‖ Lodge, Brit. Assoc. Report, 1884, pp. 477–8.

de la surface métallique, modifications qui mettent un certain temps à se produire." The smallest pressures for which Pellat made his experiments were from 3 to 4 or 5 cm. of mercury.*

§ 14. The same method was used by Mr. J. T. Bottomley in an investigation by which he demonstrated with minute accuracy the equality of the Volta-contact-difference measured in a glass tube exhausted to less than $\frac{1}{523}$ mm. of mercury* ($2\frac{1}{2}$ millionths of an atmosphere), and immediately after in the same tube filled with air to ordinary atmospheric pressure; and again exhausted and filled with hydrogen to atmospheric pressure three times in succession; and again exhausted and filled to atmospheric pressure with oxygen. In some cases the electrical test was repeated several times, while the gas was entering slowly. The actual apparatus which he used is before you, and in it I think you will see with interest the little Volta-condenser, with plates of zinc and copper a little larger than a shilling, the upper hung on a spiral wire by a long hook carrying also a small globe of soft iron. Thus you see by aid of an external magnet I can lift and lower the upper plate without moving the vacuum tube which, during the experiments, was kept in connection with a Sprengel pump and phosphoric acid drying tubes. Mr. Bottomley sums up thus: "The result of my investigation, so far as it has gone, is that the Volta contact effect, so long as the plates are clean, is exactly the same in common air, in a high vacuum, in hydrogen at small and full pressure, and in oxygen. My apparatus, and the method of working during these experiments, was so sensitive that I should certainly have detected a variation of 1 per cent. in the value of the Volta contact effect, if such a variation had presented itself." †

§ 15. With the same method further researches have been carried on by Mr. Erskine Murray, and important and interesting results obtained, within the last four years, in the Physical Laboratories of the Universities of Glasgow and Cambridge. He promises a paper for early communication to the Royal Society, and, from a partial copy of it which he has already given me, I am able to tell you of some of his results. Taking generally as standard a gilt brass disc which he found among the apparatus remaining from my experiments of 1859–61, he measured Volta-differences from it in terms of the modern standard *one volt*. These differences are what we may call the Volta-potentials of the different metallic surfaces, or surfaces of metallic oxides, iodides, &c., or metallic surfaces altered by cohesion to them of gases or vapours, or residues of liquids which had been used for washing them; if for simplicity we agree to call the Volta-potential of the gold, zero. As a rule he began each experiment by

* A very high exhaustion had been maintained for two days, and finally perfected by two and a half hours' working at the pump immediately before the electric testing experiment.

† Brit. Assoc. Report, 1885, pp. 901-3.

polishing the metal plate to be tested on clean glass paper or emery cloth, and then measured its difference of potential from the standard gold plate. After that the plate was subjected to some particular treatment, such as filing or burnishing; or polishing on leather or paper; or washing with water, or alcohol, or turpentine, and leaving it wet or drying it; or heating it in air, or exposing it to steam or oxygen, or fumes of iodine or sulphuretted hydrogen; or simply leaving it for some time under the influence of the atmosphere. The plate as altered by any of these processes was then measured for potential against the standard gold. Very interesting and instructive results were found; only of one can I speak at present. Burnishing by rubbing it firmly with a rounded steel tool, or by rubbing two plates of the same metal together, increased the potential in every case; that is to say made the metallic surface more positive if it was positive to begin with; or made it less negative or changed it from negative to positive, if it was negative to begin with. Thus:—

Zinc immediately after being scratched sharply by polishing on clean glass paper was found .	+ ·70 volt.
After being burnished with hard steel burnisher it was found	+ ·94 volt.
After being left to itself for 2 hours it was found	+ ·92 volt.
After further burnishing	+ 1·00 volt.
After still further burnishing	+ 1·02 volt.
It was then scratched by polishing on glass paper, and its surface potential returned to its original value of	+ ·70 volt.

§ 16. This seems to me a most important result. It cannot be due to the removal of oxygen, or oxide, or of any other substance from the zinc. It demonstrates that change of arrangement of the molecules at the free surface, such as is produced by crushing them together, as it were, by the burnisher, affects the electric action between the outer surface of the zinc and the opposed parallel gold plate. It shows that the potential * in zinc (uniform throughout the homogeneous interior)

* There has been much of wordy warfare regarding potential in a metal, but none of the combatants has ever told what he means by the expression. In fact the only definition of electric potential hitherto given has been for vacuum, or air, or other fluid insulator. Conceivable molecular theories of electricity within a solid or liquid conductor might admit the term potential at a point in the interior; but the function so called would vary excessively in intermolecular space, and must have a definite value for every point, whether of intermolecular space or within the volume of a molecule, or within the volume of an atom, if the atom occupies space. It would also vary intensely from point to point in the ether or air outside the metal at distances from the frontier small or moderate in comparison with the distance from molecule to molecule in the metal.

But when, setting aside our mental microscopic binocular which shows us atoms and molecules, we deal with the mathematical theory of equilibrium and motion of electricity through metals with outer surfaces bounded by ether or air or other

increases from the interior through the thin surface layer of a portion of its surface affected by the crushing of the burnisher, more by ·32 volt than through any thin surface-layer of portions of its surface left as polished and scratched by glass paper. The difference of potentials of copper and zinc across an interface of contact between them is only about 2½ times the difference of potential thus proved to be produced between the homogeneous interior of the zinc and its free surface, by the burnishing. Pellat had found that polished metallic surfaces, seemingly clean and free from visible contamination of any kind, became more positive by rubbing them forcibly with emery paper, zinc showing the greatest effect, which was ·23 volt. Murray's burnished surface of zinc actually *fell* ·32 volt when scratched by polishing on glass paper.

§ 17. With two copper plates (*a*), (*b*) polished on emery and each compared with standard gold, Murray found	(*a*) – ·11 volt. (*b*) – ·06 volt.
They were then burnished by rubbing them forcibly together, and again tested separately; he found	(*a*) – ·02 volt. (*b*) – ·02 volt.

Rises of Volta-potential of about the same amount were produced by burnishing with a steel burnisher copper plates which had been polished and scratched in various ways. Such experiments as those of Murray with burnishing ought to be repeated with hammering or crushing by a Bramah's press. Indeed Pellat * suggested that metals treated bodily "par le laminage ou le martelage" (rolling or hammering) might probably show Volta-electric properties of the same kind as, but more permanent than, those which he had found to be produced by violent scratching with emery paper.

§ 18. It is interesting to remark that Murray's most highly burnished zinc differed from his emery-polished copper (*a*) by 1·18

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insulating fluids or solids, we find it convenient to use a mathematical function of position called potential in the interior of each metal. This function must, for the case of equilibrium, fulfil the condition that it is of uniform value through each homogeneous portion of metal. Its value must, as a rule, change gradually (or abruptly) with every gradual (or abrupt) change of quality of substance occupying space.

To illustrate the difficulty and complexity of expression with which I have struggled, and to justify if possible my ungainly resulting sentence in the text, consider the case of a crystal of pure metal: suppose, for example, an octahedron with truncated corners, all natural faces and facets. In all probability Volta-differences of potential would be found between the octahedronal and truncational faces. We might arbitrarily define the uniform interior potential as the potential of the air either near an octahedronal face or near a truncational face; or, still arbitrarily, we might define it as some convenient mean or average related to measurements of Volta-differences of potential between the different faces.

* Ann. de Chimie et de Physique, 1881, vol. xxiv. footnote on p. 83.

volts. This is considerably greater, I believe, than the highest hitherto recorded Volta-difference between pure metallic surfaces of zinc and copper.

By far the greatest Volta-difference between two metallic surfaces hitherto measured is, I believe, 3·56 volts, which Murray, in another part of his work, found as the Volta-difference between bright sodium protected by glass and his standard gold. He had previously found a copper surface after exposure to iodine vapour to be −·34 relatively to his standard gold. The difference between this iodised surface and the bright metallic surface of sodium was therefore 3·90 volts: which is the highest dry Volta electromotive force hitherto known.

§ 19. Seebeck's great discovery of thermoelectricity (1821) was a very important illustration and extension of the twenty years' earlier discovery of the contact electricity of dry metals by Volta. It proved independently of all disturbing conditions that the difference of potentials between two metals in contact varies with the temperature of the junction. Thus, for instance, in the copper-iron arrangement

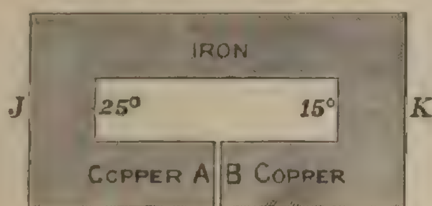

Fig. 9.

represented in Fig. 9, with its hot junction at 25° and its cold at 15°, the electromotive force tends to produce current from copper to iron through hot, and its amount is ·00148 volt: that is to say, if the circuit is broken at A B the two opposed faces A, B, at equal temperatures, present a difference of electric potential of ·00148 volt, with B positive relatively to A. This is not too small a difference to be tested directly by the Volta-static method, worked by two exactly similar metal discs connected to A and B, when they are at their shortest distance from one another, and then disconnected from A and B, and separated and tested by connection with a delicate quadrant electrometer. But the test would be difficult, because of the difficulty of preparing the opposed surfaces of two equal and similar discs, so as to make them equal in their surface-Volta-potentials within one one-thousandth of a volt, or even to make their difference of potentials constant during the time of experiment within one one-thousandth of a volt. There would, however, be no interest in making the experiment in this way, because by the electromagnetic method we can with ease exhibit and measure with great accuracy the difference of potentials between A and B, by keeping them exactly at one tempe-

rature and connecting them by wires of any kind with brass or other terminals of a galvanometer of high enough resistance not to sensibly diminish the difference of potentials between A and B, provided all the connections between metals of different quality except J and K are kept at one and the same temperature (or pairs of them, properly chosen, kept at equal temperatures).

§ 20. Suppose, now, instead of breaking a circuit of two metals at a place in one of the metals, as A B in copper in Fig. 9, we break it at one of the junctions between the two metals, as at C′ C, I′ I, Fig. 10. C D represents a movable slab of copper which (for § 22 below) may be pushed in so as to be wholly opposite to I′ I, or at pleasure drawn out to any position, still resting on the copper below it as shown in the diagram. Calling zero the uniform potential over the surfaces C′ C D, the potential at I′ I would be about + ·16 volt (according to

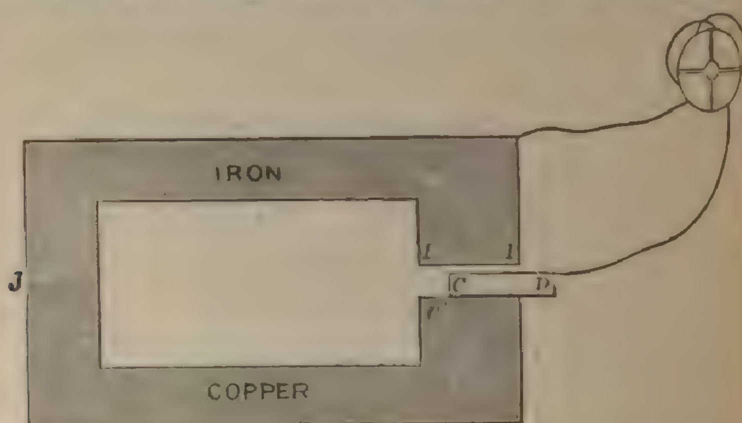

Fig. 10.

Murray's results for emery-polished copper and iron surfaces) if the temperature at J and throughout the system is uniform at about 15° C. Keeping now the temperature of C′ C, I′ I exactly at 15°, let the temperature of J be raised to 25°. The difference of potentials between C′ C and I′ I would be increased to ·16148 volt, supposing ·16000 to have been exactly the difference of potentials when the temperature of J was 15°. This difference of differences of potentials would be just perceptible on the most delicate quadrant electrometer connected as indicated in the diagram. Lastly, raise the temperature of C′ C and I′ I to exactly 25°, J being still kept at this temperature: the spot of light of the electrometer will return exactly to its metallic zero. But, would the Volta-difference of potentials between the surfaces C′ C, I′ I remain unchanged, or would it return exactly to its previous value of ·16000, or would it come to some other value? We cannot answer this question without experiment. The

proper method, of course, would be to use the metal-sheathed Volta-condenser and compensation (§ 9 above), and with it measure the Volta-differences between copper and iron at different temperatures, the same for the two metals in each case. The sheath and everything in it should, in each experiment, be kept at one and the same constant temperature. But it would probably be very difficult to get a decisive answer, because of the uncertainties and time-lags of changes in the Volta-potential of metallic surfaces with change of temperature, which, if we may judge from Pellat's and Murray's experiments on effects of temperature when the two metals are unequally heated, would probably also be found when the temperatures of the two metals, kept exactly equal, are raised or lowered at the same time.

§ 21. The thermoelectric difference between bismuth and antimony is about ten times that between copper and iron for temperature differences of ten or twenty degrees on the two sides of 20° C., and their Volta-contact difference is exceedingly small (according to Pellat, just one one-hundredth of a volt when both their surfaces are strongly scratched by rubbing with emery). It would be very interesting, and probably instructive, to find how much their Volta-contact difference varies with temperature by the method at present suggested. The great variations of Volta-surface potentials, found by Pellat and Murray, when one of the two metals is heated, may have been due to difference of temperatures between the two opposed plates with air between them; and it is possible that no such large variation, or that large variation only due to changes of cohering gases, may be found when the two metals are kept at equal temperatures, and these temperatures are varied as in the experiment I am now suggesting.

§ 22. Peltier's admirable discovery (1834) of cold produced where an electric current crosses from bismuth to antimony, and heat where it crosses from antimony to bismuth, in a circuit of the two metals, with a current maintained through it by an independent electromotive force, is highly important in theory, or in attempts for theory, of the contact electricity of metals.

From an unsatisfactory* hypothetical application of Carnot's principle to the thermodynamics of thermoelectric currents I long ago inferred† that probably electricity crossing a contact between copper and iron in the direction from copper to iron would produce cold, and in the contrary direction heat when the temperature is below 280° C. (the thermoelectric neutral temperature of copper and iron),‡ and I verified this conclusion by experiment.§

* 'Mathematical and Physical Papers,' vol. i. art. xlviii. § 106, reprinted from 'Transactions of the Royal Society of Edinburgh,' May 1854.

† Ibid. § 116 (19).

‡ In a thermoelectric circuit of copper and iron the current is from copper to iron through hot when both junctions are below 280° C. It is from iron to copper through hot when both junctions are above 280° C.

§ 'Experimental Researches in Thermoelectricity,' Proc. R. S. May 1854; republished as art. li. in 'Mathematical and Physical Papers,' vol. i. (see pp. 164–165).

Hence we see, looking to Fig. 10, if the movable copper plate C D is allowed to move inwards (in the position shown in the diagram it is pulled inwards by the Volta-electrifications of the opposed surfaces of iron and copper), cold will be produced at the junction J, all the metal being at one temperature to begin with; and if we draw out the copper plate C D, heat will be produced at J. The thermodynamics of this action,* because it does not involve unequal temperatures in different parts of the metals concerned, is a proper subject for unqualified application of Carnot's law, and has nothing of the unsatisfactoriness of the thermodynamics of thermoelectric currents, which essentially involves dissipation of energy by conduction of heat through metals at different temperatures in different parts. At present we cannot enter further into thermodynamics than to remark that when the plate C D is drawn out, the heat produced at J is not the thermal equivalent of the work done by the drawing out of the copper plate, but in all probability is very much less than the thermal equivalent. Probably by far the greater part of the work spent in drawing out the plate against the electric attraction goes to storing up electrostatic energy, and but a small part of it is spent on heat produced at J; or on excess (positive or negative) of this Peltier heat above quasi-Peltier (positive or negative) absorptions of heat in the surface layers of the opposed surfaces when experiencing changes of electrification.

§ 23. Returning to Fig. 9; suppose, by electrodes connected to A B and an independent electromotive force, a current is kept flowing from copper to iron through one junction, and from iron to copper through the other; the Peltier heat produced where the current passes from iron to copper is manifestly not the thermal equivalent of the work done. In fact, if the two junctions be at equal temperatures the amounts of Peltier heat produced and absorbed at the two junctions will be equal, and the work done by the independent electromotive force will be spent solely in the frictional generation of heat.

§ 24. Many recent writers,† overlooking the obvious principles of §§ 22, 23, have assumed that the Peltier evolution of heat is the thermal equivalent of electromotive force at the junction. And in consequence much confusion, in respect to Volta's contact electricity and its relation to thermoelectric currents, has largely clouded the views

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* [March, 1898.] It has been given in a communication to the Royal Society of Edinburgh entitled 'The Thermodynamics of Volta-contact Electricity': Feb. 21, 1898.

† Perhaps following Clerk Maxwell, or perhaps independently. At all events we find the following in his splendid book of 1873: "Hence J Π represents the electromotive contact force at the junction acting in the positive direction. . . Hence the assumption that the potential of a metal is to be measured by that of the air in contact with it must be erroneous, and the greater part of Volta's electromotive force must be sought for, not at the junction of the two metals, but at one or both of the surfaces which separate the metals from the air or other medium which forms the third element of the circuit."—'Treatise on Electricity and Magnetism,' vol. i. § 249.

of teachers and students. We find over and over again the statement that thermoelectric electromotive force is very much smaller than the Volta-contact electromotive force of dry metals. The truth is, Volta-electromotive force is found between metals all of one temperature, and is reckoned in volts, or fractions of a volt, without reference to temperature. If it varies with temperature, its *variations* may be stated in fractions of a volt per degree. On the other hand, thermoelectric electromotive force depends essentially on difference of temperature, and is essentially to be reckoned *per degree*; as for example, in fraction of a volt per degree.

§ 25. Volta's second fundamental discovery, that is, his discovery (§ 5 above) that vitreous and resinous electricity flow away from zinc and copper to insulated metals connected with them (for example, the two electrodes of an insulated electrometer) when the two metals are separated after having been in metallic contact, makes it quite certain that there must be electric force in the air or ether in the neighbourhood of two opposed surfaces of different metals metallically connected. This conclusion I verified about thirty-six years ago by experiments described in a letter to Joule, of January 21, 1862, which he communicated to the Literary and Philosophical Society of Manchester, published in the Proceedings of the Society and in 'Electrostatics and Magnetism' (§ 400) under the title of "A New Proof of Contact-electricity."

§ 26. Volta's second fundamental discovery also makes it certain that movable pieces of two metals, metallically connected, attract one another, except in the particular case when their free surfaces are Volta-electrically neutral to one another. This force, properly viewed, is a resultant of chemical affinity between thin surface layers of the two metals. And the work done by it, when they are allowed to approach through any distance towards contact between any parts of the surfaces, is the dynamical equivalent of the portion of their heat of combination due to the approach towards complete chemical combination constituted by the diminution of distance between the two bodies. To fix the ideas, let the metals be two plane parallel plates of zinc and copper, with distance between them small in comparison with their diameters, and let us calculate the amount of the attractive force between them at any distance. Let V be the difference of potentials of the air or ether very near the two metallic frontiers, but at distances from these frontiers amounting at least to several times the distance from molecule to nearest molecule in either metal (see footnote on § 16 above). The electric force in air or ether between these surfaces will be V/D, if D denotes the distance between them. Hence (our molecular microscopic binocular set aside) if ρ is the electric density of either of the opposed surfaces, A the area of either of the two, and P the attraction between them, we have

$$\frac{V}{D} = 4\pi\rho, \quad P = \frac{1}{2}\rho\frac{V}{D}A.$$

Hence,

$$P = \frac{V^2 A}{8 \pi D^2}.$$

Hence the work done by electric attraction in letting them come from any greater distance asunder D′ to any smaller distance D is:—

$$\frac{V^2 A}{8 \pi}\left(\frac{1}{D} - \frac{1}{D'}\right), \text{ or approximately, } \frac{V^2 A}{8 \pi D},$$

if D is very small in comparison with D′.

§ 27. For clean sand-papered copper and zinc * we may take V as $\frac{3}{4}$ of a volt c.g.s. electromagnetic, or $\frac{1}{400}$ c.g.s. electrostatic.

Let now A be 1 sq. cm. and D, ·001 of a centimetre. We find P equal to ·249 dyne, and the work done by attraction to this distance from any much greater distance is ·000249. This is sufficient to heat $5 \cdot 9 \times 10^{-12}$ grammes of water, 1°.

The table on the next page shows corresponding calculated results for various distances ranging from 1/100 of a centimetre to $1/10^{10}$ of a centimetre.

Columns 5 and 6 are introduced to illustrate the relation between the electric attraction we are considering and chemical affinity as manifested by heat of combination. The "brass" referred to is an alloy of equal parts of zinc and copper, assumed to be of specific gravity 8 and specific heat ·093.

§ 28. It would be exceedingly difficult, if indeed possible at all, to show by direct experiment, at any distance whatever, the force of attraction between the discs; as we see from the table at a distance of 1/100 of a centimetre it amounts to only 1/400 of a milligramme-heaviness; and to only $2\frac{1}{2}$ grammes-heaviness at the distance 10^{-6} of a centimetre, which is about $\frac{1}{6}$ of the wave-length of ordinary yellow light. At the distances $10^{-7}, 10^{-8}, 10^{-9}$ of a centimetre the calculated forces of attraction are 25 kilogrammes, $2\frac{1}{2}$ tons,† and 250 tons. This last force is 2 or 3 times the breaking weight per square centimetre of the strongest steel (pianoforte wire), 6 times that of copper, 15 times that of zinc. We are, therefore, quite sure that the increase of attraction according to the inverse square of the distance is not continued to such small distances as 10^{-9} of a centimetre; and at distances less than this, the electric attraction merges into molecular force between the two metals.

* Pellat's measured values range from ·63 to ·92, according to the physical condition left by less or more violent scrubbing with emery paper. The mean of these numbers is ·77. Murray's range was still wider, from ·63 volt to 1·13, the smallest being for copper burnished, opposed to zinc scratched and polished with glass paper; and the largest, copper polished merely with emery paper, opposed to zinc polished and burnished.

† The metrical ton is about 2 per cent. less than (·984 of) the British ton in general use through the British empire for a good many years before 1890, but destined, let us hope, to be rarely if ever used after the 19th century, when the French metrical system becomes generally adopted through the whole world

ATTRACTING ONE ANOTHER.

1	2	3	4	5 (See § 27.)	6 (See § 27.)
Distance between plates.	Force of attraction in dynes.*	Work in ergs.*	Equivalent of W in heat-units (gramme-water-1° Cent.).	Heat-units per gramme of brass disc of thickness D and area 1 sq. cm.	Rise of temperature produced by giving H to copper and zinc discs of thickness ½ D, or to brass disc of thickness D and area 1 sq. cm. if specific heat constant at ·093.
D.	P.	W.	H.	H ÷ 8 D.	H ÷ (8 × D × ·093).
10^{-2} of centimetre	10^{-4} × 25 of dyne	10^{-4} × ·25 of erg	10^{-12} × ·59 of heat-unit		
10^{-3} „	10^{-2} × 25 „	10^{-3} × ·25 „	10^{-11} × ·59 „		
10^{-4} „	25 dynes	10^{-2} × ·25 „	10^{-10} × ·59 „		
10^{-5} „	10^{2} × 25 „	10^{-1} × ·25 „	10^{-9} × ·59 „		
10^{-6} „	10^{4} × 25 „	·25 „	10^{-8} × ·59 „	·00074	·0079°
10^{-7} „	10^{6} × 25 „	10 × ·25 ergs	10^{-7} × ·59 „	·074	·79°
10^{-8} „	10^{8} × 25 „	10^{2} × ·25 „	10^{-6} × ·59 „	7·4	79°
10^{-9} „	10^{10} × 25 „	10^{3} × ·25 „	10^{-5} × ·59 „	740	7,900°
10^{-10} „	10^{12} × 25 „	10^{4} × ·25 „	10^{-4} × ·59 „	74,000	790,000°

* The dyne is ·981 of a milligramme heaviness in the latitude of Greenwich. For approximate estimate it may be taken as 2 per cent. less than 1 milligramme heaviness in any latitude. The erg is the work done by a force of 1 dyne acting through the space of 1 centimetre.

§ 29. Consider, now, a large number of discs of zinc and copper, each of 1 square centimetre area, and thickness D, and polished on both sides. On one side of each disc attach three very small columns, of length D, of glass or other insulating material, and place one disc on top of the insulators of another, zinc and copper alternately, so as to make a dry insulated pile of the metal discs, separated by air spaces each equal to the thickness D. If in the building of this pile each disc is kept metallically connected with the one over which it is placed, while it is being brought into its position, work will be done upon it by electric attraction to the amount shown in column 3, and the total work of electric attraction during the building of the pile will be the amount shown in column 3, multiplied by one less than the number of discs.

But if each disc, after being metallically connected with the one on which it is to be placed, till it comes within some considerable distance—say 300 D, for example, from the disc over which it is to rest—is then disconnected and kept insulated while carried to its position in the pile, no work will be done on it by electric attraction. And if now, lastly, metallic connection is made between all the discs of the pile, currents pass from each copper to each zinc disc, and heat is generated to an amount equal to that shown in column 4, multiplied by one less than the number of discs; and if this heat is allowed to become uniformly diffused through the metals, they rise in temperature to the extent shown in column 6.

All these statements assume that the electric attraction increases according to the inverse square of the distance between opposed faces of zinc and copper. We have already (§ 28) seen that this assumption cannot be extended to such small distances as 10^{-9} of a centimetre. We have now further proof of this conclusion beyond the possibility of doubt, because the large numbers in columns 5 and 6 for 10^{-9} are enormously greater than any rational estimate we can conceive for the heat of combination of equal parts of zinc and copper per gramme of the brass formed. (See § 32 below.)

§ 30. When, on a Friday evening in February 1883—fourteen years ago—quoting from an article which had been published in *Nature* † in 1879, I first brought these views before the Royal Institution, we had no knowledge of the amount of heat of combination of zinc and copper, nor indeed of any other two metals. It appeared probable to us, from Volta's discovery of contact electricity between dry metals, that there must be some heat of combination; but I could then only express keenly-felt discontent with our ignorance of its amount. Now, however, after twenty-seven years' endurance, I am happily relieved since yesterday by Professor Roberts Austen, who most kindly undertook to help me in my preparations for this evening, with an investigation on the heat of combination of copper and zinc, by which he has found that the melting together of 30 per cent.

* 'Nature,' i. 551, "On the Size of Atoms."

of zinc with 70 per cent. of copper generates about 36 heat-units (gramme-water-Cent.) per gramme of the brass formed. I am sure you will all join with me in hearty thanks to him, both for this result and for his further great kindness in letting us now see a very beautiful experiment, demonstrating a large amount of heat of combination between aluminium and copper, in illustration of his mode of experimenting with zinc and copper, which could not be so conveniently put before you, because of the dense white fumes inevitable when zinc is melted in the open air.

[Experiment: A piece of solid aluminium dropped into melted copper: large rise of temperature proved by thermo-electric test. Result seen by all in large deflection of spot of light reflected from mirror of galvanometer.]

§ 31. Another method of investigating the heat of combination of metals, which I have long had in my mind, is to compare the heat evolved by the solution of an alloy in an acid with the sum of the heats of combination of its two constituents in mixed powders. The former quantity must be less than the latter by exactly the amount of the heat of combination. This investigation was undertaken a month ago by Mr. Galt, in the Physical Laboratory of the University of Glasgow, and he has already obtained promising results; but many experimental difficulties, as was to be expected, have presented themselves, and must be overcome before trustworthy results can be obtained.

[*Added Feb.* 1898.—By dissolving a gramme of a powdered alloy, and again a gramme of mixed powders of the two metals in the same proportion, in dilute nitric acid, Mr. Galt has now obtained approximate determinations of heats of combination for four different alloys, as shown in the following table:—

No.	Alloy.	Heat of combination per gramme of alloy in gramme-water-Cent. thermal units.
I.	.. {48 per cent. zinc / 52 ,, copper} .. (Approximately chemical combining proportions.)*	77
II.	.. {30 per cent. zinc / 70 ,, copper} ..	34·6
III.	.. {76·7 per cent. silver / 23·3 ,, copper} .. (Approximately chemical combining proportions.)*	18
IV.	.. {51·6 per cent. silver / 48·4 ,, copper} ..	7

* The combining proportions are—
(i) 50·8 zinc with 49·2 copper,
and (ii) 77·4 silver ,, 22·6 ,,

The composition stated for the alloy in each case is the result of chemical analysis. No. I. was intended to be equal parts of zinc and copper (as being approximately the chemically combining proportions); but the alloy, which resulted from melting together equal parts, was found to have 4 per cent. more copper than zinc, there having no doubt been considerable loss of the melted zinc by evaporation. No. III. turned out on analysis to be, as intended, very nearly in the chemically combining proportions of silver and copper. No. IV. was intended to be equal parts of silver and copper, but analysis showed the deviation from equality stated in the table. The proportions of No. II. were chosen for the sake of comparison with Professor Roberts Austen's result (§ 30), and the agreement (34·6 and 36) is much closer than could have been expected, considering the great difference of the two methods and the great difficulties in the way of obtaining exact results which each method presents.

From a chemical point of view it is interesting to see, from Mr. Galt's results, how much more, both in the case of copper and zinc, and copper and silver, the heat of combination is, when the proportions are approximately the chemically combining proportions, than when they differ from these proportions to the extents found in Alloys II. and IV. Mr. Galt intends, in continuance of his investigation, to determine as accurately as he can the heats of combination of many different alloys of zinc and copper and of silver and copper, and so to find whether or not it is greatest when the proportions are exactly the chemically "combining proportions." He hopes also to make similar experiments with bismuth and antimony, using *aqua regia* as solvent.]

[§ 32. *February* 1898.—Looking now to column 5 of the table of § 27, we see from Professor Roberts Austen's result, 36 thermal units, for the heat of combination of 30 per cent. copper with 70 per cent. zinc, and from Galt's 77 thermal units for equal parts of copper and zinc, that the law of electric action on which the calculations of the tables are founded is utterly disproved for discs of metal of one one-thousand-millionth of a centimetre thickness, with air or ether spaces between them of the same thickness, but is not disproved for thicknesses of one one-hundred millionth of a centimetre.

Consider now our ideal insulated pile (§ 29) of discs 10^{-8} of a centimetre thick, with air or ether spaces of the same thickness between them. Suddenly establish metallic connection between all the discs. The consequent electric currents will generate 7·4 thermal units, and heat the discs by 79° C. Take again the insulated column with thicknesses and distances of 10^{-8} of a centimetre; remove the ideal glass separators and diminish the distance to 10^{-9} of a centimetre (the thicknesses of discs being still 10^{-8} of a centimetre). Now, with these smaller distances between two opposed areas, make metallic contact throughout the column by bending the corners (the discs for convenience being now supposed square); 74 thermal units will be

immediately generated, and the discs will rise 790° in temperature, and we have a column of hot brass—perhaps solid, perhaps liquid. This last statement assumes that the law of electric action, on which the table is founded, holds for discs 10^{-8} of a centimetre thick, with ether or air spaces between them of 10^{-9} of a centimetre. In reality it is probable that the law of electric action for discs 10^{-8} of a centimetre thick, begins to merge into more complicated results of intermolecular forces, before the distance is as small as 10^{-9} of a centimetre.

Resuming our mental molecular microscopic binocular (§ 16, footnote), we cannot avoid seeing molecular structures beginning to be perceptible at distances of the hundred-millionth of a centimetre, and we may consider it as highly probable that the distance from any point in a molecule of copper or zinc to the nearest corresponding point of another molecule is less than one one-hundred-millionth, and greater than one one-thousand-millionth of a centimetre.]

§ 33. In all that precedes I have, by frequent repetition of the phrase "air or ether," carefully kept in view the truth that the *dry* Volta contact-electricity of metals is, in the main, independent of the character of the insulating medium occupying space around and between the metals concerned in each experiment, and depends essentially on the chemical and physical conditions of molecules of matter in the thin surface stratum between the interior homogeneous metal and the external space, occupied by ether and dry or moist atmospheric air or any gas or vapour which does not violently attack the metal: or by ether with vapours only of mercury and glass and platinum and steel and vaseline (caulking the glass-stopcocks), as in Bottomley's experiments (§ 14 above).

This truth has always seemed to me convincingly demonstrated by Volta's own experiments, and I have never felt that that conviction needed further foundation; though of course I have not considered quite needless or uninstructive, Pfaff's and my own and Pellat's repetitions and verifications, in different gases at different pressures, and Bottomley's extension of the demonstration to vacuum of 2½ millionths of an atmosphere. I am now much interested to see by Professor Oliver Lodge's report, already referred to (§ 4 above), that in the Bakerian Lecture to the Royal Society in 1806,* Sir Humphry Davy, who had had contemporaneous knowledge of Volta's first and second discoveries, expressed himself thus clearly as to the validity of the second: "Before the experiments of M. Volta on the electricity excited by mere contact of metals were published, I had to a certain extent adopted this opinion," an opinion of Fabroni's; "but the new fact immediately proved that another power must necessarily be concerned, for it was not possible to refer the electricity exhibited by the opposition of metallic surfaces to any chemical alterations, particularly as the effect is more distinct in a dry atmo-

* Phil. Trans. 1807.

sphere, in which even the most oxidisable metals do not change, than in a moist one, in which many metals undergo oxidation."

§ 34. It is curious to find, thirty or forty years later, De la Rive explaining away Volta's second discovery by moisture in the atmosphere! Fifty-one years ago, when I first learned Volta's second discovery, by buying, in Paris, apparatus by which it has ever since been shown in the ordinary lectures of my class in the University of Glasgow, I was warned that De la Rive had found it wrong, and had proved it to be due to oxidation of the zinc by moisture from the air. I soon tested the value of this warning by the experiments of § 5 above, and a considerable variety of equivalent experiments, in one of which (real or ideal, I cannot remember which), a varnished zinc disc, scratched in places and moistened, sometimes on the scratched parts and sometimes where the varnish was complete, was tested in the usual manner by separating from contact with an unvarnished or varnished copper disc, with or without metallic connection when the discs were at their nearest.

[§§ 35–40 *are added in Feb.* 1898.]

§ 35. Within the last eighteen or twenty years there has been a tendency among some writers to fall back upon De la Rive's old hypothesis, of which there are signs in expressions quoted by Professor Oliver Lodge in his great and valuable report of 1884, and in some statements also of Professor Lodge's own views.

In what is virtually a continuation of this report in the 'Philosophical Magazine' a year later,* we find the following with reference to writings of Helmholtz and myself on the contact-electricity of metals: "Both these contact theories, in explaining the Volta effect, ignore the existence of the oxidising medium surrounding the metals. My view explains the whole effect as the result of this oxygen bath, and of the chemical strain by it set up." With views seemingly unchanged, he returned to the subject at the end of 1897 with the following statement in the printed syllabus of his 'Six Lectures adapted to a Juvenile Auditory, on the Principles of the Electric Telegraph' (Royal Institution, Dec. 28, 1897, Jan. 8, 1898).

"Chemical method of producing a current—Voltaic cell—Two "differently oxidisable metals immersed in an oxidising liquid and "connected by a wire can maintain an electric current, through the "liquid and through the wire, so long as the circuit is closed. [The "same two metals immersed in a potentially oxidising gas and con-"nected by a wire, can maintain an electric force or voltaic difference "of potential in the space between them.]

"N.B.—No one need try too hard to understand sentences in brackets."

And lastly, after some correspondence which passed between us

† Prof. O. Lodge 'On the Seat of the Electromotive Force in a Voltaic Cell,' Phil. Mag. Oct. 1885, p. 383.

in December, I have to-day (Feb. 14), received from him a "slightly amplified statement made in order to concentrate the differences," which he kindly gives me for publication as a supplement to the shorter statement from the syllabus.

Amplification, February, 1898.

"There is a true contact-force at a zinc-copper junction,* which "on a simple and natural hypothesis (equivalent to taking an inte-"gration-constant as zero) can be measured thermoelectrically † and "is about ¼ millivolt at 10° C.

"A voltaic force, more than a thousand times larger,† exists at "the junction of the metals with the medium surrounding them; and "in an ordinary case is calculable as the difference of oxidation-"energies of zinc and copper; but it has nothing to do with the heat "of formation of brass.

"*References*:
"Phil. Mag. [5].
"vol. xix. pp. 360 and 363, brass and atoms, pp. 487 and 494, summary.
"vol. xxi. pp. 270 and 275, thermoelectric argument.
"vol. xxii. p. 71, Ostwald experiment.
"August 1878, Brown experiment."

§ 36. With respect to the first of the two paragraphs of this last statement and the first two lines of the second, the wrongness of the view there set forth is pointed out in § 24 above. With respect to the last clause of the second paragraph and the statement quoted from the syllabus, I would ask any reader to answer these questions:—

(i.) What would be the efficacy of the supposed oxygen bath in the experiments of § 2 above with varnished plates of zinc and copper? or in Erskine Murray's experiment, described in his paper communicated last August to the Royal Society, in which metallic surfaces, scraped under melted paraffin so as to remove condensed oxygen or nitrogen from them, and leave fresh metallic surfaces in contact with a hydro-carbon, are subjected to the Voltaic experiment? or in Pfaff's and my own and Pellat's experiments with different gases, at ordinary and at low pressures, substituted for air? or in Bottomley's high vacuum and hydrogen and oxygen experiments (§ 14 above)?

(ii.) What would be the result of Volta's primary experiment, shown at the commencement of my lecture (§ 1 above), if it had been performed in some locality of the universe a thousand kilometres away from any place where there is oxygen? The insulators may be supposed to be made of rock-salt or solid paraffin, so that there may be no oxygen in any part of the apparatus. This I say because I understand that some anti-Voltaists have explained Bottomley's

* See footnote on § 16 above. K. Feb. 14, 1898.
† See § 24 above. K. Feb. 14, 1898.

experiments by the presence of vapour of silica from the glass, supplying the supposedly needful oxygen!

§ 37. The anti-Voltaists seem to have a superstitious veneration for oxygen. Oxygen is entitled to respect because it constitutes 50 per cent. of all the chemical elements in the earth's crust; but this gives it no title for credit as coefficient with zinc and copper in the dry Volta experiment, when there is none of it there. Oxygen has more affinity for zinc than for copper; so has chlorine and so has iodine. It is partially true that different metals—gold, silver, platinum, copper, iron, nickel, bismuth, antimony, tin, lead, zinc, aluminium, sodium—are for dry Volta contact electricity in the order of their affinities for oxygen; but it is probably quite as nearly true that they are in the order of their affinities for sulphur, or for oxysulphion (SO_4) or for phosphorus or for chlorine or for bromine. It may or may not be true that metals can be unambiguously arranged in order of their affinities for any of these named substances; it is certainly true that they cannot be *definitely and surely* arranged in respect to their dry Volta contact-electricity. Murray's burnishing, performed on a metal which has been treated with Pellat's washing with alcohol and subsequent scratching and polishing with emery, alters the Volta quality of its surface far more than enough to change it from below to above several metals polished only by emery; and, in fact, Pellat had discovered large differences due to molecular condition without chemical difference, before Murray extended this fundamental discovery by finding the effect of burnishing.

§ 38. Returning to Professor Lodge's supposed oxygen bath (§ 35): if it exists between the zinc and copper plates, it diminishes or annuls or reverses the phenomenon, to explain which he invokes its presence (see § 5 above).

§ 39. Many years ago I found that ice, or hot glass, pressed on opposite sides by polished zinc and copper, produced deviations from the metallic zero of the quadrants of an electrometer metallically connected with them in the same direction as if there had been water in place of the ice or hot glass. From this I inferred that ice and hot glass, both of which had been previously known to have notable electric conductivity, acted as electrolytic conductors.

Experiments made by Maclean and Goto in the Physical Laboratory of the University of Glasgow in 1890,* proved that polished zinc and polished copper, with fumes passing up between them from the flame of a spirit-lamp 30 centimetres below, gave, when metallically connected to the quadrants of an electrometer, deviations from the metallic zero in the same direction, and of nearly the same amount, as if cold water had been in place of the flame. This proved that flame acted as an electrolytic conductor. They also found that hot air from a large red-hot soldering bolt, put in the place of the spirit lamp, had no such effect; nor had breathing upon the plates, nor the vapour

* Phil. Mag. Aug. 1890.

of hot water, any effect of the kind. In fact hot air, and either cloudy or clear steam, act as very excellent insulators; but there is some wonderful agency in fumes from a flame, remaining even in cooled fumes, in virtue of which the electric effect on zinc and copper is nearly the same as if continuous water, instead of fumes, were between the plates and in contact with both.*

A similar conclusion in respect to air traversed by ultra-violet light was proved by Righi, † Hallwachs, ‡ Elster and Geitel, § Branly. ‖ The same was proved for ordinary atmospheric air, with Röntgen rays traversing it between plates of zinc and copper, by Mr. Erskine Murray, in an experiment suggested by Professor J. J. Thomson, and carried out in the Cavendish Laboratory of the University of Cambridge.¶

§ 40. The substitution for ordinary air between zinc and copper, of ice or hot glass, or of air or gas modified by flame or by ultra-violet rays, or by Röntgen rays, or by uranium (§§ 41, 42 below), gives us, no doubt, what would to some degree fulfil Professor Lodge's idea of a "potentially-oxidising" gas, and each one of the six fails wholly or partially to "maintain electric force or voltaic difference of potential in the space between them." In fact, Professor Lodge's bracketed sentence, so far as it can be understood, would be nearer the truth if in it "cannot" were substituted for "can." I hope no reader will consider this sentence too short or sharp. I am quite sure that Professor Lodge will approve of its tone, because in his letter to me of the 14th, he says, "In case of divergence of view it is best to have both aspects stated as crisply and distinctly as possible, so as to emphasise the difference." I wish I could also feel sure that he will agree with it, but I am afraid I cannot, because in the same letter he says, "I am still unrepentant."

Continuation of Lecture of May 21, 1897.

§ 41. In conclusion, I bring before you one of the most wonderful discoveries of the century now approaching its conclusion, made by the third of three great men, Antoine Becquerel, Edmond Becquerel, Henri Becquerel—father, son and grandson—who by their inventive genius and persevering labour have worthily contributed to the total of the scientific work of their time; a total which has rendered the nineteenth century more memorable than any one of all the twenty-three centuries of scientific history which preceded it, excepting the seventeenth century of the Christian era.

You see this little box which I hold in my right hand, just as I received it three months ago from my friend Professor Moissan, who will be here this day week to show you his isolation of fluorine. It

* Kelvin and Maclean, R.S.E. 1897.
† Rend. R. Acc. dei Lincei, 1888, 1889.
‡ Wiedemann's Annalen, 34, 1888.
§ Ibid. 38, 41, 1888.
‖ Comptes Rendus, 1888, 1890.
¶ Proc. R.S. March 1896.

induces electric conductivity in the air all round it. If I were to show you an experiment proving this, you might say it is witchcraft. But here is the witch. You see, when I open the box, a piece of uranium of about the size of a watch. This production of electric conductance in air is only one of many marvels of the "uranium rays" discovered a year ago by Henri Becquerel, of no other of which can I now speak to you, except that the wood and paper of this box, and my hand, are to some degree transparent for them.

I now take the uranium out of its box and lay it on this horizontal copper plate, fixed to the insulated electrode of the electrometer. I fix a zinc plate, supported by a metal stem which is in metallic connection with the sheath of the electrometer, horizontally over the copper plate at a distance of about one centimetre from the top of the uranium. Look at the spot of light; it has already settled to very nearly the position which you remember it took when we had a water-arc between the copper and zinc plates, connected as now, copper to insulated quadrants and zinc to the sheath. I now lift the uranium, insulating it from the copper plate by three very small pieces of solid paraffin, so as to touch neither plate, or, again, to touch the zinc but not the copper. This change makes but little difference to the spot of light. I tilt the uranium now to touch the zinc above and the copper below; the spot of light comes to the metallic zero as nearly as you can see. I leave it to itself now, resting on its paraffin supports and not touching the zinc, and the spot of light goes back to where it was; showing about three-quarters of a volt positive.

§ 42. I now take this copper wire, which is metallically connected with the zinc plate and the sheath of the electrometer, and bring it to touch the under side of the copper shelf on which the uranium is supported by its paraffin insulators. Instantly the spot of light moves towards the metallic zero, and after a few vibrations settles there. I break the contact; instantly the spot of light begins to return to its previous position, where it settles again in less than half a minute. You see, therefore, that if I re-make and keep made the metallic contact between the zinc and copper plates, a current is continuously maintained through the connecting wire, by which heat is generated and radiated away, or carried away by the air; as long as the contact is kept made. What is the source of the energy thus produced? If we take away the uranium, and send cool fumes from a spirit-lamp, or shed Röntgen rays or ultra-violet light, between the zinc and copper, the results of breaking and making contact would be just what you see with uranium. So would they be—you have already, in fact, seen them (§ 5)—without either Röntgen rays or ultra-violet light, but with the copper and zinc a little closer together and with a drop of water between them: and so would they be with dry ice, or with hot glass, between and touched by the zinc and copper. In each of these six cases we have a source of energy; the well-known electro-chemical energy given by the oxidation of zinc in the last

mentioned three cases; and the energy drawn upon by the cooled fumes, or by the Röntgen rays or ultra-violet light, acting in some hitherto unexplained manner, in the three other cases. We may conjecture evaporations of metals; we have but little confidence in the probability of the idea. Or does it depend on metallic carbides mixed among the metallic uranium? I venture on no hypothesis. Mr. Becquerel has given irrefragable proof of the truth of his discovery of radiation from uranium of something which we must admit to be of the same species as light, and which may be compared with phosphorescence. When the energy drawn upon by this light is known, then, no doubt, the *quasi* electrolytic phenomena, induced by uranium in air,* which you have seen, will be explained by the same dynamical and chemical principles as those of the previously known electrolytic action of cooled fumes from a spirit-lamp, and of air traversed by Röntgen rays or ultra-violet light.

Appendix.

On a Method of Measuring Contact Electricity.†

In my reprint of papers on Electrostatics and Magnetism (§ 400, of original date, January 1862) I described briefly this method, in connection with a new physical principle, for exhibiting contact electricity by means of copper and zinc quadrants substituted for the uniform brass quadrants of my quadrant electrometer. In an extensive series of experiments which I made in the years 1859–61, I had used the same method, but with movable discs for the contact electricity, after the method of Volta, and my own quadrant electrometer substituted for the gold-leaf electroscope by which Volta himself obtained his electric indications.

I was on the point of transmitting to the Royal Society a paper which I had written describing these experiments, and which I still have in manuscript, when I found a paper by Hankel in Poggendorf's 'Annalen' for January, 1862, in which results altogether in accordance with my own were given, and I withheld my paper till I might be able not merely to describe a new method, but if possible, add something to the available information regarding the properties of

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* Experiments made in the Physical Laboratory of the University of Glasgow [§ 33 of Kelvin, Beattie and Smolan, Proc. R.S.E.; also 'Nature,' March 11, 1897, and Phil. Mag. March 1898] show this electrolytic conductivity to be produced by uranium to nearly the same amount in common air oxygen and carbonic acid; and to about one-third of the same amount in hydrogen, at ordinary atmospheric pressure; but only to about $\frac{1}{150}$ of this amount in each of these four gases at pressures of 2 or 3 millimetres. There seems every reason to believe that it would be non-existent in high vacuum, such as that reached by Bottomley in his Volta-contact experiments (§ 14 above).

† First published in the British Association, Swansea meeting, August 1880, and 'Nature,' April 4, 1881.

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matter to be found in Hankel's paper. I have made many experiments from time to time since 1861 by the same method, but have obtained results merely confirmatory of what had been published by Pfaff in 1820 or 1821, showing the phenomena of contact electricity to be independent of the surrounding gas, and agreeing in the main with the numerical values of the contact differences of different metals which Hankel had published; and I have therefore hitherto published nothing except the slight statements regarding contact electricity which appear in my 'Electrostatics and Magnetism.' As interest has been recently revived in the subject of contact electricity, the following description of my method may possibly prove useful to experimenters. The same method has been used to very good effect, but with a Bohnenberger electroscope instead of my quadrant electrometer, in researches on contact electricity by Mr. H. Pellat, described in the 'Journal de Physique' for May 1880.

The apparatus used in these experiments was designed to secure the following conditions: To support, within a metallic sheath, two circular discs of metal about four inches in diameter in such a way that the opposing surfaces should be exactly parallel to each other and approximately horizontal, and that the distance between them might be varied at pleasure from a shortest distance of about one-fiftieth of an inch to about a quarter or half an inch. This part of the apparatus I have called a "Volta-condenser." The lower plate, which was the insulated one, was fixed on a glass stem rising from the centre of a cast-iron sole plate. The upper plate was suspended by a chain to the lower end of a brass rod sliding through a steadying socket in the upper part of the sheath. An adjustable screw on this stem prevents the upper plate from being let down to nearer than about one-fiftieth of an inch, or whatever shortest distance may be wanted in any particular case. A stout brass flange fixed to the lower end of this rod bears three screws, one of which S is shown in the drawing, by which the upper plate can be adjusted to parallelism to the lower plate. The other apparatus used consisted of a quadrant electrometer, and in my original experiments an ordinary Daniell's cell, in my later ones a gravity Daniell's cell of the form which I described in 'Proc. R.S.' 1871 (pp. 253–259), with a divider by which any integral number of per cents. from 0 to 100 of the electromotive force of the cell could be established between any two mutually insulated homogeneous metals in the apparatus.

Connections.—The insulated plate was connected by a brass wire passing through the case of the Volta-condenser to the electrode of the insulated pair of quadrants. The upper plate was connected to the metal sheath of the Volta-condenser, and to the metal case of the electrometer, one pair of quadrants of which were also connected to the case. One of the two terminals of the divider, connected to the poles of the cell, was connected to the case of the electrometer. To the third terminal (the bar carrying the slider) was attached one of the contact wires, which was a length of copper wire having soldered

to its outer end a short piece of platinum. The other contact surface was a similar short piece of platinum fixed to the insulated electrode of the electrometer. Hence it will be seen that metallic connection between the two plates was effected by putting the divider at zero and bringing into contact the two pieces of platinum wire.

Order of Experiment.—The sliding piece of the divider was put to zero, and contact made and broken, and the upper plate raised: then the deflection of the spot of light was observed. These operations were repeated with the sliding piece at different numbers on the divider scale, until one was found at which the make-break and separation caused no perceptible deflection. The number thus found on the divider scale was the percentage of the electromotive force of the Daniell cell, which was equal to the contact electric difference of the plates in the Volt-condenser.

[*Addendum*, November 23, 1880.—Since the communication of this paper to the British Association, I have found that a dry platinum disc, kept for some time in dry hydrogen gas, and then put into its position in dry atmospheric air in the apparatus for contact electricity, becomes positive to another platinum disc which had not been so treated, but had simply been left undisturbed in the apparatus. The positive quality thus produced by the hydrogen diminishes gradually, and becomes insensible after two or three days.]

P.S.—On December 24, 1880, one of two platinum plates in the Volta-condenser was taken out; placed in dried oxygen gas for forty-five minutes; taken out, carried by hand, and replaced in the Volta-condenser at 12.30 on that day. It was then found to be negative to the platinum plate, which had been left undisturbed. The amount of the difference was about ·33 of a volt. The plates were left undisturbed for seventeen minutes in the condenser, and were then tested again, and the difference was found to have fallen to ·29 of a volt. At noon on the 25th they were again tested, and the difference found to be ·18. The differences had been tested from time to time since that day, the plates having been left in the condenser undisturbed in the intervals. The following table shows the whole series of these results:—

Time.	Electric difference between surfaces of a platinum plate in natural condition, and a platinum plate after 45 minutes' exposure to dry oxygen gas.
Dec. 24, 12.30 p.m.	·33 of a volt.
24, 12.47 p.m.	·29 „
25, noon	·18 „
27, noon	·116 „
28, 11.20 a.m.	·097 „
31, noon	·047 „
Jan. 4, 11.0 a.m.	·042 „
11, 11.40 a.m.	·020 „

Mr. Rennie, by whom these experiments were made during the recent Christmas holidays, had previously experimented on a platinum

plate which had been made the positive pole in an electrolytic cell with an electromotive force of one volt, tending to decompose water acidulated with sulphuric acid; the other pole being a piece of platinum wire. After the plate had been one hour under this influence in the electrolytic cell he removed it, and dried it by lightly rubbing it with a piece of linen cloth. He then placed it in the Volta-condenser, and found it to be negative to a platinum plate in ordinary condition; the difference observed was ·27 of a volt. This experiment was made on October 21; and on November 8 it was found that the difference had fallen from ·27 to ·07. Mr. Rennie also made similar experiments with the platinum disc made the negative pole in an electrolytic cell, and found that this rendered the platinum positive to undisturbed platinum to a degree equal to about ·04 of a volt. The effect of soaking the platinum plate in dry hydrogen gas, alluded to in my first postscript, which also was observed by Mr. Rennie, was found to be about ·11 of a volt. Thus in the case of polarisation by hydrogen, as well as in the case of polarisation by oxygen, the effect of exposure to the dry gas was considerably greater than the effect of electro-plating the platinum with the gas by the electromotive force of one volt.

[K.]

WEEKLY EVENING MEETING,

Friday, January 22, 1897.

Sir Frederick Bramwell, Bart. D.C.L. LL.D. F.R.S. Honorary Secretary and Vice-President, in the Chair.

Professor Dewar, M.A. LL.D. F.R.S. *M.R.I.* Fullerian Professor of Chemistry R.I.

Properties of Liquid Oxygen.

aseous Absorption.—During recent years a great deal of research been directed to the study of what may be called the low tempera- absorption spectrum of gaseous and liquid oxygen. It has been n that gaseous oxygen gives two types of absorption spectrum, one posed of a number of well-defined groups of lines of exquisite sym- y, like the great groups A and B of the solar spectrum, the other nds relatively broad and more or less black. The band spectrum is cially marked in gaseous oxygen under high pressure, and Janssen shown that the intensity of absorption in different columns of under different pressure is identical when the length multiplied the square of the density is the same in each case. The band is most easily seen is one in the yellow, and, in order just to see 8 metres of oxygen under 11 atmospheres pressure (or 11 times density under ordinary pressure) must be traversed by white light re it enters the spectroscope. From this result and Janssen's law given, it follows that in order to detect the same band in a mn of gaseous oxygen at atmospheric pressure, it would require to 2178 metres long or about 1¼ miles. The question arises what ld be the length of an oxygen tube at atmospheric pressure, equi- nt to the absorption of a beam passing vertically through the h's atmosphere. This problem has been answered by Janssen, has shown that an oxygen column 172 metres long would have a lar action. It follows at once from this result that the band in yellow cannot be seen in the spectrum of the midday sun, as ould require a column of oxygen at least twelve times longer in r to make it visible; but that it ought to be seen provided the was observed near the horizon. When the sun is 4° above the zon, the depth of atmosphere the rays have to penetrate is about ve times that of the zenithal thickness. This theoretical result sen has confirmed by a series of observations made at sunrise he dry air of the Desert of Sahara.

Liquid Absorption.—Both types of spectra are well marked in the trum of liquid oxygen, the only marked difference being that the

liquid absorption known as A and B of Frauenhoffer appear now as bands with sharp edges on the less refrangible side, fading away gradually towards the more refrangible, which is just the opposite character to that of the gaseous absorption of the same groups. The change from the gaseous to the liquid state has not caused any material alteration in the general character of the absorption from what it was under high gaseous compression. The question may therefore naturally be put, does Janssen's law expressing the relation of absorption and density in the gaseous state extend to the liquid condition? This may be answered by calculating what thickness of the liquid at its boiling point, taken as being 800 times denser than the gas at ordinary temperatures, would be required (provided the same law held) to render visible the absorption band in the yellow. The resulting number is about 3·4 mm., and this is confirmed by laboratory experiments which show that between 3 and 4 mm. thickness of liquid oxygen at $-183°$ is sufficient to cause the appearance of this band. Thus it appears Janssen's law extends to the liquid condition, the square of the density still defining the intensity of the absorption. It is probable that the band spectrum has its origin either in complex molecules generated by condensation, or it may originate from encounters between molecules of the ordinary mass which become more frequent when the free path is diminished. The following table gives the results of observations (made with my colleague Prof. Liveing) in order to find the gaseous pressure required to originate definite absorption bands together with some data of liquid absorption.

Wave-Length of Band.	Atmospheric Pressure. 18-metre tube.	Atmospheric Pressure. 1·65-metre tube.	Atmospheric Pressure. 3178 metres tube.	Thickness of Liquid.
A	1	20	..	30 mm.
B	12	40		
5785 (yellow band)	11	35	1	3 to 4 mm.
6300 } 4700 }	20			
5350 } 4470 }	30	110		

The gaseous oxygen in the 1·65-metre tube under 85 atmospheres compression appears to be very transparent for violet and ultra-violet up to the wave-length 2745, or about the limit of the magnesium spark spectrum. When the pressure was increased to 140 atmospheres the ultra-violet absorption was complete beyond wave-length 2704. In the 18-metre tube with the oxygen under 90 atmospheres pressure, a faint absorption band appeared about L of the solar spectrum, a strong one between 3640 and 3600 wave-length, and a

diffuse band about the solar line O with complete absorption beyond P. The intensity of the absorption in the latter case was, following Janssen, $4\frac{1}{2}$ times what it was under the highest pressure in the short tube. From this we should infer that in the liquid state medium thicknesses like a centimetre or two would be transparent to the ultra-violet, but depths of 10 to 20 cm. would become more and more opaque. Actual experiments confirm this suggestion.

Thus the passage of light through a layer of liquid 3 to 4 mm. thick is sufficient to cause visible absorption in the yellow, while it requires more than five hundred thousand times this thickness of oxygen gas at atmospheric pressure to do the same thing. Provided the density of the oxygen gas is much below that corresponding to the atmosphere, then an enormous thickness of gaseous oxygen would be required to cause any visible absorption. This may explain why such a spectrum is not shown in sunlight, quite independently of the earth's atmosphere, provided we assume that any oxygen in the solar atmosphere must have a relatively small density.

Absorption of Liquid Air.—If the surface of the earth was cooled to below $-200°$ C. then the atmosphere would liquefy, and the ocean of liquid air would form a depth of about 30 to 35 feet. The actual proportionate depth can be experimentally observed by taking a tube about 52 feet long, or about $\frac{1}{500}$th part of the height of the homogeneous atmosphere, and cooling one end to $-210°$, when about $\frac{3}{4}$ inch of liquid is obtained. Of this liquid air layer, about 6 to 7 feet may be taken as the equivalent of the oxygen portion. A question of considerable interest arises as to the effect of the presence of liquid nitrogen on the oxygen absorption; although nitrogen is colourless yet the dilution of the liquid oxygen in a neutral solvent has altered the concentration of the colour-absorbing medium. In order to examine into this matter Professor Liveing and the author compared the absorption of 1·9 cm. of liquid air with 0·4 cm. of liquid oxygen, or the proportionate thickness of oxygen which the layer of 1·9 cm. of liquid air contains. The light which had passed through the latter was, by means of a reflecting prism, brought into the field of view of the spectroscope at the same time with that which had passed through the liquid air. The positions of the lamps were then adjusted so that the brightness of the spectra of those parts where there were no absorption bands was equal in the two spectra. Under these circumstances it was seen that the absorption bands were very much more strongly developed by 0·4 cm. of liquid oxygen than by five times that thickness of liquid air.

Another sample of liquid air was rapidly mixed with an equal volume of liquid oxygen, and the absorption of this liquid compared as before with that of liquid oxygen. It was seen that the absorption of 2·4 cm. of the mixture was much greater than that of 0·4 cm. of liquid oxygen. The density of the liquid oxygen in the mixture was, in fact, three times that in pure liquid air, and by an extension of Janssen's law to liquid mixtures the absorption should have been

increased ninefold. The observations, so far as they go, accord with this theory. In order to examine the effect of temperature, the absorption of a thickness of 3 cm. of liquid oxygen boiling under 1 cm. pressure, or at a temperature of $-210°$, was compared with a like thickness of the liquid boiling at atmospheric pressure. With the colder liquid the bands in the orange and yellow were sensibly widened, mainly on the more refrangible side; the faint band in the green was plainly darker, and the band in the blue appeared somewhat stronger. The difference between the temperatures of the two liquids was about 27°, or approaching to one-third the absolute boiling-point of oxygen. The density of oxygen at $-210°$ C. is not known, but in any case it is greater than that at $-183°$ C., and an increased absorption of about one-fourth by the cooling might be anticipated.

At the low temperature reached by the use of a hydrogen jet taken in liquid air, the latter solidifies into a hard white solid resembling avalanche snow. The solid has a pale bluish colour, showing by reflection all the absorption bands of the liquid.

The refractive power of the liquid, as determined by Prof. Liveing and the author, was given in a previous lecture.* Later investigations resulted in the determination of the dispersive power. The refractive constant of the liquid oxygen was found to be almost identical with Mascart's value for the gas, and similarly the dispersive constant in the liquid and gas seems to be identical.

Magnetic Properties of Liquid Oxygen.

The remarkable magnetic properties of liquid oxygen were described to the Royal Institution in a lecture delivered in 1892.† Professor Fleming and myself have for some time past directed our attention to the question of determining the numerical values of the magnetic permeability and magnetic susceptibility of liquid oxygen,‡ with the object of determining not only the magnitude of these physical constants, but also whether they vary with the magnetic force under which they are determined.

Although a large number of determinations have been made by many observers of the magnetic susceptibility of different liquids taken at various temperatures, difficulties of a particular kind occur in dealing with liquid oxygen. One method adopted for determining the magnetic susceptibility of a liquid is to observe the increase of mutual induction of two conducting circuits suitably placed, first in air, and then when the air is replaced by the liquid in question, the

* "Liquid Atmospheric Air," Proc. Roy. Inst. 1893.

† See Roy. Inst. Proc. June 15th, 1892, "On the Magnetic Properties of Liquid Oxygen." Friday evening discourse, by Professor J. Dewar, F.R.S.

‡ Proc. Roy. Soc. vol. lx. 1896, p. 283, "On the Magnetic Permeability of Liquid Oxygen and Liquid Air," by Professor J. A. Fleming, F.R.S. and Professor J. Dewar, F.R.S.

susceptibility of which is to be determined. A second method consists in determining the mechanical force acting on a known mass of the liquid when placed in a non-uniform magnetic field. Owing to the difficulty of preventing entirely the evaporation of liquid oxygen, even when contained in a good vacuum vessel, and the impossibility of sealing it up in a bulb or tube, and having regard to the effect of the low temperature of the liquid in deforming by contraction and altering the conducting power of coils of wire placed in it, it was necessary to devise some method which should be independent of the exact constancy in mass of the liquid gas operated upon, and independent also of slight changes in the form of any coils of wire which might be used in it. After many unsuccessful preliminary experiments the method which was finally adopted by Professor Fleming and myself as best complying with the conditions introduced by the peculiar nature of the substance operated upon is as follows:—

A small closed circuit transformer was constructed, the core of which could be made to consist either of liquid oxygen or else immediately changed to gaseous oxygen, having practically the same temperature. This transformer consisted of two coils, the primary coil was made of forty-seven turns of No. 12 S.W.G. wire; this wire was wound into a spiral having a rectangular shape, the rectangular turns having a length of 8 cm. and a width of 1·8 cm. This rectangular-sectioned spiral, consisting of one layer of wire of forty-seven turns, was bent round a thin brass tube, 8 cm. long and $2\frac{1}{2}$ cm. in diameter, so that it formed a closed circular solenoid of one layer of wire. The wire was formed of high conductivity copper, doubly insulated with cotton, and each single turn or winding having a rectangular form.

The turns of covered wire closely touched each other on the inner circumference of the toroid, but on the external circumference were a little separated, thus forming apertures by which liquid could enter or leave the annular inner core.

The nature of this transformer is shown in Fig. 1.

The mean perimeter of this rectangular-sectioned endless solenoid was $13\frac{1}{2}$ cm. and the solenoid had, therefore, very nearly 3·5 turns per cm. of mean perimeter. When immersed in liquid oxygen a coil of this kind will carry a current of 50 ampères. When a current of A ampères is sent through this coil the mean magnetising force in the axis of this solenoid is, therefore, represented by 4·375 times the current through the wire, hence it is clear that it is possible to produce in the interior of this solenoid a mean magnetising force of over 200 C.G.S. units. This primary coil had then wound over it, in two sections, about 400 or 500 turns of No. 26 silk-covered copper wire to form a secondary coil. The primary and secondary coils were separated by layers of silk ribbon. The exact number of turns was not counted, and, as will be seen from what follows, it was not necessary to know the number. The coil so constructed constituted a small

induction coil or transformer, with a closed air-core circuit, but which, when immersed in a liquid, by the penetration of the liquid into the interior of the primary coil, became changed into a closed circuit transformer, with a liquid core. The transformer so designed was capable of being placed underneath liquid oxygen contained in a large vacuum vessel, and when so placed formed a transformer of the closed circuit type, with a core of liquid oxygen. The coefficient of mutual induction of these two circuits, primary and secondary, is

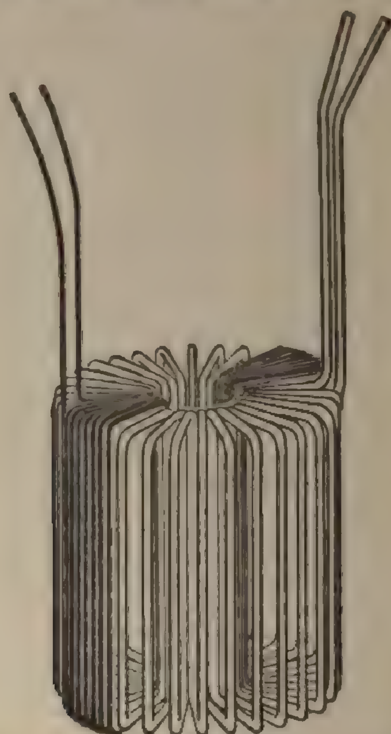

FIG. 1.—Diagram of the Closed Circuit Transformer used in Experiments.

therefore altered by immersing the transformer in liquid oxygen, but the whole of the induction produced in the interior of the primary coil is always linked with the whole of the turns of the secondary coil, and the only form-change that can be made is a small change in the mean perimeter of the primary turns due to the contraction of the coil as a whole. In experiments with this transformer the transformer was always lifted out of the liquid oxygen into the cold gaseous oxygen lying on the surface of the liquid oxygen, and

which is at the same temperature. On lifting out the transformer, the liquid oxygen drains away from the interior of the primary coil, and is replaced by gaseous oxygen of very nearly the same temperature.

The vacuum vessel used had a depth of 60 cm. outside and 53 cm. inside, and an internal diameter of 7 cm. It held 2 litres of liquid oxygen when full; but, as a matter of fact, 4 or 5 litres of liquid oxygen were poured into it in the course of the experiment.

Another induction coil was then constructed, consisting of a long cylindrical coil wound over the four layers of wire, and a secondary circuit was constructed to this coil, consisting of a certain number of turns wound round the outside of the primary coil, and a small adjusting secondary coil, consisting of a thin rod of wood wound over with very open spirals of wire. The secondary turns on the outside of the primary coil were placed in series with the turns of the thin adjusting coil, and the whole formed a secondary circuit, partly outside and partly inside the long primary cylindrical coil, the coefficient of mutual induction of this primary and secondary coil being capable of being altered by very small amounts by sliding into or out of the primary coil the small secondary coil. This last induction coil, which will be spoken of as the balancing coil, was connected up to the small transformer, as just described, as follows:—

The primary coil of the small transformer was connected in series with the primary coil of the balancing induction coil, and the two terminals of the series were connected through a reversing switch and ammeter with an electric supply circuit, so that a current of known strength could be reversed through the circuit, consisting of the two primary coils in series. The two secondary coils, the one on the transformer and the one on the balancing induction coil, were connected in opposition to one another through a sensitive ballistic galvanometer in such a manner that on reversing the primary current the galvanometer was affected by the difference between the electromotive forces set up in the two secondary coils, and a very fine adjustment could be made by moving in or out the adjusting coil of the balancing induction coil.

The arrangement of circuits is shown in Fig. 2.

For the purpose of standardising the ballistic galvanometer employed, the primary coil of the balancing induction coil could be cut out of circuit, so that the inductive effect in the ballistic galvanometer circuit was due to the primary current of the closed circuit transformer alone. A resistance box was also included in the circuit of the ballistic galvanometer. The resistance of the ballistic galvanometer was about 18 ohms, and the resistance of the whole secondary circuit 30·36 ohms. The experiment then consisted in first balancing the secondary electromotive forces in the two coils exactly against one another, then immersing the transformer in liquid oxygen, the result of which was to disturb the inductive balance, and in consequence of the magnetic permeability of the liquid oxygen core

being greater than unity, a deflection of the ballistic galvanometer was observed on reversing the same primary current. The induction through the primary circuit of the small transformer is increased in the same proportion that the permeability of the transformer core is increased by the substitution of liquid oxygen for gaseous oxygen, and hence the ballistic deflection measures at once the amount by which the magnetic permeability of the liquid oxygen is in excess over that of the air or gaseous oxygen forming the core of the transformer when the transformer is lifted out of the liquid. As a matter of fact, it was never necessary to obtain the inductive balance pre-

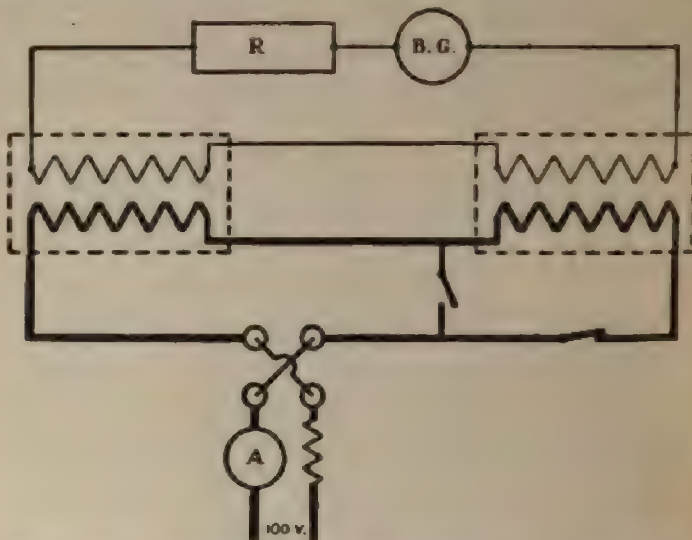

FIG. 2.—Arrangement of the Circuits of the Transformer and Induction Coil.

cisely. All that was necessary was to observe the throw of the ballistic galvanometer, first when the transformer was wholly immersed under the surface of liquid oxygen, and, secondly, when it was lifted out into the gaseous oxygen lying on the surface of the liquid, the strength of the primary current reversed being in each case the same. In order to standardise the galvanometer and to interpret the meaning of the ballistic throw, it was necessary to cut out of circuit the primary coil of the balancing induction coil, and to reverse through the primary circuit of the small transformer a known small primary current, noting at the same time the ballistic throw produced on the ballistic galvanometer, this being done when the transformer was underneath the surface of liquid oxygen. It will be seen, therefore, that this method requires no calculation of any

coefficient or mutual induction, neither does it involve any knowledge of the number of secondary turns on the transformer, nor of the resistance of the secondary circuit; all that is necessary for a successful determination of the magnetic permeability of the liquid oxygen is that the secondary circuit of the transformer should remain practically of the same temperature during the time when the throw of the ballistic galvanometer is being observed, both with the transformer underneath the liquid oxygen and out of the liquid oxygen. If then the result of reversing a current of A ampères through the two primary coils in series when the secondary coils are opposed is to give a ballistic throw D, and if the result of reversing a small current a ampères through the primary coil of the transformer alone is to produce a ballistic throw d, then, if μ is the magnetic permeability of liquid oxygen, that of the gaseous oxygen lying above the liquid and at the same temperature being taken as unity, we have the following relation:—

$$\frac{\mathrm{D}}{\frac{\mathrm{A}}{a}d} = \mu - 1,$$

which determines the value of μ.

TABLE OF RESULTS OF OBSERVATIONS ON THE MAGNETIC PERMEABILITY OF LIQUID OXYGEN.

A = primary current, in ampères, passing through primaries of the transformer and balancing coil.	Corresponding mean magnetising force in C.G.S. units in primary circuit of transformer.	Total ballistic throw which would be produced if primary current of A ampères were reversed through primary of transformer alone $= \frac{\mathrm{A}}{a}d$.	Ballistic throw of galvanometer resulting from immersion of the transformer in liquid oxygen. Transformer and balancing induction coil being opposed = D.	μ = permeability, calculated from $\mu - 1 = \frac{\mathrm{D}}{\frac{\mathrm{A}}{a}d}$.
8·037	35·2	1,734	4·33	1·00250
28·13	123·0	6,068	14·9	1·00246
37·8	165·4	8,153	21·18	1·00260
36·8	161·0	7,938	23·57	1·00297
50·5	220·9	10,894	32·98	1·00304

The values of the permeability given in the foregoing table are not all of equal weight.

The value, viz. 1·00287, found by Professor Fleming and the author for the magnetic permeability of liquid oxygen, shows that the magnetic susceptibility (k) per unit of volume is $228/10^6$. It is interesting to compare this value with the value obtained by

Mr. Townsend for an aqueous solution of ferric chloride, and which he states can be calculated by the equation

$$10^6 k = 91 \cdot 6 w - 0 \cdot 77,$$

where w is the weight of salt in grams per cubic centimetre, and k the magnetic susceptibility. Even in a saturated solution, w cannot exceed $0 \cdot 6$, hence, from the above equation, we find the value of the magnetic susceptibility of a saturated solution of one of the most paramagnetic iron salts, viz. ferric chloride, is $54/10^6$ for magnetic forces between 1 and 9. This agrees fairly well with other determinations of the same constant. On the other hand, the magnetic susceptibility of liquid oxygen for the same volume is $228/10^6$, or more than *four times as great.* The unique position of liquid oxygen in respect of its magnetic susceptibility is thus strikingly shown. It is, however, interesting to note that its permeability lies far below that of certain solid iron alloys generally called non-magnetic.

In the course of these investigations valuable assistance has been given by Mr. Robert Lennox and Mr. J. W. Heath.

[J. D.]

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Royal Institution of Great Britain.

WEEKLY EVENING MEETING,

Friday, January 21, 1898.

Sir Frederick Bramwell, Bart. D.C.L. LL.D. F.R.S. Honorary Secretary and Vice-President, in the Chair.

The Right Hon. Sir John Lubbock, Bart. M.P. D.C.L. LL.D. F.R.S. *M.R.I.*

Buds and Stipules.

The lecturer commenced by saying that his attention had been drawn to the subject by a remark of Vaucher's in his 'Histoire Physiologique des Plantes,' calling attention to the fact that some species of Rock-rose have stipules while others have none, and suggesting that it would be interesting, if possible, to determine the reason for the difference. Stipules are the small leaflets found at the base of the leaf in many plants. In some they drop early, so that on a cursory examination they might be supposed to be absent, as, for instance, in the Beech or Elm; in others they live as long as the leaves, and in some few cases even survive them. The study of stipules led him to that of buds.

Every gardener knows to his cost how often the bright promise of spring is ruined by late frosts. All through the winter the young leaves, which commenced in the previous year and are formed in the bud even early in the previous summer, lie snugly enclosed in many warm wraps, covered, moreover, by furry hairs, and often still further protected from insects and browsing quadrupeds by gummy secretions.

A complete leaf may be considered as consisting of four parts, the blade, the leaf-stalk, the stipules and the leaf-base; or perhaps of two portions: the upper, with its expansion, forming the blade; and the lower, with two appendages, the stipules. Sometimes the stipules are absent, as in Maples; sometimes the leaf-stalk is absent, as in Gentians; sometimes the blade is absent, and its function is performed by the flattened petiole, as in most of the Australian Acacias; sometimes the stipules alone are present, as in a very curious Pea, Lathyrus Aphaca.

He then described a number of different forms of stipules and the construction of buds in a variety of common shrubs and trees. In the Oak the bud has over forty coverings before a normal leaf is reached, and the peculiar form of the leaf-blade is due to the way it is packed in the bud. He showed the leaves and flowers of the coming

summer, and in the Pine even the rudiments of the leaves of the following year. He showed in some cases how the form of the bud influenced the leaves, pointing out that the seed-leaves, or cotyledons, differ from the subsequent leaves mainly because they are influenced, not by the form of the bud, but by that of the seed, and showed for instance how the form of the seed-leaf in the Mustard and other plants was thus determined.

In conclusion, he described the fall of the leaf, which is a vital process, and not merely one of death. Finally, he showed in the Rock-roses that those species in which the young bud is protected by a broad petiole have no stipules, while those in which the petiole is narrow are provided with stipules, which serve for the protection of the bud.

Thus then, he said in conclusion, I have endeavoured to answer Vaucher's question, to explain at any rate in some cases the presence, the uses and the forms of stipules, and the structure of buds in some of our common trees, shrubs and herbs. If I shall have induced you to look at them for yourselves in the coming spring, you will be amply rewarded.

You will often be reminded of Tennyson's profound remark about Nature:

> "So careless of the single life,
> So careful of the type she seems,"

and you will, I am sure, be more and more struck with wonder and admiration at the variety and beauty of the provisions by which Nature preserves these tender and precious buds from the severity of winter, and prepares with loving care and rich profusion for the bright promise of spring and the glorious pageant of summer.

WEEKLY EVENING MEETING,

Friday, January 28, 1898.

Sir James Crichton-Browne, M.D. LL.D. F.R.S. Treasurer and Vice-President, in the Chair.

Professor C. Lloyd Morgan, F.G.S. Principal of University College, Bristol.

Instinct and Intelligence in Animals.

Biology is a science, not only of the dead, but of the living. The behaviour of animals, not less than their form and structure, demands our careful study. Both structure and behaviour are, however, dependent on that heredity which is a distinguishing characteristic of the organic world, and in each case heredity has a double part to play. It provides much that is relatively fixed and stereotyped, but it provides also a certain amount of plasticity, or ability to conform to the modifying conditions of the environment. Instinctive behaviour belongs to the former category; intelligent behaviour to the latter. When a caterpillar spins its silken cocoon, unaided, untaught and without the guidance of previous experience; or when a newly-mated bird builds her nest and undertakes the patient labours of incubation, before experience can have begotten anticipations of the coming brood, we say that the behaviour is instinctive. But when an animal learns the lessons of life and modifies its procedure in accordance with the results of its individual experience, we no longer use the term instinctive, but intelligent. Instinct, therefore, comprises those phases of active life which exhibit such hereditary definiteness as fits the several members of a species to meet certain oft-recurring or vitally important needs. To intelligence belong those more varied modes of procedure which an animal adopts in adaptation to the peculiar circumstances of its individual existence. Instinctive acts take their place in the class of what are now generally known as congenital characters; intelligent acts in the class of acquired characters.

But the study of instinct and intelligence in animals opens up problems in a different field of scientific investigation. They fall within the sphere not only of biological, but also of psychological inquiry. And in any adequate treatment of their nature and origin, we must endeavour to combine the results reached by different methods of research in one harmonious doctrine. This involves difficulties both practical and theoretical. For those invertebrates, such as the insects, which to the naturalist present such admirable examples of

instinctive behaviour, are animals concerning whose mental processes the cautious psychologist is least disposed to express a definite opinion. While the higher mammalia, with whose psychology we can deal with greater confidence, exhibit less typical instincts, are more subject to the disturbing influence of imitation, and, from the greater complexity of their behaviour, present increased difficulties to the investigator who desires carefully to distinguish what is congenital from what is acquired.

Nor do the difficulties end here. For the term "instinct" is commonly, and not without reason, employed by psychologists with a somewhat different significance, and in a wider sense than is necessary or even desirable in biology. The naturalist is concerned only with those types of behaviour which lie open to his study by the methods of direct observation. He distinguishes the racial adaptation which is due to congenital definiteness, from that individual accommodation to circumstances which is an acquired character. But for the psychologist, instinct and intelligence comprise also the antecedent conditions in and through which these two types of animal activity arise. The one type includes the conscious impulse, which in part determines an instinctive response; the other includes the choice and control which characterise an intelligent act. When a spider spins its silken web, or a stickleback builds the nest in which his mate may lay her eggs, the naturalist describes the process and seeks its origin in the history of the race; but the psychologist inquires also by what impulse the individual is prompted to the performance. And when racial and instinctive behaviour is modified in accordance with the demands of special circumstances, the naturalist observes the change and discusses whether such modifications are hereditary; but the psychologist inquires also the conditions under which experience guides the modification along specially adaptive lines. Each has his part to play in the complete interpretation of the facts; and each should consent to such definitions as may lead to an interpretation which is harmonious in its results.

In view, therefore, of the special difficulties attendant on a combined biological and psychological treatment of the problems of animal behaviour, I have devoted my attention especially to some members of the group of birds in the early days of their life, and I shall therefore draw my examples of instinct and intelligence almost entirely from this class of animals. The organisation and the sensory endowments of birds are not so divergent from those of man, with whose psychology alone we are adequately conversant, as to render cautious conclusions as to their mental states altogether untrustworthy; when hatched in an incubator they are removed from that parental influence which makes the study of the behaviour of mammals more difficult; while the highly developed condition in which many of them first see the light of day affords opportunity for observing congenital modes of procedure under more favourable circumstances than are presented by any other vertebrate animals.

Even with these specially selected subjects for investigation, however, it is only by a sympathetic study and a careful analysis of their behaviour that what is congenital can be distinguished from what is acquired; for from the early hours of their free and active life, the influence of the lessons taught by experience makes itself felt. Their actions are the joint product of instinct and intelligence, the congenital modes of behaviour being liable to continual modification in adaptation to special circumstances. Instinct appears to furnish a ground plan of procedure, which is shaped by intelligence to the needs of individual life, and it is often hard to distinguish the original instinctive plan from the subsequent intelligent modification.

It is not my purpose to describe here in detail, as I have done elsewhere, the results of these observations. It will suffice to indicate some of the more salient facts. In the matter of feeding the callow young of such birds as the jackdaw, jay or thrush instinctively open wide their beaks for the food to be thrust into their mouths. Before the eyes have opened, the external stimulus to the act of gaping would seem to be either a sound or the shaking of the nest when the parent bird perches upon it. Under experimental conditions, in the absence of parents, almost any sound, such as a low whistle, lip-sound, or click of the tongue, will set the hungry nestlings agape, as will also any shaking or tapping of the box which forms their artificial nest. And no matter what is placed in the mouth the reflex acts of swallowing are initiated. But even in these remarkably organic responses the influence of experience soon makes itself felt. For if the material given is wrong in kind or distasteful, the effect is that the bird ceases to gape as before to the stimulus. Nor does it continue to open the beak when appropriate food has been given to the point of satisfaction. These facts show that the instinctive act is prompted by an impulse of internal origin, hunger, supplemented by a stimulus of external origin, at first auditory, but later on, when the eyes are opened, visual. They show also that when the internal promptings of hunger cease, owing to satisfaction, the sensory stimulus by itself is no longer operative; and they show, too, that the diverse acts of gaping and swallowing become so far connected that the experience of distasteful morsels tends, for a while at least, to prevent further gaping to the usual stimulus.

With those birds which are active and alert soon after hatching, the instinctive acts concerned in feeding are of a different character. At first, indeed, the chick does not peck at grains which are placed before it, and this is probably due to the fact that the promptings of hunger do not yet make themselves felt, there being still a considerable supply of unabsorbed yolk. Soon, however, the little bird pecks with much, but not quite perfect, accuracy at small near objects. But here again experience rapidly plays its part. For if distasteful objects, such as bits of orange peel, are the first materials given, pecking at them soon ceases; and if this be repeated the little bird cannot be induced to peck, and may even die of starvation. This

makes it very difficult to rear by hand some birds, such as plovers, whose natural food, in due variety, is not readily obtainable. It must be remembered, too, that under natural conditions the parent bird calls the young and indicates with her beak the appropriate food; and this appears to afford an additional stimulus to the act of pecking. Pheasants and partridges seem to be more dependent on this parental guidance than domestic chicks, and they are more easily reared when they have somewhat older birds as models, whose pecking they may imitate. Passing allusion may here be made to a type of instinctive response in some respects intermediate between the upward gaping of the jay and the downward pecking of the chick. It is seen in the young moorhen, which pecks upwards at food held above it, and cannot at first be induced to take any notice of food on the ground. Under natural conditions it is fed by the parent, which holds the food in her beak above the little bird as it floats on the water.

We have, then, in these simple instinctive acts examples of behaviour which is congenitally definite in type for each particular species; of actions which are the joint product of an internal factor, hunger, and an external factor, sensory impressions; of complex modes of procedure which subserve certain vital needs of the organism. It should be mentioned, however, that the relative definiteness of instinctive responses has been subjected to criticism from a psychological source. It has been urged that the nutritive instincts, the play instincts, the parental instincts, those of self-preservation, and those concerned in reproduction, are so varied and multifarious that definiteness is the last thing that can be predicated of them. Varied and multifarious they are indeed, and each of the groups above mentioned contains many differing examples; but that is because we are dealing with comprehensive classes of instinctive behaviour. The fact that the group of fishes includes organisms of such wide structural diversity as the salmon, the globe fish, the eel and the sole, does not affect the fact that these species have a relatively definite structure each after his kind. It is only when we treat a group of fishes as if it were an individual fish that we are troubled by indefiniteness of structure. And it is only when we deal with a group of instincts comprised under a class-name as if it were a particular instinctive act, that we fail to find that definiteness which to the naturalist is so remarkable.

From the physiological point of view, instinctive procedure would seem to have its origin in an orderly group of outgoing neural discharges from the central office of the nervous system, giving rise to a definite set of muscular contractions. And this appears to have an organic basis in a congenital preformation in the nervous centres, the activity of which is called into play by incoming messages, both from internal organs in a state of physiological need, and from the external world through the organs of special sense. The naturalist fixes his attention chiefly on the visible behaviour, which is for him the essential feature of the instinctive act. But in view of the requirements of psychological interpretation it is advisable to comprise under

the term instinct, in any particular manifestation of its existence, the net result of four things: first, internal messages giving rise to the impulse; secondly, the external stimuli which co-operate with the impulse to affect the nervous centres; thirdly, the active response due to the co-ordinated outgoing discharge; and fourthly, the message from the organs concerned in the behaviour by which the central nervous system is further affected. Now I shall here assume, without pausing to adduce the arguments in favour of this view, that consciousness is stirred in the brain only by incoming messages. If this be so, the outgoing discharges which produce the behaviour are themselves unconscious. Their function is to call forth adaptive movements; and these movements give rise to messages which, so to speak, afford to consciousness information that the instinctive act is in progress. Hence I have urged that the instinctive *performance* is an organic and unconscious matter of the purely physiological order, though its effects are quickly communicated to consciousness in the form of definite messages from the motor organs. I have not denied that the stimuli of sight, touch, hearing, and so forth, have conscious effects; I do not deny (though here I may have spoken too guardedly) that the initiating impulse of internal origin is conscious. In both these cases we have messages transmitted to the central office of the brain. What I have ventured to urge is that the consciousness of instinctive behaviour, *in its completed form*, does not arise until further messages come in from the motor organs implicated in the performance of the act, lodging information at the central office concerning the nature of the movement. A diagram will perhaps serve to make this conception clearer.

The circle represents the brain, in some part of which consciousness arises through the effects of incoming nerve-currents. Under the influence of the two primary groups of messages due to impulse and to sensory stimulus, consciousness is evoked, and the brain is thrown into a state of neural strain, which is relieved by the outgoing discharge to the organs concerned in the instinctive behaviour. It is this outgoing discharge which I regard as unconscious. But the actions which are thus produced give rise to a secondary group of incoming messages from the moving limbs. This it is which gives origin to the consciousness of instinctive behaviour as such. And I regard it as psychologically important that these incoming messages are already grouped so as to afford to consciousness information rather of the net results of movement than of their subsidiary details.

So much for our general scheme. If now we turn to the instinctive behaviour concerned in locomotion, we find a congenital basis upon which the perfected activities are founded. There is on the part of the chick no elaborate process of learning to walk; ducklings and moorhens a few hours old swim with perfect ease when they are placed in water; these birds also dive without previous practice or preliminary abortive attempts; while young swallows, if their wings are sufficiently large and strong, are capable of short and guided flights the first time they are committed to the air. In these cases neither the internal impulse nor the sensory stimuli are so well defined as in that of the nutritive activities. The impulse probably takes the form of an uneasy tendency to be up and doing, perhaps due to ill-defined nervous thrills from the organs of locomotion, which are in need of exercise. The sensory stimuli are presumably afforded by the contact of the feet with the ground or with the water, and by the pressure of the air on the wing surfaces. It is a curious fact that if young ducklings be placed on a cold and slippery surface, such as that of a japanned tea tray, they execute rapid scrambling movements suggestive of attempts to swim, which I have never seen in chicks, pheasants or other land birds.

It will not be supposed that I claim for perfected locomotion, so admirably exemplified in the graceful and powerful flight of birds, an origin that is wholly instinctive and unmodified by the teachings of experience. Here, as elsewhere, instinct seems to form the ground plan of activities, which intelligence moulds to finer and more delicate issues. This is the congenital basis on which is built the perfected superstructure. And if our opportunities for observation and our methods of analysis were equal to the task, we should be able to distinguish, in the development of behaviour, the congenital outline from the shading and detail which are gradually filled in by the pencil of experience.

The difficulties which render this analysis at the best imperfect are therefore twofold. In the first place, intelligence begins almost at once to exercise its modifying influence; and in the second place, many instinctive traits do not appear until long after intelligence has begun its work. Much of the intelligent detail of the living picture is filled in before the instinctive outlines are complete. The term "deferred instincts" has been applied to those congenital modes of procedure which are relatively late in development. The chick does not begin to scratch the ground, in the manner characteristic of rasorial birds, till it is four or five days old, nor does it perform the operation of sand-washing till some days later; the moorhen does not begin to flick its tail till it is about four weeks old; the jay does not perform the complex evolutions of the bath till it has left the nest and felt its legs, when the stimulus of water to the feet, and then the breast, seems to start a train of acts which, taken as a whole, are of a remarkably definite type. The development of the reproductive organs brings with it, apart from the act of pairing, a number of associated modes of

behaviour—nest building, incubation, song, dance, display, and strange aerial evolutions—which are presumably, in large degree, instinctive, though of this we need more definite evidence; for it is difficult to estimate, with any approach to accuracy, the influence of imitation. There seems to be no reason for doubting that, when an animal grows up in the society of its kind, it is affected by what we may term the traditions of its species, and falls into the ways of its fellows, its imitative tendency being subtly influenced by their daily doings. The social animal bears the impress of the conditions of its peculiar nurture. Its behaviour is in some degree plastic, and imitation helps it to conform to the social mould.

The exact range and nature of the instinctive outline, independently of those modifications of plan which are due to the inherent plasticity of the organism, are therefore hard to determine. And if, as we have good grounds for believing, the growth of intelligent plasticity, in any given race, is associated with a disintegration of the instinctive plan, congenital adaptation being superseded by an accommodation of a more individualistic type, to meet the needs of a more varied and complex environment, the problems with which we have to deal assume an intricacy which at present defies our most subtle analysis.

We must now turn to the consideration of the manner in which individual accommodation, through the exercise of intelligence under the teachings of experience, is brought about; and it will be well to pave the way by adducing certain facts of observation.

Although the pecking of a young chick, under the joint influence of hunger and the sight of a small near object, would seem to belong to the instinctive type, the selection of appropriate food, apart from the natural guidance of the hen, seems to be mainly determined by individual experience. There is no evidence that the little bird comes into the world with anything like hereditary knowledge of good and evil in things eatable. Distasteful objects are seized with not less readiness than natural food, such as grain, seeds and grubs. The conspicuous colours of certain nasty caterpillars do not appeal to any inherited power of immediate discrimination, so as to save the bird from bitter experience. They seem rather to serve the purpose of rendering future avoidance, in the light of this bitter experience, more ready, rapid and certain. Bees and wasps are seized with neither more nor less signs of fear than large flies or palatable insects. Nor does there seem to be any evidence of the hereditary recognition of natural enemies as objects of dread. Pheasants and partridges showed no sign of alarm when my dog quietly entered the room in which they were kept. When allowed to come to closer quarters, they impudently pecked at his claws. A two-days chick tried to nestle down under him. Other chicks took no notice of a cat, exhibiting a complete indifference which was not reciprocated. A moorhen, several weeks old, would not suffer my fox-terrier to come near his own breakfast of sopped biscuit, but drove him away with angry pecks until the higher powers supervened.

It is not, of course, to be inferred from these observations that such an emotion as fear has no place in the hereditary scheme, or that the associated acts of hiding, crouching or efforts to escape, do not belong to the instinctive type. I have seen little pheasants struck motionless, plovers crouch, and moorhens scatter, at the sound of a loud chord on the violin, or of a shrill whistle. A white stoneware jug, placed in their run, caused hours of uneasiness to a group of birds including several species. But there is no evidence that, in such cases, anything like hereditary experience defines those objects which shall excite the emotion. It is the unusual and unfamiliar object, especially after some days of active life amid surroundings to which they have grown accustomed; it is the sudden sound (such as a sneeze), or rapid movement, as when a ball of paper is rolled towards them, that evokes the emotion. Hence, if the parent birds are absent, the stealthy approach of a cat causes no terror in the breast of inexperienced fledgelings. But when she leaps, and perhaps seizes one for her prey, the rest scatter in alarm, and for them the sight of a cat has in the future a new meaning.

The elementary emotions of fear, anger, and so forth, stand in a peculiar and special relationship to instinct. At first sight they seem to take rank with the internal impulses which are the part-determinants of instinctive behaviour. The crouching of a frightened plover or land-rail, the dive of a scared moorhen, result partly from the external stimulus afforded by the terrifying object, partly from the emotional state which that object calls forth. But in their primary genesis I am disposed—here following to some length the lead of Professor Wm. James—to assign to such emotions an origin similar to that of the consciousness which follows on the execution of the instinctive act. Assuming, as before, that consciousness owes its genesis to messages which reach the sensorium through incoming nerve-channels, the sensory stimuli, afforded, let us say, by the sight of a terrifying object, do not seem, in the absence of inherited experience, capable of supplying messages which in themselves are sufficient to generate the emotion of fear. Now the well known accompaniments of such an emotional state are disturbances of the heart-beat, the respiratory rhythm, the digestive processes, the action of the glands, and the tone of the minute blood vessels throughout the body. And all these effects are unquestionably *produced* by outgoing discharges from the central nervous system. But they are *felt* as the result of incoming messages, like vague and disquieting rumours, transmitted to the central office from the fluttering heart, the irregular breathing, the sinking stomach, and the disturbed circulation. Is it not therefore reasonable to suppose that the emotion in its primary genesis, is due to the effect on the sensorium of these disquieting messages? If this be admitted as a working hypothesis—and it cannot at present claim to be more than this—we reach at any rate a consistent scheme. As primary messages to the central office of consciousness we have, on the one hand those due to stimuli of the special senses, and on the

other hand those resulting from the condition of the bodily organs, taking the form of a felt craving for their appropriate exercise. These co-operate to throw the brain into a state of unstable equilibrium, or neural strain, which is relieved by outgoing streams of nervous energy. And these in turn fall into two groups: first, an orderly set of discharges to the voluntary muscles concerned in behaviour; and secondly, a more diffuse group of discharges to the heart, respiratory apparatus, digestive organs, glands and vascular network. In so far as these are outgoing discharges, they do not directly affect consciousness. But there quickly returns upon the sensorium an orderly group of incoming messages from the motor apparatus concerned in instinctive behaviour, and a more indefinite group from the heart and other visceral organs. The former gives the well-defined consciousness of activity, the latter the relatively ill-defined feelings which are classed as emotional. But so swift is the back-stroke from the body to the brain, that, ere the instinctive behaviour is complete, messages from the limbs—and, under the appropriate circumstances, from the heart—that is to say, of both instinctive and emotional origin—begin to be operative in consciousness; and the final stages of a given performance may be guided in the light of the experience gained during its earlier stages.

The exact manner in which consciousness exercises its guiding influence is a matter of speculation. Perhaps the most probable hypothesis is that the central hemispheres are an adjunct to the rest of the central nervous system, and exercise thereon, by some such mechanism as the pyramidal tract in the human subject, a controlling influence. Given an hereditary ground plan of automatic and instinctive responses, the cerebral hemispheres may, by checking here and enforcing there, limit or extend the behaviour in definite ways. In any case, from the psychological point of view, their action is dependent on three fundamental properties: first, the retention of modifications of their structure; secondly, differential results according as these modifications have pleasurable or painful accompaniments in consciousness; and thirdly, the building of the conscious data, through association, into a system of experience. The controlling influence of this experience is the essential feature of active intelligence. Or, expressed in the almost obsolete terminology of the older psychology, intelligence is the faculty through which past inexperience is brought to bear on present behaviour.

Professor Stout, whose careful work in analytical psychology is well known, has done me the service of criticising, in a private communication, my use of the phrase "past experience," urging that present experience is not less important in determining behaviour than that which is past, and which can only be operative through its revival in memory. The criticism is valid in so far as it shows that I have not been sufficiently careful to define what I mean by past experience. But I certainly had in mind, though I did not clearly indicate, the inclusion of what Mr. Stout regards as present ex-

perience. My conception of "present," as I have elsewhere described it, is that short but appreciable period of time, occupying only some small fraction of a second, which is comprised in the fleeting moment of consciousness. All anterior to this, if it were but a second ago, I regard as past—past, that is to say, in origin, though still operative in the limited field of the present moment. When we are reading a paragraph and near its close, the net result of all that we have read in the earlier sentences is present to influence the course of our thought. But the very words—"all that we *have* read"—by which we describe this familiar fact, imply that the guiding experience originated in a manner which demands the use of the past tense. Still I am none the less grateful to Mr. Stout for indicating what to many may have seemed a serious omission in my interpretation. Suffice it to say that if we include under the phrase "present experience" the occurrences of five minutes or even of five seconds ago (all of which I regard as past), I fully agree that present experience (in this sense) exercises a most important guiding influence.

We have distinguished four classes of messages affecting consciousness in the central office of the sensorium: first, stimuli of the special senses; secondly, internal cravings; thirdly, motor sensations due to bodily activity; and fourthly, emotional states. These are combined in subtle synthesis during the growth of experience, and are associated together in varied ways. Into the manner in which experience grows we cannot enter here. It will be sufficient to indicate very briefly the effects of this growth on the behaviour of animals in the earlier stages of their life. This may be considered from a narrower or from a broader standpoint. In the narrower view we watch how, within the field of widening synthesis, particular associations are formed. We see how, within experience, the taste and appearance of certain caterpillars or grubs become so associated that for the future the larva is left untouched. Or we see how that terrible pounce of the cat becomes so associated with her appearance as thenceforth to render her an object of dread to enlightened sparrows. But of the physiological mechanism of association we know little.

There is a familiar game in which a marble is rolled down an inclined board at the bottom of which are numbered compartments. The lower part of the board is beset with a series of vertical pins so arranged that the marble, rebounding from one to another, pursues a devious course before it reaches its destination. But if we tie threads from pin to pin we may thus direct the course of the marble along definite lines. Now the brain may be roughly likened to a set of such pins, and the marble to an incoming nerve current. The congenital structure is such that a number of hereditary threads connect the pins in definite ways, and direct the discharge into appropriate channels. But a vast number of other threads are acquired in the course of individual experience. These are the links of association which direct the marble in new ways. Observation of

behaviour can only give us information that new directing threads have been introduced. The psychology of association can only indicate which pins have been connected by linking threads. Even such researches as those of Flechsig can at present do no more than supplement the psychological conclusion by general anatomical evidence. Of the details of brain modification by the formation of association fibres we are still profoundly ignorant.

Nor when we turn from the narrower to the wider point of view are we in better case. We are forced to content ourselves with those generalities which are the makeshift of imperfect knowledge. Still even such generalities are of use in showing the direction in which more exact information is to be sought. And we can, perhaps, best express the net result of acquired modification of brain-structure by saying that every item of experience makes the animal a new being, with new reactive tendencies. The sparrows, which yesterday were unaffected by the stealthy approach of the cat, garrulously scatter to-day, because they are not the same simple-minded sparrows that they were. The chick comes into the world possessed of certain instinctive tendencies, with certain hereditary directing threads. But at the touch of experience its needs are modified or further defined. New connecting threads are woven in the brain. On the congenital basis has been built an acquired disposition. The chick is other than it was, and reacts to old stimuli with new modes of behaviour.

In its early days, the developing animal is reading the paragraph of life. Every sentence mastered is built into the tissue of experience, and leaves its impress on the plastic yet retentive brain. By dint of repetition the results of acquisition become more and more firmly ingrained. Habits are generated, and habit becomes second nature. The organism which, to begin with, was a creature of congenital impulse and reaction, becomes more and more a creature of acquired habits. It is a new being, but one with needs not less imperious than those with which it was congenitally endowed.

All of this is trite and familiar enough. But it will serve its purpose if it help us to realise how large a share acquired characters take in the development of behaviour in the higher animals, and how fundamentally important is the plasticity of brain-tissue, and its retentiveness of the modifications which are impressed on its yielding substance.

Such being the relations of intelligence to instinct in the individual, what are their relations in the evolution of the race? Granting that instinctive responses are definite through heredity, how has this definiteness been brought about? Has it been through natural selection? Or are the acquired modifications of one generation transmitted through heredity to the next? Is instinct inherited habit? Mr. Herbert Spencer has long advocated and still advocates the latter view; while Mr. A. R. Wallace attributes instinct entirely to natural selection. Darwin, who wrote before the transmission of acquired characters was seriously questioned, admitted both factors.

And Romanes, to whose ever-kindly sympathy I am deeply indebted, adhered to this view in spite of modern criticism. There is not much in my own observational work which has any decisive bearing on the question. But there are one or two points which are perhaps worthy of consideration. The part played by acquisition in the field of behaviour is the establishment of definite relations between particular groups of stimuli and adaptive responses. If this be so, and if acquired modifications of brain-structure be transmitted, we might reasonably expect that the sight of a dog would have a similar effect on young pheasants to that which it has on their parents. But this does not appear to be the case. Again, one might reasonably expect that the sight of water would evoke a drinking response in recently hatched birds, just as the sight or scent of a Yucca flower excites a definite response in the Yucca moth. But here, too, this is not so. Thirsty chicks and ducklings seem to be uninfluenced by the sight of water in a shallow tin. They may even run through the liquid and remain unaffected by its presence. But if they chance to peck at a grain at the bottom of the tin or a bubble on the water, as soon as the beak touches the liquid, *this* stimulus at once evokes a drinking response again and again repeated. Why does the touch of water in the beak excite a congenital response, while the sight of water fails to do so? I believe it is because under natural conditions the chicks peck at the water in imitation of the mother, who thus shields them from the incidence of natural selection. Under these circumstances there is no opportunity for the elimination of those who fail to respond at the mere sight of water, and consequently no selective survival of those who do thus respond. But though the hen can lead her young to peck at the water, she cannot teach them the essential movements of beak, mouth and gullet which are necessary for the completed act of drinking. In this matter she cannot shield them from the incidence of natural selection. Those which, on pecking the water, failed to respond to the stimulus by drinking, would assuredly die of thirst and be eliminated; the rest would survive and transmit the congenital instinctive tendency. Thus it would seem that when natural selection is excluded, a special mode of behaviour has not become congenitally linked with a visual stimulus; but where natural selection is in operation, this behaviour has become so linked with a touch or taste stimulus in the beak. Similarly in the case of the pheasants and the dog. The parent birds warn the young of his approach, and thus prevent the incidence of natural selection. Hence there is no instinctive response to the sight of a terrier.

No doubt there are many cases of complex behaviour, seemingly instinctive, which are difficult to explain by natural selection alone, and which have the appearance of being due to the inheritance of acquired habits. I have, however, elsewhere suggested that acquired modifications may, under the conditions of natural selection, foster the development of "coincident" variations of like nature and direc-

tion, but having their origin in the germinal substance. But into a consideration of this hypothesis I cannot here enter. Without assuming a dogmatic attitude, I am now disposed to regard the direct transmission of acquired modes of behaviour as not proven.

Thus we come back to the position assumed at the outset—that heredity plays a double part. It provides, through natural selection or otherwise, an outline sketch of relatively definite behaviour, racial in value; it provides also that necessarily indefinite plasticity which enables an animal to acquire and to utilise experience, and thus to reach adaptation to the circumstances of its individual life. It becomes therefore a matter of practical inquiry to determine the proportion which the one kind of hereditary legacy bears to the other. Observation seems to show that those organisms in which the environing conditions bear the most uniform relations to a mode of life that is relatively constant, are the ones in which instinct preponderates over intelligent accommodation; while those in which we see the most varied interaction with complex circumstances, show more adaptation of the intelligent type. And the growth of individual plasticity of behaviour in race development would seem to be accompanied by a disintegration of the definiteness of instinctive response, natural selection favouring rather the plastic animal capable of indefinitely varied accommodation than the more rigid type, whose adaptations are congenitally defined.

I have dealt, it will be observed, only with the lower phases and earlier manifestations of intelligence. Its higher development, and the points in which it differs from the more complex modes of human procedure, offer a wide and difficult field for careful observation and cautious interpretation. I have recently attempted further investigations in this field, but they concern rather the relation of intelligence to logical thought than that of instinct to intelligence, which forms the subject of this discourse.

[C. Ll. M.]

WEEKLY EVENING MEETING,

Friday, February 4, 1898.

Sir William Crookes, F.R.S. Vice-President, in the Chair.

Alan A. Campbell Swinton, Esq. *M.R.I.*

Some New Studies in Cathode and Röntgen Radiations.

The researches of Crookes, Lenard and Röntgen have given to man a new eye. They have perhaps also given to nature a new light. They have certainly given to science more than one new problem.

This small glass bulb which I hold in my hand, which, being exhausted to a high vacuum, contains, besides its two aluminium electrodes, only a few billions of molecules of residual gas, may appear but a simple piece of apparatus. Could it, however, only be induced while under the stimulus of an electric discharge to reveal in their entirety the secrets that it contains, we should know much at present utterly unknown, not only as regards the nature of electrical action, but also in reference to the fundamental constitution of matter, and the true mechanism of energy. It is, in fact, for the reason that within the Crookes radiant-matter tube, where molecules are separated by comparatively long distances, it is possible to deal not as in everyday life with aggregates of matter, but even individually, perhaps, with single molecules and atoms floating apart in space, that so much attention is at present being devoted to this particular branch of physics.

Every one is now acquainted with what has become the quite ordinary phenomenon of the cathode rays. I turn on the induction coil spark to this highly exhausted tube, and from the aluminium plate that forms the negative electrode or cathode, there proceeds, as you see, some kind of ray that excites a green luminescence in the glass upon which it falls. I interpose in the path of these cathode rays a screen, made of aluminium in the form of a cross, and the latter casts a sharp shadow on the glass. I have here a coil of wire, through which an electric current is passing, and as I slowly move the coil so as to encircle the tube, and consequently gradually increase the strength of the magnetic field within the tube, it will be observed that the shadow of the cross rotates, becoming at the same time smaller. Here we obviously have a deflection of the cathode rays from their rectilinear path, the action of a magnetic field of this description being to concentrate the rays and also to give them a twist, the direction of which depends upon the direction in which the current is sent through the coil of wire.

This concentration or focussing of the cathode rays by means of a magnetic field, which has been studied by Birkeland and by Fleming, can be also shown by means of another tube, the interior of which is free from any obstruction. This tube, when excited in the ordinary manner, shows, as you will observe, the usual green fluorescence nearly all over its surface, but especially at the rounded end opposite the cathode. I suspend this tube over one pole of a powerful electro-magnet, placed with its axis in line with that of the tube as shown in Fig. 1. As more and more electric current is passed round the electro-magnet, and the magnetic field becomes stronger and stronger, it will be observed that the beam of cathode rays becomes more and more concentrated to a point opposite the pole of the magnet, until at length when the magnet is fully excited the whole of the green fluorescence in the tube has now entirely died out, and the cathode stream can be seen as a bluish cone, the base of which is the cathode disc, and the apex is a very small point exactly over the centre of the magnet pole. It is not possible to keep the tube in this condition for more than a few seconds, as the heat produced on the glass where the cathode rays are concentrated is so intense as to quickly perforate the latter. Indeed, by slowly moving the tube it is possible to engrave on its interior surface any desired figure, the action of the cathode rays being sufficient to erode the glass. Fig 2 is a photograph of the globular end of a tube, upon the interior glass surface of which, as can be seen, a square with diagonals has been roughly engraved by this means. Whether the action is due directly to the bombardment of the atoms which form the cathode rays breaking off little pieces of glass as a volley of minute bullets would do, or whether it is a secondary effect due to heat, is perhaps uncertain. The result in any case is that where the concentrated cathode rays impinge upon the glass, the latter is eroded and visibly roughened.

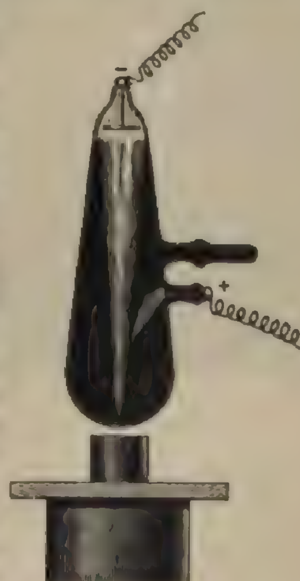

FIG. 1.—Cathode rays focussed to a point by means of a magnet.

A concentrated cathode discharge can also be obtained by employing as cathode a spherically concave aluminium cup, so arranged relatively to the glass of the tube that the rays are given off only from the hollow side, this being the arrangement now universally used in tubes for the production of the Röntgen rays. It is a method origi-

nally introduced by Crookes, more especially for showing the heating effect of the cathode rays when allowed to impinge upon a piece of platinum foil, and it is to Herbert Jackson that we owe its application to the production of the Röntgen rays.

FIG. 2.—Figure engraved on the interior of a glass bulb by cathode rays.

Here is a tube arranged as in Fig. 3 with two concave cathodes opposite one another, both focussing upon a small fragment of quicklime. I employ in this case two cathodes because I am going to use an alternating electric current, such as is supplied from the mains, but transformed up to some 20,000 volts by being passed through an induction coil. Each aluminium cup serves in turn as cathode and anode, and, as will be observed, when the current is turned on and conditions are favourable, a very brilliant and beautiful light is produced. This, however, only lasts for a short time and then dies out, the strong light recurring from time to time at unequal intervals. This curious effect, which in result is analogous to the hunting of a badly adjusted arc lamp, requires explanation. It appears to be due to absorption of the residual gas by the lime while the latter is white hot, and the giving of it out again at a lower temperature; this producing a periodic increase and decrease of the vacuum, and a consequent decrease and increase of the energy of the discharge through the tube and of the light. Another curious fact, and one that supports the bombardment theory of the cathode rays, is that the rays after having been allowed to fall upon the block of lime for a little time, are found to bore perfectively straight and very minute holes in the material. This block, which has been used on several occasions, and has also been turned round a little, was solid originally, but has now several holes passing right through it, some of these not being more than about half a millimetre in diameter. At the edges the material is somewhat broken away, but in the interior the holes have been so accurately eroded by the cathode rays that they look as though they might have been bored with a small drill. This shows the great accuracy with which the cathode rays can be focussed. Again, it is remarkable that though the current is alternating, and the arrangement of the tube and electrodes perfectly symmetrical, so that one would expect the heating and luminous effect

th sides of this piece of lime to be the same, the light appears to ven off sometimes only on one side and sometimes only on the

ith a tube such as this, excited with an alternating current, it is o produce exceedingly high temperatures confined to a very small

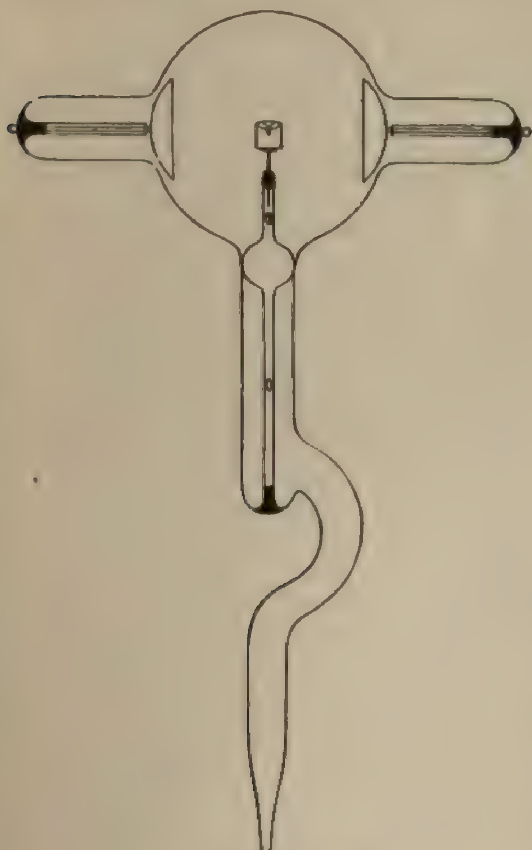

FIG. 3.—Cathode ray lamp.

and it is not at all improbable that it may be eventually found le to produce commercially and practically in this way, high e electric lamps of much higher efficiency than the ordinary descent filament lamp, and possibly even rivalling arc lamps. In of these latter it is necessary that the incandescent substance

should be a fairly good electrical conductor; whereas in this cathode ray arrangement there is no such limitation, and consequently there is a much wider range of available refractory substances. It is also quite conceivable that in future an electric furnace of this nature may be found of service in some of the more delicate of chemical investigations where it is necessary to obtain in isolated substances very high temperatures. Indeed, already Crookes and Moissan have employed this means for turning into graphite the surface of a diamond.

It is now becoming more and more generally believed that Sir William Crookes' original theory, enunciated some twenty years ago, as to the nature of these cathode radiations, is at any rate to a large extent correct. According to this theory the cathode rays consist of material particles of the residual gas, which being similarly electrified by contact with the cathode are violently repelled by the latter. This has been the view held for a long time by most English physicists, and the chief point of difference now appears to be whether these material particles are single atoms, single molecules, or larger aggregations of matter.

I have here a model which roughly shows what is supposed to take place. As you see, there are facing one another two plate electrodes, which I am able to charge positively and negatively respectively by means of a Wimshurst machine; between them is suspended by a silk thread, what for the moment we will assume to be a single atom. It is in fact a gilded pith ball. As soon as I turn the handle of the Wimshurst machine and electrify the electrodes, as you see, the ball oscillates rapidly from one to the other. If it starts in contact with the negative electrode it receives from this a negative charge; it thereupon is repelled until it strikes the positive electrode, where it gives up its negative charge and receives a positive one. Again, owing to mutual repulsion, it is driven across to the negative electrode, and so on backwards and forwards. This is a very simple and elementary experiment, which I would not have ventured to show you except that it leads to another which is perhaps of more interest. If the atoms in a tube were caused to fly backwards and forwards at equal velocities, as did the pith ball, between anode and cathode, it is obvious that there would be an anode stream similar in most if not in all respects to the cathode stream, which does not appear to be the case. If, however, I now remove the connection between the positive electrode and the Wimshurst machine, and instead, connect the positive electrode to earth, leaving the negative electrode connected to the Wimshurst machine as before, it will be seen that the pith ball flies with much greater violence and rapidity from cathode to anode than it does on its return journey from anode to cathode. This is for the reason that while in the former case we have both the repulsion of the cathode on the similarly electrified ball and also the attraction of the anode urging the ball on its path, on the return journey both ball and anode are at zero potential, and consequently there is the attraction of the distant cathode only causing the ball to move. Now, if we

consider the condition of affairs inside a focus tube while a discharge is taking place, this last experiment may help us to understand at least one possible reason for the atoms not being projected from the anode at anything like the velocity that they are projected from the cathode.

Fig. 4 has been prepared to show the probable distribution of positively and negatively electrified atoms in a focus tube while the discharge is taking place. It is largely based upon previous similar illustrations due to Crookes, applied to a tube of a different form. As will be seen, the greater portion of the bulb is filled with positively electrified atoms, as denoted by crosses, while it is only behind the cathode and in the cathode stream itself that any negatively electrified atoms are to be found. That this is at any rate approximately true can be proved by means of exploring poles and in other ways, and it is curious to note that some of the very beautiful photographs published by Lord Armstrong in his recent monograph on 'Electric Movements in Air and Water' show that in air at ordinary atmospheric pressure there is a similar tendency for the positive discharge to be much more dispersive than the negative.

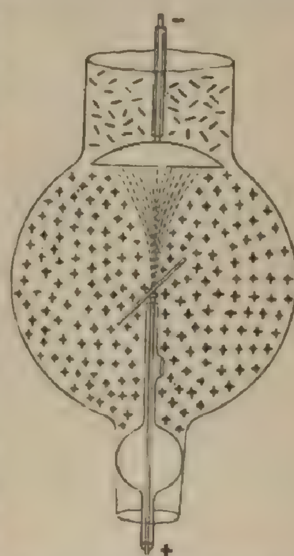

FIG. 4.—Diagram showing probable distribution of positively and negatively charged atoms in a focus tube.

Now assuming that the figure correctly denotes the condition of the atoms inside the tube, it is evident that considering only the contents of the tube and disregarding everything outside, the anode is very much in the same condition as the earthed electrode in the pith ball experiment; being at the same electrical potential as the great bulk of its environment. It is very probably for a similar reason that in a tube of the form illustrated the cathode rays are only given off from the concave side of the cathode, the whole environment of the convex side being negatively charged, with the result that the atoms there are in a state of equilibrium.

Whether this explanation is sufficient or not—and no doubt there are at work other causes—in any case there is no question that the velocity of the negative stream is very much greater than the velocity of the positive stream. That there is something of the nature of a positive stream, which increases in velocity the higher the exhaustion, can, however, be shown experimentally.

Fig. 5 is a radiometer tube, exactly similar in principle to those of Crookes. It consists of an ordinary focus tube, on one side of which a glass annex has been blown, containing a sliding carrier, holding half inside a glass cup a small and delicately pivoted wheel with mica vanes. By the employment of a magnet, which acts on a piece of iron attached to the sliding carrier inside the tube, I can move the wheel bodily, either out into the centre of the tube, so that the cathode stream impinges upon the vanes, or back into the annex,

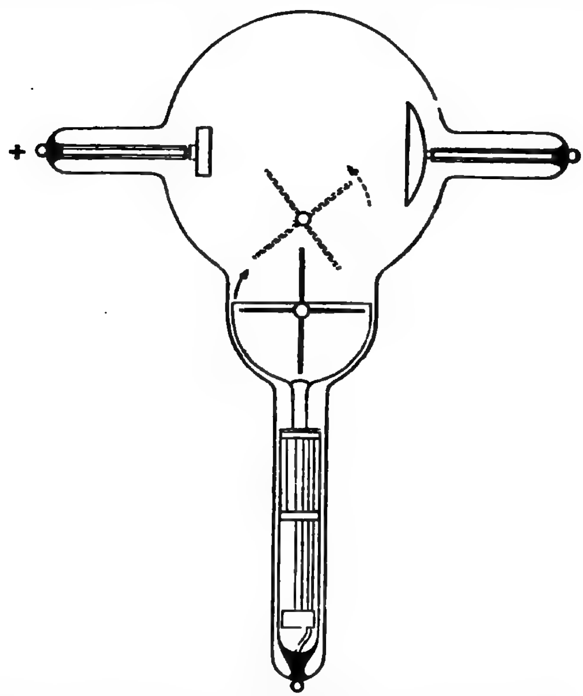

FIG. 5.—Adjustable radiometer tube for showing both cathode and anode streams.

when the vanes are quite outside of the cathode line of fire. When the tube is put into operation in this latter position (that shown in full lines in the illustration), immediately the current is turned on the wheel begins slowly to revolve in a direction that indicates a stream from the anode to the cathode. On the other hand, when the wheel is moved out into the bulb (in the position indicated in dotted lines), so that the cathode stream impinges upon the vanes, the wheel immediately begins to revolve with great rapidity in the opposite direction.

Here, therefore, we have direct experimental evidence that in a focus tube, while the cathode stream of negatively electrified atoms proceeds at a great velocity through the centre of the bulb, the anode stream of positively electrified atoms returns to the cathode at a much lower velocity round the outside of the cathode stream. Fig. 6 shows approximately what probably occurs in a tube of the ordinary focus type, the direction of the two opposite streams of positively and negatively charged atoms being shown by the arrow-heads.

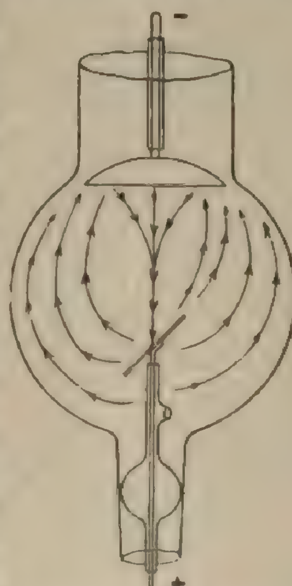

FIG. 6.—Diagram showing probable circulation of atoms in a focus tube.

If the discharge within a focus tube be closely watched during the process of exhaustion, it will be found to alter as the vacuum increases. First of all, at a low vacuum, the cathode rays can be seen converging in the form of a cone from the concave cathode to a focus, and then immediately diverging again in another cone on the other side of the focus, as shown on the extreme left of Fig. 7. It can further be shown that the individual rays cross at the focus. As the exhaustion proceeds, both convergent and divergent cones, but especially the latter, become smaller and smaller, while the thread that joins them becomes longer and longer as shown in the succeeding sections of Fig. 7, till at last, at the highest vacuum at which the discharge will pass, the cathode rays, which are now very nearly invisible, appear to come off only from a small area at the centre of the cathode, and not very appreciably to diverge again after once having come together, as indicated on the extreme right of the illustration.

Now I have found that if the anti-cathode or anode upon which the cathode rays impinge is made not of aluminium or of platinum as usual, but of ordinary electric light carbon, the carbon becomes luminescent where struck by the rays. Further, if the carbon anti-cathode be so placed as to intersect either the convergent or divergent cones of rays, these, instead of producing a uniform luminous patch upon the carbon, produce a bright ring with a dark interior. This ring becomes smaller as the vacuum is increased. It develops a bright spot in the centre as exhaustion proceeds still further, and finally with a still higher vacuum it closes round the spot until only the spot itself is left. These effects are shown for each condition of vacuum in the lower portion of Fig. 7, and I have here a tube that I

will put into action and show the effect for one degree of vacuum. As you observe, the luminescence on the carbon is very bright, in fact the surface appears white hot. It, however, takes the shape of a well defined hollow ring with a dark interior and a bright spot at the centre, and as I deflect the stream of cathode rays with a magnet, the ring also moves with no perceptible lag, being at the same time somewhat deformed, but still retaining its hollow character.

By means of a tube in which the carbon anti-cathode is connected to the positive terminal by sliding connections, and can be caused to move along the tube so as to intersect either the convergent or di-

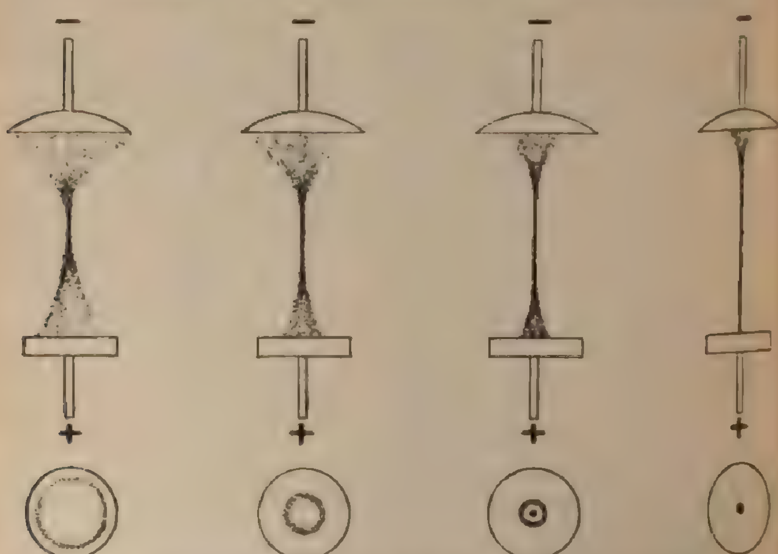

FIG. 7.—Appearance and effect of the cathode rays in a focus tube at four different degrees of exhaustion.

vergent cones at any desired point, it can be shown that with cathodes of considerable concavity, both the convergent and divergent cones of cathode rays are never solid but always more or less hollow in section.

Now, how can this remarkable effect be explained? Perhaps the most satisfactory explanation is that suggested by Professor G. F. Fitzgerald, which accords with the Crookes theory of cathode rays and also with what I have already mentioned as to the anode stream of positively charged atoms returning to the cathode outside of the cathode stream passing in the opposite direction. If we return to Fig. 6, it is evident that the supply of atoms to the active cathode

surface is from all round the edge of the latter, so that the atoms may very possibly be all shot off again from the cathode in the form of a hollow cone before they get further than a certain distance towards the centre. Further, as the vacuum increases we know from our experiments with our radiometer tube that the velocity of the positive stream also increases very considerably, so that under the conditions of a higher vacuum the atoms approaching the cathode have more momentum and consequently get nearer to the centre before they obtain a negative charge and are repelled in the cathode stream, thus making the stream and the rings smaller in diameter. Of course, once we start with a hollow convergent cone it is easy to understand that the divergent cone will also be hollow, seeing that the atoms fly more or less rectilinearly crossing one another's paths at the focus. How to explain the bright spots in the centres of the rings, which appears to indicate a central negative stream down the axis of the hollow cones, is more difficult, but possibly the heterogeneous nature

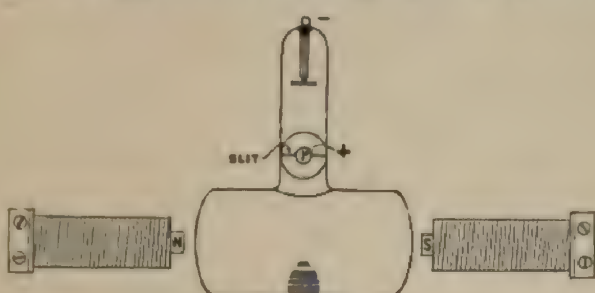

FIG. 8.
Apparatus for showing the cathode ray spectrum.

of the cathode stream, due very probably to the varying velocities of the negatively charged atoms, may be sufficient to account for this.

Crookes observed many years ago that cathode rays were deflected by a magnet. Lenard was the first to show that the rays are not homogeneous, but some are more easily deflected than others. Birkeland went one step further than this, and showed that if a thin cathode beam was deflected by a suitable magnetic field it was split up into bundles of rays, and if allowed to fall upon the glass walls of the tube, it gave fluorescent bands of alternate brightness and darkness, which he termed the magnetic spectrum.

Fig. 8 represents an apparatus for showing this effect. The cathode rays proceeding from a flat aluminium disc are caused to pass through a narrow slit in a piece of platinum which serves as the anode. After passing through the slit, the rays impinge upon the bulb, and if otherwise unaffected, produce a narrow band of intense luminescence upon the glass. At each side of the bulb is

fixed an electromagnet, producing straight magnetic lines across the path of the rays. As soon as the magnets are excited the cathode beam is deflected and split up, and instead of having a single narrow line of luminescence, we now have many lines with dark intervening spaces, all in constant movement. An experiment like this cannot be shown to an audience, but I have prepared photographs which will make the effect produced clear. Fig. 9 is a photograph taken without camera or lens, and produced simply by binding a strip of sensitive photographic film round the bulb of the tube and making a single discharge by a single break of the contact-breaker of the induction coil. The film being in close contact with the glass is impressed by the luminous bands that the unequally deflected cathode rays produce on the latter, and we have a photographic image of the bands for a single electrical discharge. Nor is this all. By inserting between the glass and the photographic film a piece of very thin black paper, so placed as to cover only one-half of the spectrum, it is possible to obtain a photograph of the bands, one half of which is due to the visible fluorescent luminosity of the glass, and the other half to the invisible Röntgen rays produced by the impact of the cathode rays on the glass.

Fig. 10 is such a photograph, and it will be seen that the Röntgen rays are also given off in bands, which are co-terminous with the fluorescent bands though photographically fainter than the latter. In the photographs shown, this difference in density between the two images is lessened by the interposition between the glass and the film in the case of the luminous portion of a thin sheet of slightly yellow celluloid. Without this the difference would be so great that it would not be possible to show both images upon a single film. Of course, this faintness of the Röntgen ray bands is only to be expected, as in the photograph of the luminous bands the Röntgen rays are also present, so that in the one case the photographic image is the result of both descriptions of radiations, and in the other is caused by only one. It is worthy of note that in the spectrum image produced by the Röntgen rays, the greatest photographic effect is always produced by the least deflected of the cathode ray streams, that is to say, by that stream which presumably was travelling at the greatest velocity. It is obvious that the cathode ray atoms which are travelling most rapidly will be the ones least deflected, just as the faster is the flight of a bullet the flatter is its trajectory. Here we have a probable explanation of the existence of the bands which most likely are due to the atoms of the cathode rays having either from the first different velocities imparted to them, due to the oscillatory character of the induction coil discharge, or from their gathering into groups travelling at different velocities on the well-known principle that occasions the traffic in the street to form knots of maxima and minima, owing to the faster vehicles catching up the slower and being impeded by them.

The axial stream in the centre of the hollow cathode ray cones may possibly also be due to the same cause.

FIG. 9.

Cathode ray spectrum image photographed directly by the visible fluorescent radiations.

FIG. 10.

Cathode ray spectrum image photographed, one half by the visible radiations, and one half by the invisible Röntgen radiations.

In any case the photographs that I have shown you prove very conclusively that those negative atoms which are least deflected by a magnet are those which produce the most active Röntgen rays, and therefore it follows that the quality of the Röntgen rays is very largely dependent upon the velocity with which the negative atoms strike upon the anti-cathode. Quite in harmony with this theory is an experiment which I will now show you. I have here a Röntgen ray tube with two cathodes, as shown in Fig. 11. The cathodes are both in the same tube, and therefore the conditions as regards vacuum must be the same for both. They both focus upon opposite sides of the same platinum anti-cathode, and they only differ in the fact that

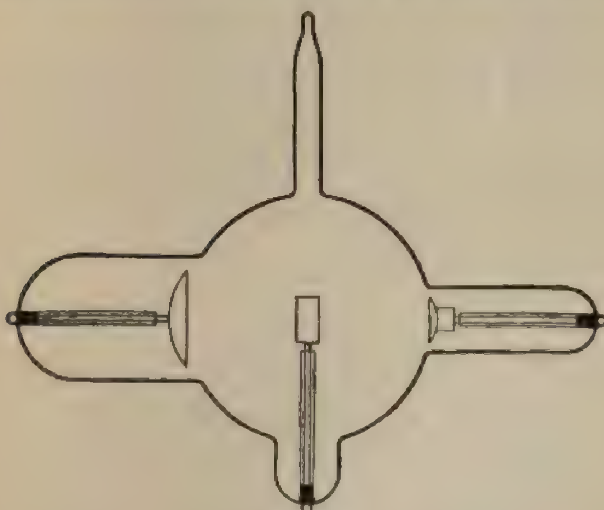

Fig. 11.
Focus tube with two cathodes of different diameters.

one is considerably larger than the other. I will now put the tube into operation, using the larger cathode, and as you see, scarcely any Röntgen rays are produced, while what there are do not penetrate my hand. I will now alter the connections and use the smaller cathode instead of the larger one. Now very penetrative Röntgen rays are generated in abundance, and you can clearly see the shadow of the bones in my hand on the fluorescent screen.

Here is another tube which is furnished with four cathodes all of different sizes and all arranged to focus upon the same anti-cathode, which can be rotated so as to face the particular cathode in use. This tube behaves just like the other, and for any given degree of exhaustion gives more penetrative Rontgen rays the smaller the cathode employed.

It is found that the smaller the cathode the greater is the E.M.F. required to cause the electric discharge to pass through the tube, and probably in consequence of this, and also perhaps because a less number of atoms can get into the vicinity of the cathode at one time, the greater is in all probability the velocity of the stream of atoms that form the cathode rays.

The particular material employed for the anti-cathode surface also materially affects the production of the Röntgen rays. This is a subject that was, I believe, first investigated by Professor Silvanus Thompson, who found that the best absorbents were the best emitters of the Röntgen rays—in other words, that the best materials for the anti-cathode were metals of the highest atomic weight. If the Röntgen rays are produced by the sudden removal of velocity from the cathode ray atoms, or by a sudden change in this velocity by collision with the atoms of the anti-cathode, this is in accordance with what would be expected, as substances of high atomic weight would obviously be the most efficient by reason of the greater inertia of their atoms.

I have made numerous experiments with various metals for the anti-cathode, and I have here a tube which has a movable anti-cathode made half of aluminium and half of platinum. By jerking the tube, either the platinum or the aluminium portions can be brought opposite the cathode and put into use, so that under exactly similar conditions as regards vacuum, size of cathode and bulb and distance, it is possible accurately to compare the efficiency of the two substances. Fig. 12 is a photograph of my wrist taken with the platinum portion of the anti-cathode, and Fig. 13 one taken with the aluminium portion. The conditions were otherwise identical, but, as is very obvious, the result with the platinum is much superior to the other.

The usual method adopted for varying the resistance of a Röntgen ray tube, and thus modifying the character of the Röntgen rays that it produces, so as to obtain the exact penetrative quality that is desired, is by varying the vacuum. The higher the exhaustion the greater is the resistance to the passage of the discharge, the greater is the velocity of the cathode rays, and the more penetrative are the Röntgen rays. This variation of the vacuum is usually effected by heating the tube, which has the effect of driving out into the interior molecules of the residual gas condensed or occluded upon the glass. Apart from this, very possibly the temperature of the contents of the tube and the kinetic energy of the molecules, which is greater the higher the temperature, may in itself assist the passage of the discharge.

There are, however, other means of varying the resistance of a tube and altering the character of the rays that it generates which do not depend upon either the degree of exhaustion or upon the temperature. One method for effecting this regulation consists in making the anti-cathode, which is also the anode, movable, and altering the distance between it and the cathode; another in making the cathode movable and altering its position relative to the glass walls of the tube.

FIG 12.

PLATINUM.

FIG. 13.

ALUMINIUM.

Photographs showing relative results obtained with anti-cathodes of platinum and aluminium.

ıe former case the tube may be constructed as shown in Fig. 14, ıich the anti-cathode is mounted on a sliding stem so that by

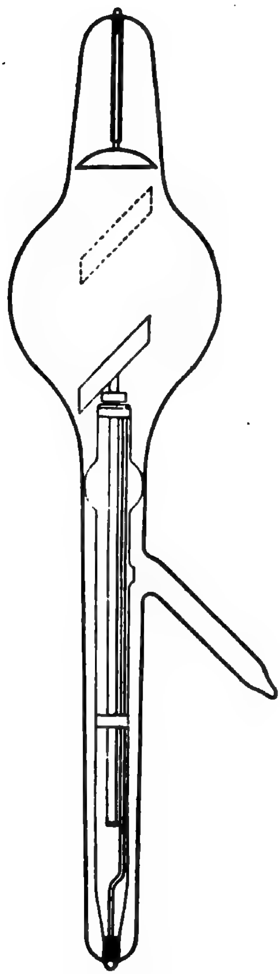

Fig. 14.
Adjustable anode tube.

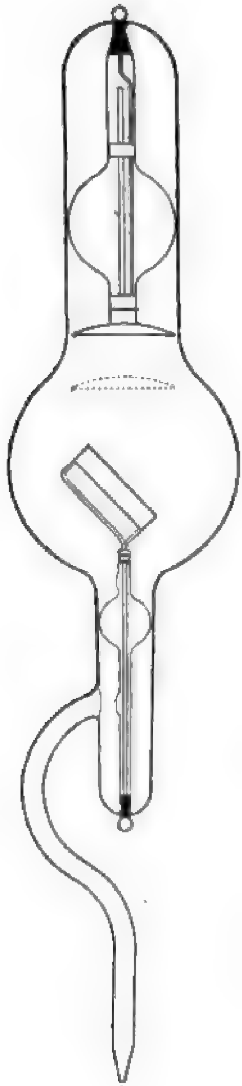

Fig. 15.
Adjustable cathode tube.

shaking the tube its distance from the cathode can be varied. In this case the nearer the anti-cathode is placed to the cathode the higher is the resistance of the tube and the more penetrative are the Röntgen rays that are generated.

Fig. 15 shows another form of adjustable tube in which the anti-cathode is stationary, and it is the cathode that is movable. The cathode is here so mounted upon a sliding stem that it can be moved in and out of a slightly conical annex blown upon one side of the glass bulb of the tube. I will put a tube of this description into operation, beginning with the cathode in the position shown in the illustration in dotted lines, when it is outside the annex in the bulb, and let you see the effect of gradually moving it backwards into the annex. We will observe the character of the resulting Röntgen rays produced at each position with a fluorescent screen. The tube used has a small piece of iron attached to the cathode so that we can move the latter by means of a magnet according to the suggestion of Dr. Dawson Turner and others.

You observe that, to commence with, with the cathode right out in the bulb we get Röntgen rays which can do little more than penetrate the black paper backing of the screen. My hand throws a dark shadow on the fluorescent surface, but you can see no bones, as the rays will not penetrate my hand. I now move the cathode a little back towards the edge of the annex. The bones are now just visible. The hand is still very black, but the bones can be seen; now on moving the cathode just inside the edge of the annex the bones become very clear, and when I move it still further into the annex the rays become very penetrative, and even pass through the bones so that their structure can be observed.

Figs. 16, 17 and 18 show a series of three photographs of my hand obtained in this manner. They were all taken with the same tube under identical conditions as regards vacuum, distance, exposure, photographic plate and development. The position of the cathode only was altered, and, as will be observed, the results show a marked increase of penetration the further the cathode was moved towards and into the glass annex. In the case of Fig. 16 the cathode was right out in the bulb, in Fig. 18 it was completely in the annex. In Fig. 17 it was in an intermediate position.

Now we have studied the cause of these effects by means of a tube in which positions of both anode and cathode can be altered independently by a magnetic adjustment. Fig. 19 shows a portion of the tube, and above it is drawn a curve representing approximately the difference of potential required to cause a discharge to pass through the tube with varying positions of the anti-cathode. In the diagram the abscissæ represent the distance between anti-cathode (which also formed the anode) and the cathode, divided in tenths of an inch, while the ordinates represent also in tenths of an inch the length of the alternative sparks in air between two brass balls ¾ inch in diameter. Starting with the anti-cathode in its furthest position

FIG. 16.

FIG. 17.

FIG. 18.

Photographs showing varying penetration obtained with an adjustable cathode tube.

from the cathode, and moving it gradually towards the latter, it will be observed that at first there is a slight gradual increase in the length of the alternative spark. Then for the next small movement

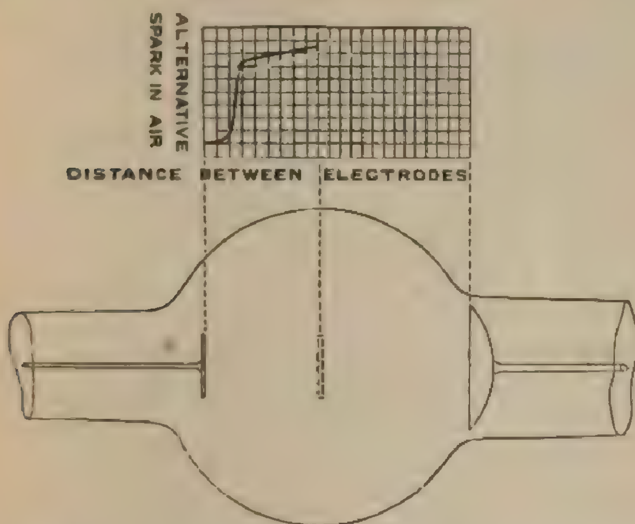

FIG. 19.—Diagram showing how the resistance of a tube is altered by varying the position of the anode.

there is a very sudden increase, and after that again a gradual increase till we get to the point marked in dotted lines, which denotes the limit of travel that the anti-cathode was allowed.

Now let us come to Fig. 20, which represents the effect of moving the cathode in the same tube, the anti-cathode being stationary in the position shown. Here, as will be seen, the less the distance between cathode and anti-cathode the less is the length of the alternative spark.

This distance in this case does not appear, however, to be the determining factor, as it is more than counterbalanced by the more important factor of the position of the cathode relatively to the glass walls of the tube. We have a gradual decrease in the length of the alternative spark as the cathode is moved a little towards the anti-cathode, then a further much more rapid decrease as the cathode emerges from the annex, and a still further slight decrease as it is moved away from the glass walls out into the bulb.

Now as to the effect upon the Röntgen rays, as it has been before remarked, the greater the resistance of the tube and the greater the E.M.F. necessary to cause a discharge to pass, the greater is the

velocity of the atoms that form the cathode rays, and the more penetrative are the Röntgen rays produced. Further, so far as the moving

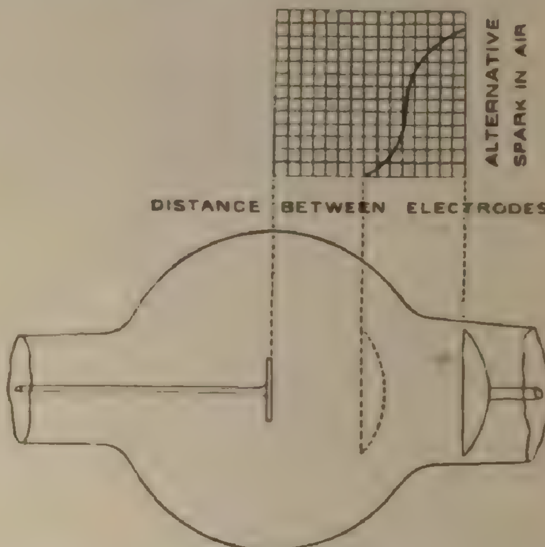

Fig. 20.—Diagram showing how the resistance of a tube is altered by varying the position of the cathode.

cathode is concerned, the supply of atoms appears to be of great importance. If penetrative Röntgen rays are desired the access of atoms to the cathode must be restricted. If only a few atoms can get to the cathode these are projected at great velocity; if there is too ready access the atoms crowd in upon the cathode and the electrical charge of the latter is unable to throw them off with much speed. It is possible to restrict the supply of atoms to the cathode either by bringing the latter back into a recess or annex, as in the tube just shown, or a tube such as is illustrated in Fig. 21, in which both cathode and anti-cathode are fixed, but in which there is a movable conical glass shield which can be brought up from behind the cathode so as to impede the access of the atoms, which, as we have seen, come in round the edges of the cathode, to any desired extent. This tube regulates just as did the adjustable cathode tube, and its efficacy goes a long way to prove that the theory as explained above is substantially correct.

In order to produce sharply defined Röntgen photographs it is of course of the utmost importance that the rays should be given off

from a very small area or point. The sharpness of definition varies considerably with different tubes, and a ready means of judging as to their quality in this respect is very useful. I have here a very pretty arrangement for this purpose which is the idea of Mr. Mackenzie Davidson. It consists simply of a square wooden frame over which are stretched at equal distances a number of parallel wires. There are two sets of wires crossing one another at right angles. By holding this screen near the tube and examining the shadows cast by the wires upon a fluorescent screen at different distances, it is easy to see whether the definition of the tube is good or bad. Here are three Röntgen photographs of the wires, all taken at the same distance but with different tubes. As will be observed, they vary very considerably as regards distinctness, showing that the tubes were very unequal in definition.

Fig. 22 shows a photograph of the wires taken almost in the plane of the anti-cathode, the shadow of which is visible at the right of the picture. As will be observed, the shadows of the wires parallel to the plane of the anti-cathode become less and less distinct the further they are from the latter, while the wires that are at right angles to the anti-cathode plane are exceedingly indistinct. This is of course due to the Röntgen rays being given off from a spot of considerable area in the particular tube with which this photograph was taken, and to the projection of the active area

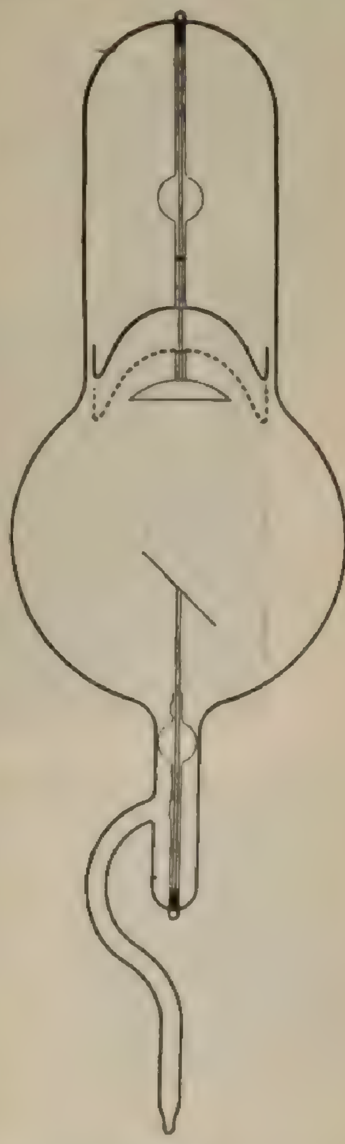

Fig. 21.
Tube with adjustable glass shield.

becoming more and more of a line when viewed nearer and nearer towards the plane of the anti cathode.

The best and most accurate way of investigating the area of the anti-cathode from which the Röntgen rays proceed is by means of pin-hole photography. Seeing that the Röntgen rays are not refracted, photography with a lens is of course out of the question, but with a pin-hole very fairly accurate and distinct images can be obtained. It is only necessary to place a sheet of lead, pierced by a pin-hole, near the tube, and then to examine the rays coming through the hole with a fluorescent screen placed some way behind the lead

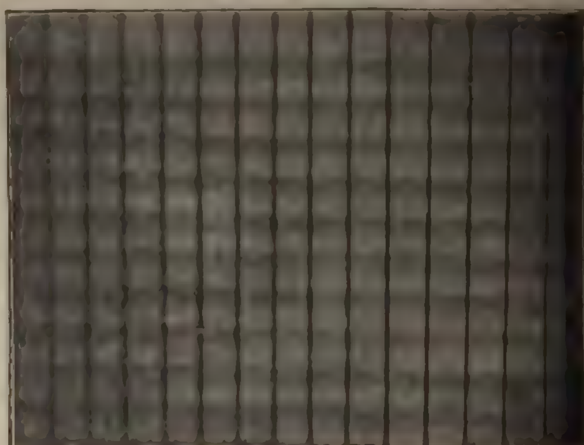

Fig. 22.—Röntgen ray photograph of a wire screen, taken almost in the plane of the anti-cathode, showing astigmatic effect.

sheet, in order to see exactly the size and shape of the active area of the anti-cathode; or instead of the screen a photographic plate may be employed and the effect recorded.

Fig. 23 shows four pin-hole photographs of the anti-cathode taken in this way, giving the effect produced with four different distances between the cathode and anti-cathode. The largest figure is produced with the greatest distance, and *vice versâ*. It will be observed that owing to the anti-cathode being placed obliquely to the cathode the figures are all oblique, though somewhat imperfect, conic sections: further, that when the distance between cathode and anti-cathode is great, we have a section of the divergent cone giving a hollow ring with a central spot, just as was visible with the carbon anti-cathode. The ring gets smaller and smaller, and finally disappears as the distance between the electrodes is reduced and the focus approaches the anti-cathode. It will also be noticed that where in the ring portion of the figures the cathode rays strike most normally, that is to say,

Fig. 23.

Pin-hole Röntgen ray photographs of the active anti-cathode surface for four different distances between cathode and anti-cathode.

Fig. 24.

Similar photographs to the above, but taken almost in the plane of the anti-cathode, across the major and minor axes of the active anti-cathode surface.

at one of the two points of greatest curvature of each ellipse, the Röntgen rays are produced more actively than in the remaining portion, where the cathode rays impinge on the anti-cathode more on the slant. This is still more marked in Fig. 24, which shows what are practically sections through the major and minor axes of one of the images shown in Fig. 23. They were taken similarly to the others, but with the pin-hole and photographic plate almost in the plane of the anti-cathode.

By some it is imagined that because the Röntgen rays are so very penetrating, therefore they are of the nature of an invisible light of great intensity, which though not affecting the human retina acts upon photographic plates very powerfully. This is quite erronous, and as a matter of fact the photographic effect of Röntgen rays is relatively very feeble. I have investigated this by means of two photographic plates which I have exposed respectively to a very powerfully excited Röntgen ray tube, screened by black paper to remove the visible luminosity, and to the light of a single standard candle. The Röntgen ray tube was employed at a distance of two feet, and the candle at a distance of ten feet, so that according to the law of inverse squares, which holds good for Röntgen rays as for light, the intensities of the two radiations, supposing them to be equal to start with, would be in the proportion of 25 to 1. Each plate was exposéd in sections for varying lengths of time, five, ten, fifteen seconds, and so on, each succeeding section being exposed five seconds longer than the preceding one. By sliding the two negatives past one another it is possible to compare them very accurately, and the section exposed to the light of the standard candle for ten seconds is almost exactly of equal density to the section exposed to the Röntgen rays for twenty-five seconds. The photographic power of this particular Röntgen ray tube—and it was a very good one—was therefore less than one-sixtieth of that of one standard candle.

With regard to the true nature of the Röntgen rays there have been many theories. There is the original suggestion of Röntgen himself, that they may possibly consist of longitudinal waves in the ether. Others have thought that they were possibly ether streams or vortices. There is a theory propounded by Tesla and others that they consist of moving material particles, atoms or corpuscles, similar to the cathode rays, which reminds one of Newton's corpuscular theory of light. There is the more generally received doctrine that they are simply exceedingly short transverse ether waves similar in all respects to the waves of light, only so much shorter than the most ultra-violet waves hitherto known that they pass between the molecules of matter, and are consequently neither refracted nor easily absorbed or reflected by any media. Lastly, there is the theory first suggested to the writer early in 1896 by Professor George Forbes, and recently independently enunciated and elaborated by Sir George Stokes, which imagines them to be frequently but irregularly repeated, isolated, and independent disturbances or pulses of the ether, each pulse being similar perhaps to a single wave of light, and consisting

of a single transverse wave or ripple, but the pulses following one another in no regular order, or at any regular frequency as do the trains of vibration of ordinary light.

Then again, there is the question of the mechanism by means of which the Röntgen rays are produced. They are generated by the impact of the cathode rays upon the anti-cathode, and it is now becoming more and more certain that the cathode rays consist of negatively charged atoms travelling at enormous velocity. If we accept this view, there are obviously several methods by which we may imagine the Röntgen rays being generated by the impact of the travelling atoms upon the anti-cathode. Each cathode ray atom carries a negative charge, while the anti-cathode is positively charged, so that when the two come into contact an electrical discharge will take place between them. An electrical oscillation will thus take place in the atom just as in the brass balls of a Hertz oscillator, and transverse electro-magnetic waves will be propagated through the ether in all available directions. As the electro-static capacity of the atom must be exceedingly small, the periodicity of oscillation and the wave frequently will be enormous, while at the same time the oscillation will probably die out with sufficient rapidity to admit of only one or two complete periods. At the same time the greater the difference of potential between atom and anti-cathode at the moment of impact the greater will be the amplitude of oscillation, and the more vigorous and far-reaching the etheric disturbances.

Or we may imagine a more purely mechanical origin for the Röntgen rays. It is believed that the velocity of the cathode rays is enormous, being, as recently measured by J. J. Thomson, over 10,000 kilometres per second, and though Lodge, in his well-known endeavours to detect a movement of the ether by dragging a material body through it, obtained only negative results, of course he could not possibly obtain any velocity at all comparable to this. Assuming that at the velocity of the cathode ray atoms these do appreciably drag the ether with them, there may be some ether effect produced analogous to the atmospheric effect that is noted as the crack of a whip or a clap of the hands, as each atom hits the anti-cathode and rebounds.*

Or again, it is conceivable that the phenomenon is merely one of heating, and that the cathode ray atoms are by impact with the anti-cathode raised to such an enormous temperature that they give off for a short space of time super-ultra-violet light. Taking a velocity for the atoms of 10^9 centimetres per second, as found by J. J. Thomson to be the minimum velocity of the cathode rays, and calculating the temperature to which a nitrogen atom would be raised if, when travelling at this speed, it were instantly brought to rest

* Since the above was written, the writer's attention has been drawn to Professor J. J. Thomson's paper, "A Theory of the Connection between Cathode and Röntgen Rays," in the 'Philosophical Magazine' for February, in which it is suggested that Röntgen rays consist of very thin and intense electro-magnetic pulses produced in the ether by the sudden stoppage by the anti-cathode of the electrified particles of the cathode rays.

and the whole of its energy converted into heat in the atom itself, we have according to the formula

in which
$$T = \frac{V^2}{2JS},$$

T = the rise in temperature in degrees centigrade;
V = the velocity in centimetres per second;
J = joules equivalent;
S = the specific heat of nitrogen, we have the result that the rise in temperature is no less than the stupendous figure of approximately 50,000,000,000 degrees centigrade.

This is upon the probably erroneous assumption that the specific heat remains constant; but allowing for this, and even allowing for the merest fraction of the energy being converted into heat in the atom itself, there is obviously an ample margin to admit of a temperature being actually obtained enormously transcending the electric arc or anything of which man has any knowledge. Perhaps it may be objected that it is only when we come to deal with aggregations of atoms that we can speak of heat, and that a hot atom is a physical absurdity. If, however, we look upon heat as a rhythmic dance of the atoms, perhaps we may also contemplate the possibility of a single atom executing a *pas seul*, and giving pulses to the ether at each of its movements. In any case this difficulty disappears if we imagine the cathode ray particles each to consist of an aggregation of atoms.

The fact that substances of high atomic weight form the most efficient anti-cathodes, lends force to the suggestion that the Röntgen rays are produced in some way by the sudden removal of velocity from the atoms that form the cathode rays, owing to the collision of these latter with the comparatively stationary atoms of which the anti cathode is composed; while the effect observed with the pin-hole photographs of the anti-cathode, in which, as has been seen, the cathode rays that strike the anti-cathode most normally are the most effective in producing Röntgen rays, is also in accordance with this view. At the same time, the fact that in Röntgen ray photographs of Birkeland's cathode ray spectrum it is always the least deflected ray that produced the greatest photographic effect, goes to show that the higher the velocity of the cathode ray atoms the more effective these latter are in generating the Röntgen rays.

In conclusion, I must express my great indebtedness to the very able assistance of Mr. J. C. M. Stanton and Mr. H. L. Tyson Wolff. The latter has blown nearly all the tubes that I have been able to show this evening, while the aid of the former has also been of great value in a class of experimenting that requires much time and labour.

More than two years have now elapsed since the date of Röntgen's discovery, and nearly twenty years since the commencement of the researches of Crookes. Here, as always, we find that "Art is long, opportunity fleeting, experiment uncertain, judgment difficult." Thus wrote the Greek Hippocrates some twenty-three centuries ago, and time has not impaired the truth of the ancient aphorism.

[A. A. C. S.]

GENERAL MONTHLY MEETING,

Monday, February 7, 1898.

Sir James Crichton-Browne, M.D. LL.D. F.R.S. Treasurer and Vice-President, in the Chair.

Enrique Cortes, Esq.
J. S. Fairfax, Esq.
Mrs. S. Fisher,
George Humphreys-Davies, Esq.
Oliver Imray, Esq.
John William Jarvis, Esq.
Ivan Levinstein, Esq.
John Stewart MacArthur, Esq. F.C.S.
The Right Hon. Lord Monk Bretton.
Frederick James Quick, Esq.
Emanuel Ristori, Esq
Charles L. Samson, Esq.
Mrs. R. Lawrence Smith,
Rear-Admiral Arthur Knyvett Wilson, V.C. C.B.

were elected Members of the Royal Institution.

The following letters were read:—

"*To the Treasurer of the Royal Institution of Great Britain.*

"Dear Sir James, "*January*, 1898

"As an expression of his attachment to the Institution with which he was so long connected, and of his sympathy with its objects, my dear husband desired me (at such time as should be most convenient to myself) to present, in his name, to the Royal Institution a thousand pounds: to be disposed of, as the Board of Managers may see fit, for the promotion of science.

"I have now the pleasure of remitting to you this sum.

"Yours faithfully,
(Signed) Louisa C. Tyndall."

"61 Carlisle Place Mansions,
"Victoria Street, S.W.

"Dear Mrs. Tyndall, "*January* 17*th*, 1898.

"I have to acknowledge your letter, enclosing a crossed cheque of the value of £1000. This generous donation to the funds of the Royal Institution, given by your late husband's expressed wish, will be notified to the Managers and to the Members generally at their next meeting, when a formal acknowledgment of their grateful appreciation of it will be communicated to you. Meanwhile I trust you will allow me to express my own sense of the munificence of the gift and of the simple and touching terms in which it has been conveyed.

"The Managers would I am sure, desire to be guided by any wish of yours as to the application of the gift, but in the absence of any explicit directions they will, I have no doubt, employ it in the promotion of original scientific research, in which your husband's vivid and penetrating intellect delighted to exercise itself.

"Revered as your late husband's memory is, and ever must be, in the Royal Institution, this posthumous mark of his solicitude for its welfare will, if possible, deepen the affectionate esteem in which he is held.

"There is not, I regret to say, in the Royal Institution any worthy presentment of the late Professor Tyndall. You have, I believe, a really good bust of him, and I should be glad to know if you would feel disposed to afford facilities for having a replica of that made for the Royal Institution.

"With kind regards,

"Yours very faithfully,

(Signed) JAMES CRICHTON-BROWNE."

Moved, seconded, and carried unanimously,

"That the Special Thanks of the Members of the Royal Institution of Great Britain, in General Meeting assembled, be returned to Mrs. Tyndall, for her generous Donation of One Thousand Pounds given in fulfilment of the wish of her husband, the late Professor Tyndall, for the promotion of Science, which he did so much by his life-long labours to advance, and in token of his sympathy with the objects of the Royal Institution to which he rendered such signal service."

The Special Thanks of the Members were returned for the following donations to the Fund for the Promotion of Experimental Research at Low Temperatures:—

Sir Frederick Abel, Bart.	£50
Professor Dewar	£100
Sir Andrew Noble, K.C.B.	£100

The Special Thanks of the Members were returned to the Rev. William J. Packe for his present of an Electric Lamp and Fittings.

It was announced from the Chair that the Managers had resolved, at their Meeting held this day, that the Centenary of the Royal Institution, which was founded in 1799, would be properly celebrated next year.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

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WEEKLY EVENING MEETING,

Friday, February 11, 1898.

Sir Edward Frankland, K.C.B. D.C.L. LL.D. F.R.S. Vice-President, in the Chair.

J. H. Gladstone, Esq. D.Sc. F.R.S. *M.R.I.*

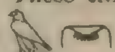
The Metals used by the Great Nations of Antiquity.

At the beginning of this century little was known of the great nations of antiquity, except through the classic poets and historians, and the sacred writings of the Hebrew people. Since then our knowledge has been enormously increased by the labours of scholars and explorers; the ruins of ancient cities have been exhumed, and the contemporary literature of Egypt and Assyria, inscribed on papyri or tablets of clay, and painted or carved on the walls of temples, palaces and tombs, has been deciphered. What is in some respects still more important is, that objects found in these ruins have thrown great light upon the daily life of the people and their ornamental and useful arts. One of the departments of this inquiry concerns the metals used by the different nations, and at the different epochs of their history; and it is to this department that my attention will be confined this evening. The difficulty I experience is the vast amount of material; and I cannot attempt anything more than a general view of the subject and some of the most salient points.

The area over which the inquiry extends is that of the lands bordering on the eastern half of the Mediterranean, and stretching eastwards to the Persian Gulf. The time, so far as Egypt is concerned, includes the whole period from the first Pharaoh, Menes, to the conquest of the country by Alexander the Great; ranging from about B.C. 4400 to B.C. 332. The chronology employed throughout is that of Dr. Wallis Budge, of the British Museum, who has adopted in the main that of Brugsch Bey. This period of 4000 years appears to me of reasonable length, and errs, if anything, on the side of moderation. Our knowledge of the other nations does not extend to anything like so remote a time.

Egypt.

If we take as our starting-point Seneferu's triumphal tablet in Wady Maghara, in the Sinaitic peninsula, we see the king flourishing his battle-axe over the head of his enemy. This symbolises the conquest of the copper and turquoise mines of that region, and implies of course their previous existence as a source of wealth. In the

hieroglyphic inscription above his head there is not only the king's name spelt phonetically, but in the royal titles are seen two ideographs which bear upon our subject. One is the necklace or ornamental collar , which is the well-known symbol for gold; and the other an axe , the head of which resembles that of a copper rather than of a stone weapon. These titles have no reference to the metals themselves, but mean  "Golden Horus," and  "Beneficent Divinity." Before such symbols could be used to express abstract ideas, they must have been well known in their concrete form. The date assigned to Seneferu is B.C. 3750; but the discoveries of the past year have put in our possession the actual metals themselves, of a much greater antiquity. M. de Morgan, late Director General of Antiquities in Egypt, has explored an enormous royal tomb at Nagada, the centre chamber of which contained the mummy of the Pharaoh, with the cartouche of King Menes, the reputed first King of Egypt. If it be really his tomb, the probable date will be B.C. 4400. What is interesting to us is that in two of the chambers, among a multitude of articles made of ivory, quartz, porphyry, wood, alabaster, tortoiseshell, mother-of-pearl, obsidian, earthenware, cornelian, glass and cloth, there were found some small pieces of metal, viz. two or three morsels of gold, and a long bead of that metal of a somewhat crescent form, together with some articles of copper—a kind of button, a bead, and some fine wire.* The button was analysed by M. Berthelot, the well-known French chemist and politician, to whom we are indebted for the examination of a very large number of ancient metallic objects; he states that it is nearly pure copper, without arsenic or any other metal in notable proportion.†

These are the oldest metallic objects in the world to which we can assign a probable date. But Prof. Flinders Petrie had discovered three years ago, also at Nagada, a great number of objects of the same character, and among them a few small copper implements. Some filings from a dagger, a celt, and a little harpoon were analysed by me, and found to consist of practically pure copper, without any trace of tin. The remains of these filings are in the little bottles on the table. The age of these tools must be comparable with that of the royal tomb, and may possibly be even older.

Of about the same period, and perhaps even earlier, are a number of tombs at and near Abydos, which have been explored by M. Amélineau, bearing the names of kings unknown to history, accompanied by hieroglyphics of archaic form.‡ In these have been found

* See 'Ethnographie Préhistorique et Tombeau royal de Négadah,' par J. de Morgan; Paris, 1897; pp. 162-3 and 195-8, in which these articles are described and drawn.

† Annales Ch. Ph. Avril, 1895.

‡ See 'L'Age de la Pierre et les Metaux,' par J. de Morgan; Paris, 1896; chap. viii.

larger quantities of copper utensils, viz. pots, hatchets, needles, chisels, &c., which M. Berthelot also finds to be nearly pure metal, but some contain a little arsenic. It would appear, therefore, that the Egyptians, at the very beginning of the historic period, were acquainted with the use of gold and copper.* Let us follow the history of these two metals, beginning with gold, which, as it is generally found native, was probably the first known to man.

According to a letter just received by me from M. Berthelot, all or nearly all the ancient gold that he has examined contains more or less silver. This pale coloured gold is sometimes termed electrum, and was found in great quantity in Asia Minor, where the Pactolus and other streams "rolled down their golden sands." Gold is frequently represented in the Egyptian sculptures and pictures; for instance, in the very interesting scenes of social life at Beni Hassan, *circa* B.C. 2100 illustrations of which I now throw upon the screen, we see the goldsmiths making jewellery, weighing out the metal, melting it in their little furnaces with the aid of blow-pipe and pincers, washing it, and working it into the proper forms. In the picture of a bazaar at Thebes we find a lady bargaining for a necklet; and in another picture we see the weighing of thick rings of gold and of silver, which were used as articles of exchange. I wish I could show you the exquisite gold jewellery, inlaid with gems, found in the tombs of four princesses buried at Dahshur, about B.C. 2350, and which is now exhibited in the museum of Gizeh; but I can throw upon the screen the photograph of the beautiful enamelled gold necklace of Queen Ahhotpu, B.C. 1700.† The great kings Seti I. and Rameses II., B.C. 1300, worked extensive gold mines in Nubia, which yielded gold free from silver.

To return to the history of copper. In the inscriptions we cannot distinguish between copper and its various alloys, for they are all expressed by the general term *chemt*, and the symbol of the battle-axe blade. But if we can get the substance itself and analyse it, we know what we are dealing with. Many specimens of copper implements, dating from the fourth to the sixth dynasty, say from B.C. 3750 to 3100, have been examined. They consist of almost pure copper. One of the earliest, analysed by me, was a piece of a vessel from El Kab, which contained 98 per cent. of copper, the remaining 2 per cent. being made up of bismuth, arsenic, lead, iron, sulphur and oxygen, evidently the impurities in the original ore.

It was evidently very important for the Egyptians to harden the copper as much as possible; and this might be effected in several

* Since the lecture was delivered the Egypt Exploration Fund has issued a memoir, under the title of 'Deshasheh,' from which it appears that in the very ancient tombs at that place there were found a few gold beads and copper objects, and a picture of an artificer weighing a copper bowl.

† For drawings see 'The Struggle of the Nations,' by G. Maspero, pp. 3 and 97.

ways: (1) by hammering, (2) by the admixture of arsenic, (3) by the admixture of tin, (4) by the admixture of zinc, (5) by the presence of a certain amount of oxygen in the form of cuprous oxide. As to arsenic, some of the oldest copper implements contain a notable quantity. Dr. Percy found 2·29 per cent. in a knife which was dug up some distance below a statue of Rameses II.; and I found 3·9 per cent. in a hatchet from Kahun, dating back to B.C. 2300. It is said, however, that the addition of 0·5 per cent. of arsenic is sufficient to produce a hardening effect; and many specimens of ancient copper implements contain this amount, though the proportion of arsenic in copper ores themselves rarely exceeds 0·1 per cent.

As to the admixture of tin. It is well known that bronze, the alloy of copper and tin, is stronger than pure copper. The extent of this depends upon the proportion of the two metals, and probably on other circumstances. The oldest supposed occurrence of an admixture of tin, is in a bronze rod, found by Flinders Petrie in a mastaba at Medum, probably of the fourth dynasty, which I found to contain 9·1 per cent. of tin.* It seemed so improbable that tin should be employed at so remote a period, and that in sufficient quantity to make what we call gun-metal, that I was suspicious of its genuineness, notwithstanding the very circumstantial account of its discovery; but M. Berthelot has since found in a ring from a tomb at Dahshur, believed to be not much later than the third dynasty, 8·2 per cent. of tin; and in a vase of the sixth dynasty, 5·68 per cent. of tin.† These seem to restore the credit of Dr. Petrie's specimen. At a later period weak bronzes become common. Thus, at Kahun, tools found in a carpenter's basket by Prof. Petrie contained varying amounts of tin from 0·5 to 10·0 per cent.; 6 or 7 per cent. of tin was subsequently common. Bronze implements abound in Egypt. I am able not only to throw upon the screen representations of arrow- and spear-heads and battle-axes, but, through the kindness of Sir John Evans, to show a beautiful large spear-head with an inscription of King Kames (B.C. 1750) down the blade. I am also indebted to Prof. Flinders Petrie and Dr. Walker for this collection of implements of the twelfth dynasty from Illahun, including a fine mirror with ivory handle, necklets, and a bronze casting for a knife, which was never finished; also many objects of the eighteenth dynasty, or thereabouts, such as a sword, dagger and axe, together with mirrors, bracelets, earrings and pendants, and a steelyard. My own collection contains specimens of what are believed to be razors of different types, and small statuettes of Osiris, Isis and others.

As to the admixture of zinc. There does not seem to be any specimen of brass, properly so called, found in Egypt within the period of our inquiry; but various attempts are known to have been

* Particulars of this and other analyses may be found in 'Proceedings of the Society of Biblical Archæology,' March 1890, March 1892, and March 1894.

† Feuilles a Dahchour en 1894, pp. 136–9.

made to imitate gold, of which aurochalcum is an instance, and that may have been yellow brass.

As to oxygen. It is generally supposed to exist in copper in the form of the red cuprous oxide; and most of the copper, and many of the bronze, implements have a covering of this substance. This is caused by the gradual formation of an oxychloride of copper through the action of alkaline chlorides in the soil, aided by the air and moisture. Berthelot has worked out the chemistry of this substance very fully, and shows how when once formed it gradually works its way into the solid metal, transforming it into the suboxide, and frequently disintegrating it. Some good specimens of little bronze images suffering this disintegration are exhibited by Mr. Joseph Offord. Two at least of the copper adzes on the table consist to the extent of 30 or more per cent. of oxide of copper; they are exceedingly hard, and it becomes a question whether the formation of the oxide is due to the slow chemical change, or whether it was purposely produced in the manufacture in order to harden them. The effect of different proportions of oxygen on the tenacity of copper is known to be very various, and certainly deserves further investigation.

It is difficult, or rather impossible, to express in definite figures the advantage gained by the ancient Egyptian metallurgists through this alloying of the copper. Arsenic, tin or zinc may and do affect the hardness, or the tenacity, or the elasticity, in different ways, and also according to the proportion of the metal united with the copper. Thus, there are several very different kinds of alloys of copper and tin, though they are all included under the name of bronze; moreover, a piece of copper which has been exposed to a considerable stress, is permanently altered in its properties. Again, in any table of numerical values, it should be taken into account whether the copper with which the alloys are compared had been made as pure as possible, or contained a normal amount of oxygen.* We must rest contented with the knowledge that copper can be rendered stronger and more serviceable by these means, and that the ancient artificers were acquainted with the fact.

After the extensive use of copper and bronze in ancient Egypt, other metals were gradually employed. Silver, as distinct from electrum, seems to have been little used, except for ornamental purposes.† The diadem of one of the kings named Antef (B.C. about

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* For tabulated results of experiments bearing on these points, see 'The Testing of Materials of Construction,' by Prof. Cawthorne Unwin; and the second Report to the Alloys Research Committee of the Institution of Mechanical Engineers, by Prof. Roberts-Austen, with the discussion thereon.—Proc. Inst. Mech. Eng. April 1893.

† In the translation of 'The Book of the Dead,' by Dr. Wallis Budge, vol. iii. published since the lecture, it appears that in one of the oldest chapters, said to have been found by Herutataf, about B.C. 3600, there is a formulary to be said over a scarab of greenstone encircled with a band of refined copper, and having a ring of silver.

2700), and that of the Princess Noubhotep (B.C. 2400), were made of silver and gold. Silver also occurs among the beautiful jewellery of the princesses buried at Dahshur, and that of Queen Ahhotpu. But when the intercourse between Egypt and the neighbouring nations of Asia was better established, silver became much more common; thus we find it frequently mentioned in the Great Harris papyrus, (B.C. 1200), in which the King Rameses III. describes his magnificent presents to the temples and priesthood of Egypt.* The metal lead also occurs frequently in the same lists, and was used, as elsewhere, for mixing with copper and tin in the formation of the easily fusible bronze used for statuary.

Tin has a very interesting history. We have found it used in combination with copper as far back as perhaps B.C. 3400, and enormous quantities of it must have been afterwards employed. It is still a question whether in the first instance some stanniferous copper ore was used, or whether the Egyptians found that the addition of a certain black mineral was advantageous for hardening their copper, or whether from early days they reduced the metal from its ore and added it to the copper in the furnace. That, at any rate, they were afterwards acquainted with the metal itself, is clear from the discovery by Flinders Petrie of a small ring at Gurob (B.C. 1450), which, on examination, I found to be of tin, imperfectly reduced from its ore. Berthelot has also analysed what was essentially a tin ring, though alloyed with copper, dating about a century later; and Prof. Church describes a scarab of the same metal, which was found on the breast of a mummy of about the seventh century B.C. This metal also appears more than once among the rich gifts catalogued on the papyrus of Rameses III., if "*tehi*" is to be so translated.

Although kohl, the sulphide of antimony, was used for blackening the eyebrows from a very early period, I am not aware of any metallic antimony in Egypt of older date than some beads found by Prof. Petrie at Illahun in a tomb of about 800 B.C. They proved to be fairly pure metal. It is curious that the art of preparing this metal was afterwards lost, and only rediscovered in the fifteenth century of our era.

The period of the first use of iron in Egypt is at present a matter of great controversy. Some contend for its use even in the mythological period, while others would bring it as late as 800 or 600 B.C. There exist the oxidised remains of some wedges of iron intended to keep erect the obelisks of Rameses II. at Tanis, which is near the border of Palestine; but there is no positive proof that they were placed there during his reign. I have little doubt, however, that the Black *Baa*, mentioned several times in the Harris papyrus, B.C. 1200, is the same as the μελας σιδηρος of Hesiod: i.e. iron. In the long account which King Piankhi gives of his invasion of Egypt from the Upper Nile, he mentions iron more than once among the presents

* 'Records of the Past,' vols. vi. and viii.

made to him by the minor chieftains of the time in token of their submission, indicating that at this period, B.C. 700, it was still not very common.

Assyria.

In the country lying between or near the Euphrates and the Tigris we have some antiquities dating, perhaps, as far back as any in Egypt. We have also a great amount of Accadian and Assyrian historical and other literature on tablets and cylinders of clay, and on the walls of the great palaces and temples. As in the case of Egypt, the discoveries of the remotest age are those which have been most recently published. Dr. Peters has just given us the records of the explorations of the American Oriental Society at Nippur, and describes the successive layers of the great temple of Bel.* These appear to indicate the absence of metal in very remote periods. The oldest specimens are those recently found by M. de Sarzec at Tello (Lagash) in Southern Chaldæa. They consist of some votive statuettes, and a colossal spear, an adze and curved hatchet—all of copper without tin, according to M. Berthelot's analysis. A small vase of antimony and a large one of silver have also been found. The period of these is supposed to be some considerable time anterior to B.C. 2500. At Tel el Sifr, in the same neighbourhood, Mr. Loftus discovered a large copper factory, in which were caldrons, vases, hammers, hatchets, links of chain, ingots, and a great weight of copper dross, together with a piece of lead. The date of these is believed to be about B.C. 1500. At Nippur the American explorers found at a higher level, in the temple of Bel, what they term a jeweller's shop, which consisted of a box full of jewellery, mainly precious stones, but also containing some gold and copper nails; these apparently date from about B.C. 1300. In Babylonian graves, and other places of about the same period, there have been found objects made of copper and iron and silver wire; but the use of metals seems much more restricted in these great alluvial plains than in contemporary Egypt. Iron, however, was perhaps an exception. According to Messrs. Perrot and Chipiez, excavations at Warka seem to prove that the Chaldæans made use of iron sooner than the Egyptians; in any case, it was manufactured and employed in far greater quantities in Mesopotamia than in the Nile Valley; in fact, at Khorsabad M. Place is said to have found hooks and grappling irons, fastened by heavy rings to chain cables, picks, mattocks, hammers, ploughshares, &c., in all about 157 tons weight. Mr. Layard also found at Nimroud a large quantity of scale armour of iron in a very decomposed state, but exactly resembling what is represented in the sculptures of warriors. Of this he collected two or three basketfuls.

Coming down to the period of the great Babylonian Empire, we find very large treasures of the precious metals changing hands

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* 'Nippur,' by Dr. Peters, Philadelphia.

during their sanguinary wars. Thus, on the black obelisk of Shalmaneser II., in the British Museum, we have depicted the embassies from different nations bringing their tribute to the feet of the king. The second of these has an inscription reading: "The tribute of Jehu, son of Omri; silver, gold, bowls of gold, vessels of gold, goblets of gold, pitchers of gold, lead, sceptres for the king's hand, and staves; I received." The gates of his palace at Balawat, now at the British Museum, were of stout timber strengthened with bands of bronze, and the Trustees kindly gave me a small piece of the metal for analysis; it yielded about 11 per cent. of tin. The grandson of this king, Rimmon Narari III., probably B.C. 797, took Damascus; and the spoil, according to the inscriptions, comprised 2300 talents of silver, 20 of gold, 3000 of copper, 5000 of iron, together with large quantities of ivory, &c.

Lenormant gives two verses of a magical hymn to the god Fire, which exist both in Accadian and Assyrian; they run—"Copper, tin their mixer thou art; gold, silver, their purifier thou art."

PALESTINE.

Between the great territories of Egypt and Assyria lies a narrow strip of country, small in extent, but very important in the history of civilisation, commerce and religion. During the period of which we are speaking it was occupied by a succession of different nations. It formed part of the possession of the great Hittite people. We cannot read their inscriptions, and we know little of their history. We have, however, bronze and silver seals that are supposed to belong to them, and curious bronze figures. They seem to have had abundance of silver, probably from the mines of Bulgardagh in Lycaonia. We read of Abraham purchasing a piece of land from Ephron the Hittite for which he weighed out "four hundred shekels of silver current money with the merchant." He was, in fact, rich in silver and gold, and among the presents given to Rebekah were jewels of silver and jewels of gold.

The first notice of metals in Palestine to which we can give an approximate date is in connection with the invasion of that land, and other countries further to the eastward, by the great Egyptian King Thothmes III.* He led his army through the plain of Esdraelon, and gained a victory at Megiddo, and amongst the spoil were chariots inlaid with gold, chariots and dishes of silver, copper, lead, and what was apparently iron ore. This took place about B.C. 1600. The original of the long treaty of peace and amity between Katesir, King of the Hittites, and Rameses II. is said to have been engraved on tablets of silver.

When the children of Israel left Egypt they were, of course,

* 'Records of the Past,' vol. ii.

acquainted with the metals used in that country. They borrowed the jewels of silver and gold of their oppressors; and of these the golden calf was afterwards made. We read, too, of the "brazen serpent," * and of elaborate directions for the use of silver, gold and brass in the construction of the Tabernacle. Lead is mentioned once, but iron seems to have been unknown to them, the word never occurring in the Book of Exodus; and though it is occasionally mentioned in the later Books of Numbers, Deuteronomy and Joshua, it is always with reference, not to the Israelites, but to the nations they encountered. Thus we read of the Midianites having gold, silver, copper, iron, tin and lead, which were to be purified by passing through the fire; of the King of Bashan, a remnant of the Rephaim, who had the rare luxury of an iron bedstead, which was kept afterwards as a curiosity at Rabbah; and of the spoil of the Amorite city of Jericho, comprising gold, silver, copper and iron. Later on the Canaanites were formidable with their "nine hundred chariots of iron;" and later still the Philistines, whose champion, Goliath of Gath, was clad in armour of bronze, and bore a spear with a heavy head of iron. Among the materials collected by David in rich abundance for the building of the Temple were gold, silver, bronze and iron; but the best artificers in metals were furnished by Hiram of Tyre, at the request of Solomon. During the reign of the latter there was an immense accumulation of these precious metals in Jerusalem. The comparative value of the different materials is indicated by the words of the prophet in describing the Zion of the future, "For brass I will bring gold, and for iron I will bring silver, and for wood brass, and for stones iron" (Isaiah lx. 17). Another prophet (Jeremiah vi. 29, 30) uses the simile of the refining of silver by the process of cupellation.

The great mound of Tel el Hesy affords a very perfect example of the débris of town upon town during many centuries; and of the light that these mounds throw upon the progress of civilisation. When Joshua, after the decisive victory of Bethhoron, led his troops to the plain in the south-west corner of Palestine, he besieged and took Lachish, a city of the Amorites. It then became an important stronghold of the Israelites; its vicissitudes are frequently mentioned at various dates of the sacred history, as well as on the Tel el Amarna tablets. The mound has lately been explored by Messrs. Petrie and Bliss; and in the remains of the Amorite city (perhaps B.C. 1500) there are large rough weapons of war, made of copper without admixture of tin; above this, dating perhaps from 1250 to 800, appear bronze tools, with an occasional piece of silver or lead, but the bronze gradually becomes scarcer, its place being taken by iron, till at the

* The word "brass," at the time of the translation of our Bible was used indiscriminately for copper or any of its alloys; so was also the corresponding Hebrew term. In the Old Testament it never refers to the alloy of zinc, to which the term brass is now confined.

top of the mound there is little else than that metal. The Palestine Exploration Fund has kindly lent me specimens of these finds for exhibition. About B.C. 700, Lachish was the headquarters of Sennacherib during his invasion of Palestine. From it he sent his messengers to Hezekiah, and at the same town he received the peace offering of the Jewish king, 300 talents of silver and 30 talents of gold, to raise which he had to despoil his palace and the Temple. In Sennacherib's own version of the transaction the silver is given as 800 talents, and the gold 30. Lachish was finally deserted about 400 B.C.

Greece.

We know little of the very early history of Greece, for the most ancient monuments bear no inscriptions, and literature did not commence till the time of the Homeric poems. In these, and in Hesiod, there are many graphic descriptions of the habits and arts of the heroic period, including the use of metals; and many of the towns described in them have recently been explored with great success, and have yielded up the very materials about which they sang.

Probably the earliest find has been in the volcanic island of Santorin, where, under beds of pozzolana, which are supposed to date about 2000 B.C., have been found two little rings of beaten gold and a saw of pure copper. In the Ashmolean Museum there are a very ancient silver ball and beads of the same metal rolled from the flat, also a spear-head of copper. These were obtained from Amorgos. In Antiparos there have also been found very ancient objects of silver mixed with copper.

Passing to the mainland, the towns of the Peloponnesus and the mound of Hissarlik, the supposed Troy, have been explored by Dr. Schliemann, Dr. Tsountas, and Dr. Dorpfeld; and they reveal what is termed the Mycenæan period, which figures so largely in the poems of Homer and Hesiod. In these the precious metals, gold and silver, are constantly mentioned, together with χαλκος generally translated brass. Thus, in the description of Achilles' shield, we are introduced to Hephaistos at his great forge on Etna, heating the bars of silver, or brass, or tin, or gold, and then hammering them on the anvil, so forming the designs which represent so beautifully the various scenes of peace and war. After having fashioned the shield, he is represented as forging for the warrior a cuirass of copper, greaves of tin, and a helmet with a golden crest.

Homer frequently mentions iron, but generally gives it the epithet "worked with toil," and treats it as a rare and costly metal. Thus a huge iron discus was given as a valuable prize to the hero who could throw it the farthest in the athletic games at the funeral of Patroclus.

Mr. W. E. Gladstone, who has long turned the great powers of his mind from time to time to Homeric studies, wrote me last summer: "The poems of Homer showed me, I think, forty years ago that they

represented in the main a copper age." The reasons he assigns in his letter, as well as in his published works, are fairly conclusive, and the recent explorations, and the analyses of Dr. Percy, Prof. Roberts-Austen, and others, have shown that in the early period of the Mycenæan age copper without tin was employed for numberless purposes; but as time advanced, bronze came into use. At Hissarlik, in the lowest and second city, have been found a gilded knife-blade, needles and pins, of practically pure copper; while in the third and sixth cities occur battle-axes of copper containing 3 to 8 per cent. of tin. In the very old town of Tiryns, the palace apparently had its walls covered with sheets of copper; much lead was also found there. At Mycenai, the Achaian capital, the metals in use were gold, silver, copper, bronze and lead; copper jugs and caldrons are common, and great leaden jars for storing grain; also elegant bronze tools and cutlery; mirrors, razors and swords. In the tombs the bodies are laden with jewels, largely ornaments of gold, with a much smaller amount of silver.

Some of these objects illustrate the poems of the time; thus, in the Odyssey we find Nestor making a vow to Athenæ: "So the heifer came from the field; . . . the smith came holding in his hands his tools, the means of his craft, anvil and hammer, and well-made pincers wherewith he wrought the gold. Athenæ, too, came to receive the sacrifice. And the old knight Nestor gave gold, and the other fashioned it skilfully, and gilded therewith the horns of the heifer, that the goddess might be glad at the sight of her fair offering." Now at Mycenai there was found the model of an ox-head in silver, with its horns gilded, and between them a rosette of gold, not directly attached to the silver but to a thin copper plate. In Vaphio, a town near Sparta, of a somewhat later period, tombs were found containing many beautiful objects in silver, gold and bronze. Especially noteworthy are two golden cups embossed with figures of bulls and men: in the one case it is a spirited hunt in the woods, in the other a peaceful scene on the meadows. Iron, in Mycenai, appears only as a precious metal of which finger-rings are formed. *κύανος*, which has frequently been translated "steel," was almost certainly a blue mineral, lapis lazuli, or a carbonate of copper.

In the remains of a Greek colony in Cyprus, belonging to the end of the Mycenæan period, which is now being explored by the British Museum, iron plays a much more important part. At Athens also large iron swords, which belonged to the ninth or tenth century B.C., have been found in an old cemetery.

After this came the intellectual period of Grecian history. Aristotle must be mentioned in any account of the science of the day; and he it is who gives us the first description of the metal mercury, and also how to produce the alloy which we call brass, by heating together copper and calamine, the carbonate of zinc. Metallic zinc, however, was not known for many centuries afterwards.

CONCLUSION.

In tracing back the history of these great nations we have found evidence of a time when metals were little, if at all, employed; the potter's art was well known, and early man became wonderfully proficient in working hard stone, and especially flint. The earliest indications we have of metals are of gold and copper, both being scarce, and no doubt costly. Gold was probably the earliest to attract the attention of mankind, because it occurs native, of bright yellow colour, and is easily worked. Copper, however, dates to a similar period, so far as the remains which have come down to us are concerned. Probably the deep blue carbonate, such as occurs in Armenia, was first worked. When silver was first used is not very evident, but it is certain that it was far more common in the northern portion of the area we have been considering, than in the southern. The metallurgy of copper was doubtless a matter of much study and experiment so as to produce the hardest metal. This seems to have led to the knowledge of tin, but at what precise period we know not; nor do we know whether it was brought from Etruria, or found in some nearer region. Mines of tin were certainly worked at Cento Camarelle, as Egyptian scarabs have been found in the old workings,* and near Campiglia and in Elba, as well as in the Iberian peninsula. This search for the metals, and the necessity of carrying the ore or rough metal to the places where it was wrought, or of the finished material to distant customers, must have greatly promoted commerce. This took place both by land and sea, in caravans and ships. In this way tools and other objects were disseminated through the more distant parts of Europe and Asia; the similarity of type over large areas shows a common origin, and hence we can even roughly form an opinion as to whether they were introduced in earlier or later times. Thus, in Switzerland and Scandinavia we meet with copper implements as well as bronze, and ancient as well as modern forms; while in Britain we find no evidence of copper tools, though bronze objects are abundant.

The Phœnicians, arriving on the eastern shore of the Mediterranean from the direction of the Persian Gulf, formed an important nation for about 1000 years, from B.C. 1400 to B.C. 400. They were great artificers, but not having much originality they adopted the patterns and designs of Egypt or Assyria. They were also pre-eminently traders, and founded cities and emporia of commerce, so that their metal work was spread over all the Mediterranean. It is to be found in Cyprus, mixed with the workmanship of the Grecian Mycenæan age. Their ornamental jewellery was eagerly sought in Etruria, Greece and Calabria; for the beauty of it I may refer you to the Etruscan cup of gold in the South Kensington Museum, and

* See 'Early Man in Britain,' by Prof. W. Boyd Dawkins.

the wonderful work in gold in one of the Greek rooms in the British Museum.

Commerce implies a large extension of a medium of exchange. The whole question of money is far too wide a subject for us to deal with now; suffice it to say that Herodotus attributes to the Lydians the introduction of the use of coins. The earliest were of electrum, issued in the form of oval bullets, officially stamped on one side. They date back, perhaps, to B.C. 700; but, according to other authorities, silver money was coined at Ægina more than a century before that time.

The great period which has been under our consideration terminated in each country with an age of disorder and deterioration. The rise of the Roman Empire introduced a new era: it was in one sense an iron age—*ferrum* being synonymous with the sword. We now live in another kind of iron age, but in better and brighter times than those of Hesiod, and we may hope that our great engineering works, our iron roads and iron steam-ships may lead not to the enslaving but to the brotherhood of nations.

[J. H. G.]

WEEKLY EVENING MEETING,

Friday, February 18, 1898.

Basil Woodd Smith, Esq. F.R.A.S. F.S.A. Vice-President, in the Chair.

Professor L. C. Miall, F.R.S.

A Yorkshire Moor.

The Yorkshire moor is high, ill-drained, peaty, and overgrown with heather. Moors of this type abound in Scotland, and creep southward along the hills into Yorkshire and Derbyshire, breaking up into smaller patches as the elevation declines. In the south of England they become rarer, though famous examples occur in Dartmoor and Exmoor. In the north they may cover great stretches of country. It used to be said that a man might walk from Ilkley to Glasgow without ever leaving the heather. That was never quite true, but even to-day it is not far from the truth; a man might walk nearly all the way on unenclosed ground, mostly moorland.

Neither peat nor heather is confined to high ground. Peat often forms at sea level, and may contain the remains of sea-weed. In some places it is actually submerged by change of sea level, and the peasants go at low water and dig through the sand to get it. Heather ranges from sea level to Alpine heights.

Peat may form because there is no fall to carry off the water, or because the soil, though high and sloping, is impermeable to water. A few feet of stiff boulder clay constitute such an impermeable floor, and a great part of our Yorkshire moors rests upon boulder clay, which is attributed to ice action, because it is often packed with ice-scratched pebbles, some of which have travelled far, and because the rock beneath, when bared, exhibits similar scratches.

The rocks beneath the boulder clay of a Yorkshire moor are chiefly sandstones and shales. Where the sandstones crop out, they form tolerably bold escarpments with many fallen blocks, such as we call "edges" in the north; the shales make gentler slopes. Both the surface water and the spring water of the moors are pure and soft; they may be tinged with peat, but they contain hardly any lime, potash or other mineral substance except iron oxides.

The wettest parts of the moor are called *mosses* (in some parts of Scotland they are called *flow-mosses*) because the Sphagnum moss grows there in profusion. The Sphagnum swamps are an important feature of the moor, if only because they form a great part of the peat. Not all the peat, however; some is entirely composed of heather and heath-like plants, while now and then the hair moss (Polytrichum) and certain moorland lichens contribute their share,

but the Sphagnum swamps play the leading part, especially in starting new growths of peat. If we walk carelessly over the moor, we now and then step upon a bed of Sphagnum. We have hardly time to notice its pale green tint and the rosy colour of the new growths before all close observation is arrested by the cold trickle of water into the boots. The practised rambler takes care to keep out of the Sphagnum swamps altogether, knowing that he may easily sink to the knees or further. Sphagnum sucks up water like a sponge, and if you gather a handful, you will be surprised to see how much water can be squeezed out of it. This water abounds in microscopic life; Amœbæ and other Rhizopods, Diatoms, Infusoria, Nematoids, Rotifers and the like can be obtained in abundance by squeezing a little Sphagnum fresh from the moors.* As the stems of Sphagnum grow upwards, they die at the base, and form a brown mass, which at length turns black, and in which the microscope reveals characteristic structural details, years, perhaps centuries, after the tissues ceased to live.

FIG. 1.—Leafy branch of Sphagnum, magnified; one leaf of ditto, further magnified.

An old Sphagnum moss is sometimes a vast spongy accumulation of peat and water, rising higher in the centre than on the sides, and covered over by a thin living crust. The interior may be half-liquid, and when the crust bursts after heavy rain, the contents of a hillside swamp now and then pours forth in an inky flood, deluging whole parishes. In 1697 a bog of forty acres burst at Charleville, near Limerick. In 1745 a bog burst in Lancashire, and speedily covered a space a mile long and half a mile broad. A bog at Crowhill on the

* It is interesting to note that the same abundance of animal life characterises the mosses of Spitzbergen, where not a few of the very same species are found (D. J. Scourfield, "Non-marine Fauna of Spitzbergen," 'Proc. Zool. Soc.' 1897).

moors near Keighley burst in 1824, and coloured the river with a peaty stain as far as to the Humber. In December 1896, a bog of 200 acres burst at Rathmore, near Killarney, and the effects were seen ten miles off. Nine persons perished in one cottage.

The soaking up of water is essential to the growth of the Sphagnum, which employs several different expedients for this purpose. Its slender stems give off numerous leafy branches, and also branches which are reduced to filaments. These last turn downwards along the stem, which they may almost conceal from view. The crowded leaves have in-folded edges. There are thus formed innumerable narrow chinks, in which water may creep upwards. The microscope brings to light further contrivances, which answer the same purpose. Many of the cells of the leaf lose their living substance, and are transformed into water-holding cavities with thin transparent walls, which are prevented from collapsing by spirally wound threads. But the water must not only be lodged; it must ascend, and supply the growing branches above. Accordingly the water-holding cells are not closed, but pierced by many circular pores, which allow liquid to pass in and out freely. Perforated water cells also form the outer layers of the stem. Thus the whole surface of the plant, whether immersed or not, is overspread by a water film, which is easily replenished from below as it evaporates above. It is the water spaces which render the Sphagnum so pale. The green living substance forms only a thin network, traversing the water-holding tissue.

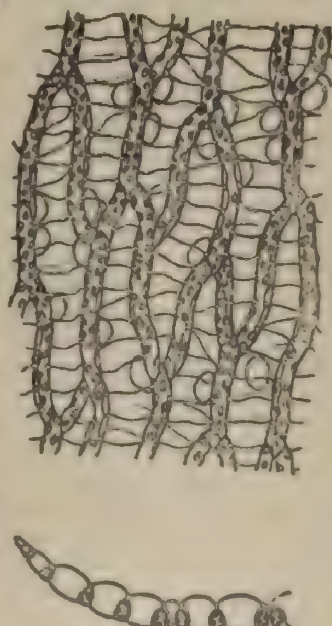

FIG. 2.—Detail of Sphagnum leaf; green cells with corpuscles, and water-cells with spiral threads and pores. Below is a section (from Sachs) of part of a leaf.

Now and then we are lucky enough to see the bed of a Sphagnum swamp. Quarrying, or a landslip, or the formation of a new watercourse, may expose a clean section. I have known the mere removal of big stones, time after time, from the bed of a stream fed by a Sphagnum swamp, gradually increase the cutting power of the running water, until the swamp is not only drained, but cut clean through down to the solid rock. Then we may see that the peat rests upon a sheet of boulder clay, and this upon the sandstones and shales.

Between the peat and the boulder clay there is sometimes found an ancient seat-earth, in which are imbedded the mouldering stumps of long dead trees. Oak, Scotch fir, birch, larch, hazel, alder, willow, yew and mountain ash have been met with.* Where a great tract of peaty moorland slowly wastes away, the tree stumps may be found scattered thick over the whole surface. Above the seat-earth and its stumps, if these occur at all, comes the peat, say from five to twenty feet deep, and above the peat the thin crust of living heather.

Every part of the moor has not, however, the same kind of floor. Streams in flood may excavate deep channels, and wash out the gravel and sand into deltas, which often occupy many acres or even several square miles. The outcrops of the sandstones crumble into masses of fallen blocks. Instead of the usual impervious bed of boulder-clay, we may get a light subsoil. The verges of the moor have commonly this character; they are by comparison dry, well drained, and overgrown with furze, bilberry, crowberry, fern, and wiry grasses; such tracts are called "roughs" or "rakes" in the north of England. A similar vegetation may be found far within the moor, though not in places exposed to the full force of the wind. Even on the verges of the moor there are very few earthworms, and at most a scanty covering of fine mould; in the heart of the moor there is no trace of either. The Nematoid worms which are so common in most soils, and easily brought to the surface by pouring a few drops of milk upon the ground, seem to be absent from the humus. Insects and insect larvæ are very seldom found in it.

In a country where population and industry grow steadily, it is rare to find the moor gaining upon the grass and woodland. We have to go back some centuries to find an example on anything like a large scale. The Earl of Cromarty (Phil. Trans. No. 330, p. 296), writing in 1710, says that in 1651 he saw a "firm standing wood" of dead fir trees on a hill-side in West Ross-shire. About fifteen years later he passed the same spot, and found no trees, but a "plain green moss" in their place. He was told that the trees had been overturned by the wind, and afterwards covered by the moss, and further that none could pass over it because it would not support a man's weight. The Earl "must needs try it," and fell in up to the arm-pits.

A section through a thick bed of peat will sometimes reveal the manner of its growth. The lower part is often compact, the upper layers of looser texture. It is not uncommon to find by microscopic examination that while the lower part is made up entirely of Sphagnum, the more recent growth is due to heather, crowberry, grasses, hair moss and lichens. In some places the whole thickness is of Sphagnum only; in others there is no Sphagnum at all. Peat formed of Sphagnum only has no firm crust, and from the circumstances of

* In Yorkshire I think that birch and alder are the commonest of the buried trees.

its growth it is likely to be particularly wet. Sphagnum often spreads over the surface of pools or even small lakes, not nearly so often in Yorkshire, however, as in a country of well glaciated crystalline rocks, where lakes abound. In such cases a peculiar kind of peat is formed as a sediment at the bottom of the water, which may in the end fill up the hollow altogether. A very slight cause is enough to start a Sphagnum bog, such as a tree falling across a stream, or a beaver dam. When a pool forms above the dam, the Sphagnum spreads into it, and the peat begins to grow. Long afterwards, when the hollow is completely filled with peat, there may be a chance for grasses, rushes, crowberry and heather.

FIG. 3.—Ling (*Calluna vulgaris*). A leafy branch; a single leaf, seen from beneath; and a cross section of the base of the leaf.

In our own time and country the moors waste faster than they form; it is much commoner to find the grass gaining on the heather than to find the heather gaining on the grass. There is no feature of the Yorkshire hills more desolate than ground covered with wasting peat. The surface is cut up by innumerable channels, with peaty mounds between. These are either absolutely bare, or thinly covered with brown grasses and sedges. The dark pools which lie here and there on the flats are overhung by wasting edges of black peat. It is cheerful to step from this dismal territory to ground clothed with close-growing grasses of a lively green, such as we find where the peat has disappeared altogether.

The moors are commonly wet, very wet in places. In certain parts and during certain seasons of the year they are, however, particularly dry, and subject to a severity of drought which the lower slopes and the floor of the valley know nothing of. At lower levels trees give shelter from sun and wind; night-mists check evaporation, and even return a little moisture to the earth; the deep, finely divided soil lodges water, which is given off little by little, and in our climate never fails to yield an effective supply to the roots; pools and streams dole out sparingly the water which fell long before as rain. But the moor lies fully open to sun and wind. In March it is exposed to the east wind; in June to hot sun and cold, clear nights; in August there is perhaps a long spell of drought; in November heavy gales with abundance of rain. The summer is late; the moorland grasses make little growth before the beginning of June; even

then the heather bears few young leaves, while the fronds of the bracken are only beginning to push through the soil. Whatever the weather, there is no protection against its extremes; there is no shelter and no shade. The air is cold; wind and the diminished pressure due to height favour rapid evaporation. Though the Sphagnum patches form permanent bogs, a great part of the moor becomes far drier in a hot summer than any pasture or meadow. The top of the peat crumbles, and is blown about as dust, the loose sand can hold no moisture, bared surfaces of clay become hard as iron. Another feature which must profoundly affect the vegetation of the moor is the poverty of its water in dissolved salts. It is pure and soft, like dis-

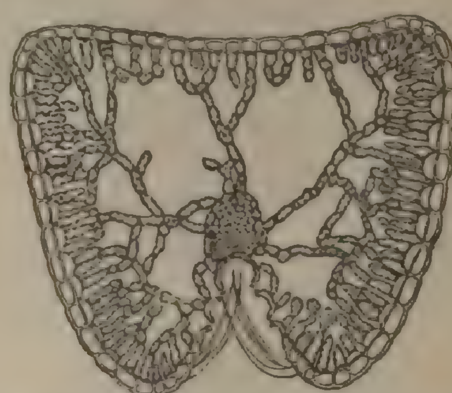

FIG. 4.—Transverse section of leaf of Ling, showing large air-spaces, the reduced lower epidermis which bears the stomates, and the long hairs which help to close the cavity into which the stomates open.

tilled water, and contains hardly any mineral food for plants. The plants of the moor are subject to the extremes of wet and dry, to cold and to famine.

The best known and most characteristic of the moorland plants are the heaths. Ling, the common heather, is the most abundant of all; it sometimes covers many square miles together, to the almost complete exclusion of other plants. Ling is a low shrub, whose wiry stems creep and writhe on the surface of the ground. When sunk in deep peat the stems are often pretty straight, but among rocks you may follow the twisted branches for many yards, and at last discover that what you took for small plants rooted near the surface are really the tops of slender trees, whose roots lie far below. Bilberry, too, wriggles among loose stones or fallen blocks till you grow weary of following it. The leaves of ling are dry, hard and evergreen. They last for two or three years, and do not fall off as soon as they die

but crumble slowly away. They are very small, densely crowded, and ranged on the branch in four regular rows. A good thin section through a leaf is not easy to cut; when you get one, you find that the interior is largely occupied by irregular air spaces, and that the stomates are sunk in a deep groove on the under side of the leaf, where they are further sheltered by hairs.

FIG. 5.—Cross-leaved Heath (*Erica tetralix*); with part of a branch, enlarged; a leaf seen from the under side; and a section of a leaf.

Ling is a plant of slow growth, and a stem which showed seventeen annual rings was only a centimetre in diameter. Stems of greater age than this are rare. After ten or twelve years the plants flower scantily, and exhibit other signs of age. Then the common practice is to burn them off.

As we travel south, we find the ling getting smaller and smaller. In Scotland it is often waist-deep, in Yorkshire knee-deep, on Dartmoor only ankle-deep. On the moors of the south of England the ling is generally much mixed up with grasses, as also on the verges of the Yorkshire moors. In Cornwall it may grow so close to sea level that it is wet with salt spray in every storm, and its tufts

are intermingled with sea-pink and sea-plantain. At the Lizard, wherever the serpentine comes to the surface, ling ceases, and the Cornish heath (*Erica vagans*) takes its place.

Here and there we find among the ling the large-flowered heaths with nodding pink or purple bells (Scotch heath, cross-leaved heath). The leaves of these plants are much larger and thinner than those of ling; they are called "rolled leaves," because the edges curve downwards and inwards, partly concealing the under surface, which bears the stomates. All our native heaths agree in possessing wiry stems, long roots and narrow evergreen leaves, with a glossy cuticle and small transpiring surfaces. The tissues are very dry, and burn readily even when green or drenched with rain. It is possible by good management to set acres of heather in a blaze, even in midwinter, with a single lucifer match. The heaths wither very slowly when gathered, and change little in withering.

Some of these features are characteristic of desert plants. Many desert plants have reduced transpiring surfaces and hidden stomates.

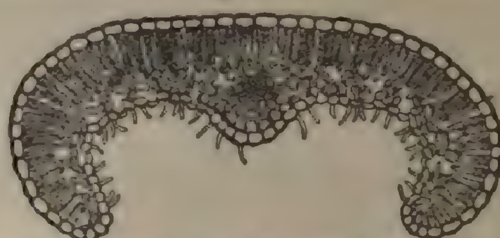

FIG. 6.—Transverse section of rolled leaf of cross-leaved Heath (*Erica tetralix*).

They often have very long roots, as was particularly observed in the excavations for the Suez Canal.* The leaves are often small and crowded, the stems woody, much branched and tufted. Bright sunlight retards growth, and green tissues hardly ever present a large absorbing surface when they are habitually exposed to bright light. Accordingly the young shoots and branches do not push out freely, but try to hide one behind another. The tissues of desert plants may be remarkably dry; they are often, however, remarkably succulent; the plant either learns to do without water for a long time together, or to store it up.

It is not without surprise that we learn how similar are the effects of tropical drought and of Arctic cold. The facts of distribution would in themselves suffice to show that our moorland heaths are well fitted to endure great cold. Ling extends far within the Arctic circle, though it seldom covers large surfaces there, and it rises to

* Examples are quoted by Warming. 'Lehrb. d. ökol. Pflanzengeographie,' p. 198.

2000 metres (6600 feet) on the north side of the Alps. It extends southward to the shores of the Mediterranean. Our large-flowered heaths have not been traced quite so far north as ling, and they are not found on the Alps, though they inhabit the Pyrenees. Many representatives of the heath family, with like structure of leaves, are found in the extreme north of the American continent. Those features which assimilate our heaths to desert plants, and which seem to be obvious adaptations to a situation of extreme drought, are equally serviceable to plants which have to face boisterous winds and low temperature. The shrubs of the far north are low, tufted, small-leaved, evergreen and dry—just like the heaths of our moors. Middendorff * shows how the Dahurian larch becomes stunted in proportion to increasing cold. Before it disappears altogether, it is cut down to a prostrate creeping shrub. One such dwarf larch, though 150 years old, was only a foot or two across. Plants much exposed to biting winds must make the most of any shelter that can be had; their branches push out timidly, and for a very short distance; the leaf surface is reduced to a minimum; since the warm season is short, evergreen leaves are profitable, for they enable the plants to take advantage of early and late sunshine.

The heaths and many other moorland plants bear the marks of the *Xerophytes*, or drought plants. Xerophytes grow under a considerable variety of conditions, some of which do not suggest drought at first sight, but their tissues are always ill-supplied with water. It may be that water is hardly to be had at all, as in the desert; or that water must not be imbibed in any quantity because of low temperature, as in Arctic and Alpine climates; or that the water is mixed with useless and perhaps injurious salts, from which it can only be separated with great difficulty, as in a salt marsh. Whatever may be the reason for abstinence, xerophytes absorb water slowly, part with it slowly, and endure drought well.

In the case of moorland plants there is an obvious reason why many of them, though not quite all (Sphagnum is one exception) should rather thirst and grow slowly than pass large quantities of water through their tissues. The water contains hardly any potash or lime, and very little that can aid the growth of a plant. But it is probable that this is not the sole reason. Except where special defences are provided, it is dangerous for a plant which may be exposed to wind or low temperature to absorb much water.

The Bilberry (or Blueberry, as we ought to call it) is one of the few exceptions to the rule that moorland plants are evergreen; it casts its leaves in early winter. But the younger stems are green, and take upon themselves the function of leaves when these are absent. Kerner has described one adaptation of the bilberry to seasons when water is scarce. Many plants, especially those of hot and wet climates, throw off the rain water from their tips, and so

* 'Sibirische Reise,' vol. iv. p. 605.

keep the roots comparatively dry; others direct the water down the branches and stem to the roots. Bilberry is one of the latter sort. The rounded leaves slope downwards towards the leaf stalk, and from the base of every leaf stalk starts a pair of grooves, which are sunk in the surface of the stem. A light summer shower is economised by the guiding of the drops towards the roots. Bilberry abounds on the loose and sandy tracts of the moor, and especially on its verges; it is seldom found upon a deep bed of peat.

FIG. 7.—Crowberry (*Empetrum nigrum*). A staminate branch, slightly enlarged; *a*, part of a pistillate branch; *b*, one staminate flower; *c*, one pistillate flower.

There is a moorland plant which may be said to mimic the heaths, as a Euphorbia mimics a Cactus, or Sarracenia a Nepenthes. Similarity of habit has brought about similarity of structure. The plant I mean is the Crowberry, which is so like a true heath in its foliage and manner of growth, that even the botanists, who did not fail to remark that the flowers are altogether different, long tried to bring the crowberry and the heaths as near together in their systems as they could. Crowberry has the long, dry, wiry stems, the small, narrow, rolled, clustered, evergreen leaves of a true heath. The leaf margins are turned back till they almost meet, and the narrow cleft between them is obstructed by close-set hairs, so that the transpiring surface is effectually sheltered. Crowberry is a peat-loving shrub, and is often found with ling and other heaths in the heart of the moor. The berries are a favourite food of birds, which help to disseminate the species. Crowberry has an uncommonly wide distribution, not only in the Arctic and Alpine

regions of the Old World, but also in the New. It abounds in Greenland, where the Eskimo use the berries as food, and extract a spirit from them. A very similar species, with red berries, occurs in the Andes.

The heaths, bilberry, crowberry, and many other peat-loving shrubs or trees, have a peculiar root structure. The usual root hairs are wanting, and in their place we find a peculiar fungus-growth, which invades the living tissues of the root, sometimes penetrating the cells. There is often a dense mycelial mantle of interwoven filaments, which covers all the finer roots. This looks like parasitism,

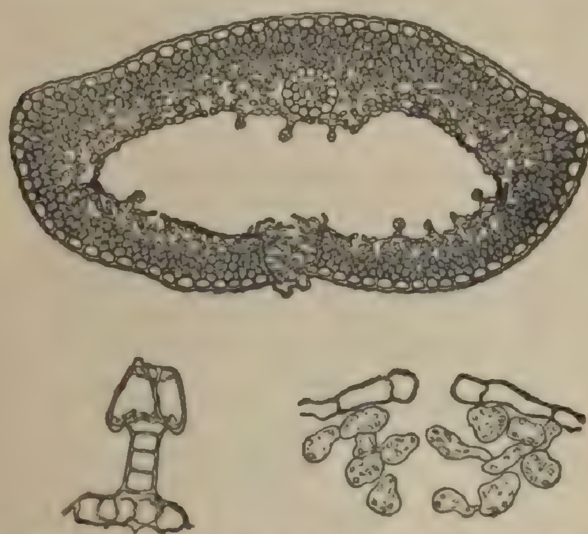

FIG. 8.—Cross section of leaf of Crowberry. The lower figures show one of the peculiar hairs and one of the stomates. Both are confined to the inner, which is properly the under surface.

but the fungus is apparently not a mere parasite, for the tree or shrub shows no sign of injury, but thrives all the better when the fungus is plentiful, and may refuse to grow at all if the fungus is removed. Rhododendron, ling, most heaths, bilberry, crowberry, broom, spurge-laurel, beech and birch are among the plants which have a mycelial mantle. If the native soil which clings to the roots of any of these is completely removed, if the fine roots with the mycelial mantle are torn off by careless transplanting, or if peaty matter is withheld, the plant dies, or struggles on with great difficulty until the mycelial mantle is renewed. Such plants cannot, as a rule, be propagated by cuttings, unless special precautions are taken. Frank

maintains that the mycelial mantle is the chief means of absorption from the peaty soil, and that the tree or shrub has come to depend upon it. The known facts render this interpretation probable, but thorough investigation is still required. In some cases at least the plant can be gradually inured to the absence of a mycelial mantle. I have repeatedly planted crowberry in a soil devoid of peat. It generally succumbs, but when it survives the first year, it maintains itself and slowly spreads. Microscopic examination shows that the roots of crowberry grown without peat contain no mycelial filaments or very few. The special function of the fungus may be to reduce the peat to a form capable of absorption as food by green plants. It is likely that the fungus gains protection or some other distinct advantage from the partnership. Most of the species of green plants which have the mycelial mantle are social. It is obvious that the fungus will be more easily propagated from plant to plant, where many trees or shrubs of the same species grow together.

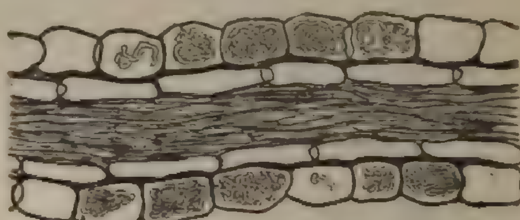

FIG. 9.—Longitudinal section of root of Ling (*Calluna vulgaris*), showing mycorhizal filaments in outer cells.

The grasses of the moor are marked xerophytes with wiry leaves, whose look and feel tell us that they have adapted themselves to drought and cold by reducing the exposed surface to a minimum. A section of the leaf of Nardus, Aira flexuosa or Festuca ovina shows that the upper surface, which in grasses bears the stomates, is in-folded, and sometimes greatly reduced. Advantage has been taken by these grasses of a structure which was apparently in the first instance a provision for close folding in the bud. The upper stomate-bearing surface is marked by furrows with intervening ridges, and where the folding is particularly complete, both furrows and ridges are triangular in section, and the leaf, when folded up longitudinally, becomes an almost solid cylinder. In the grasses of low, damp meadows, the power of rolling up may soon be lost by the leaves. Other grasses, which are more liable to suffer from drought, retain in all stages the power of rolling up their leaves. Sesleria cærulea, for instance, which covers large tracts of the limestone hills of Yorkshire, can change in a few minutes from closed to open, or from open to closed, according to the state of the air. The leaves of the true

moorland grasses (Nardus, Aira flexuosa, Festuca ovina) are permanently in-rolled, and flatten out very slowly and imperfectly, even when immersed in water for many hours.

Our moorland grasses are all arctic, and occur both in the old and the new worlds; Festuca ovina is also a grass of the steppes; it is world-wide, being found in all continents, especially on mountains, and even reaching Australia and New Zealand.

It may seem paradoxical to count the Rushes as plants which are protected against drought, for they often grow in the wettest part of the moor. They are common, however, in dry and stony places, and

FIG. 10.—Transverse section of leaf of *Nardus stricta*, showing permanent in-rolling.

their structure is completely xerophytic. The leaves are often reduced to small sheaths, which wither early, while the stems are green, and perform the work of assimilation; or else, as happens in certain species, the leaves assume the ordinary structure of the stem. The cylindrical form of the rush stem is significant, for of all elongate solid figures the cylinder exposes the smallest surface in proportion to its volume. Moreover a cylindrical stem, without offstanding leaves, and alike on all sides, is well suited, as Jungner points out, to the circumpolar light, which shines at low angles from every quarter in succession. A rush stem is singularly dry, the

centre being occupied by an abundant pith of star-shaped cells, which entangle much air.

The Hair moss (Polytrichum commune) of the moor has a defence against sun and wind, which has been described by Kerner. The leaf has wings, like an altar piece, which can open and shut. The assimilating surface occupies the centre, and rises into many green columns. In wet or cloudy weather the wings open wide, but when the sun shines they fold over the columns, and protect them from scorching.

All the most characteristic plants of the moors are Arctic. Ling, bilberry, crowberry, certain rushes, Nardus, Festuca ovina, most of our club mosses, the hair moss and Sphagnum range within the

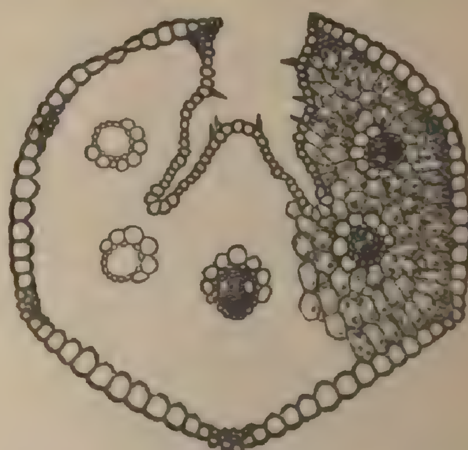

FIG. 11.—Transverse section of leaf of *Aira flexuosa.*

Arctic circle; while the large-flowered heaths get close up to it. Most of them are found on both sides of the Atlantic, and some, like the crowberry and Festuca ovina, have a singularly wide distribution.

It has often been pointed out that great elevation above sea level produces a similar effect upon the flora to that of high latitude. In the Alps, the Pyrenees, the Himalayas, and even in the Andes, the forms characteristic of northern lands reappear, or are represented by allied species. Where, as in the case of the Andes, nearly all the species differ, it is hard to draw useful conclusions, but whenever the very same species occur across a wide interval the case is instructive. In the Alps we find our moorland and Arctic flora almost complete, though Rubus Chamæmorus, Erica Tetralix, and E. cinerea (both

found in the Pyrenees), Narthecium ossifragum and Aira flexuosa have disappeared.

A favourite explanation rests upon the changes of climate to which the glaciation of the northern hemisphere bears emphatic witness. When the plains of Northern Europe were being strewn with travelled boulders, when Norway, Scotland and Canada were covered with moving ice, the vegetation of Siberia and Greenland may well have extended as far south as Switzerland.

I do not doubt the general truth of what we are taught respecting the glacial period, but I think that we are apt to explain too much by its help. We know very little for certain as to its effect upon vegetation. Our information concerning the preglacial flora is extremely meagre, nor are we in a position to say positively what sort of flora covered the plains of Europe after the severity of glacial cold had passed away, and before men had changed the face of the land by tillage.* We know rather more about the animals of these ages, for animals leave more recognisable remains than plants, but the indications of date, even in the case of animals, are apt to be slight and uncertain. On the whole, I doubt whether the glacial period marks any great and lasting change in the life of the northern hemisphere.† I think it probable that since the glacial period passed away, the countries of Central Europe possessed many species, both of plants and animals, which we should now consider to be Arctic, and that these Arctic species endured until many of them were driven out by an agent of which geologists usually take little notice. I shall come back to this point.

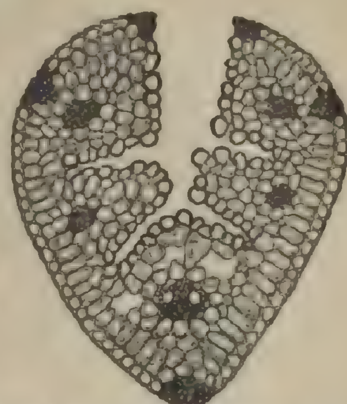

FIG. 12.—Transverse section of leaf of *Festuca ovina*. In thick sections hairs are seen to point inwards from the inner epidermis.

The animal life of the Yorkshire moors is not abundant. Hares, shrews, stoats, weasels and other small quadrupeds, which are plentiful on the rough pastures, cease where the heather begins. There are a good many birds, some of which, like the grouse, the

* Some information has been gained by investigation of plant remains found beneath the bogs of Denmark, and beneath the palæolithic brick-earth at Hoxne.

† It is well known that this position has been strongly maintained by Professor Boyd Dawkins ("Early Man in Britain," p. 123, &c. 'Q. J. Geol. Soc.' vol. xxxv. p. 727, and vol. xxxvi. p. 399). On the other side, Dr. James Geikie may be consulted ('Prehistoric Europe,' ch. iii. &c.).

ring-ouzel, the twite, or mountain-linnet, the curlew, and the golden plover, seek all their food on the moor, except in the depth of winter, when some of them may visit the sea-coast, or the cultivated fields, or even southern countries. The kestrel, blackbird, whinchat, stonechat, night-jar and lapwing abound on the "roughs" or border-pastures rather than on the moor itself. Owing to the absence of tarns and lochs there are practically no water-fowl. Gulls are hardly ever seen, though they are common enough on the Northumberland moors. Now that the peregrine, golden eagle and hen-harrier are

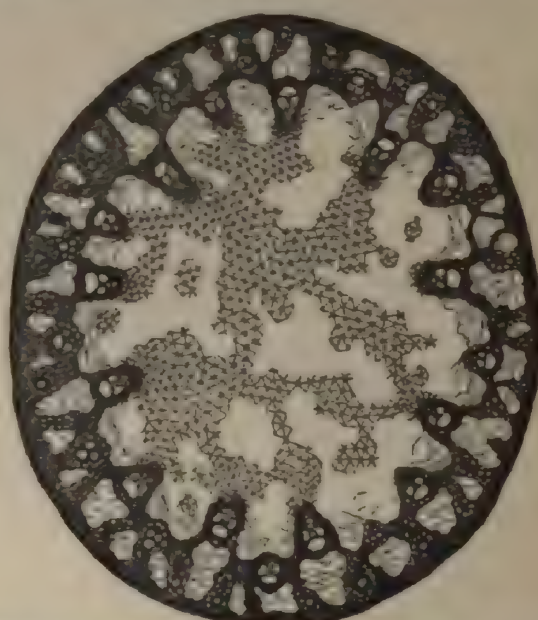

FIG 13.—Transverse section of stem of Rush (*Juncus conglomeratus*), showing the stellate pith cells, and very numerous air spaces.

exterminated, the chief moorland birds of prey are the merlin, kestrel, and sparrow-hawk. Of these, only the merlin is met with in the wilder parts of the moor, where it flies down the smaller birds. The kestrel hovers over the roughs, on the look-out for a mouse or a frog. The sparrow-hawk preys upon small birds, but rarely enters the heart of the moor.

To most people the interest of the moor centres in the grouse. There are many things about grouse which provoke discussion, such as its feeding times, or the grouse-fly, and what becomes of it during

the months when the grouse are free of it. But the absorbing topic on which every dweller by the moor is expected to have an opinion, is the grouse disease.

All sorts of causes have been assigned, such as over-stocking of the moors, destruction of the large hawks which used to kill off ailing birds, parasitic worms, cold, deficiency of food, and so on. Some Yorkshire sportsmen have attributed the disease to the scarcity of gritty sand. On shale-moors, they maintain, the gizzard of the grouse is filled with soft stones, which will not grind up the heather-tops effectively, except when they are young and tender. On sandstone moors the grouse can deal with tougher food, and there the disease, it is said, is unknown. Dr. Klein's researches * show that the disease is really due to the multiplication within the body of a specific germ, which is fungal, but not bacterial. The infection is conveyed, or may be conveyed, by the air.

The viper is rare, and until quite lately I had never heard of its presence on our Yorkshire moors. Lizards are also rare, but efts are not uncommon. Among the moorland moths are many small Tineina (allied to the clothes moth). The caterpillar of the emperor moth is characteristic, and seems to be protectively coloured, for it wears the livery of the heather—green and pink. The moths which issue from these larvæ are captured in great numbers by Sunday ramblers, who resort to the base contrivance of bringing a female moth in a cage. The self-styled "naturalist" sits on a rock, and captures one by one the eager moths which come about him, afterwards pinning out the expanded wings to form grotesque patterns, or selling his specimens to the dealers. Certain wide-spread Diptera are plentiful, and there are a few which pass their larval stages in the quick-running streams which flow down from the moor. The small number of good-sized insects partly explains (or is explained by) the paucity of conspicuous, scented or honey-bearing flowers. In this the moor contrasts strongly with the higher Alps. Bees, however, get much honey from the large-flowered heaths and ling; heather-honey is considered better than any other. A little scale insect (*Orthesia uva*) has been found plentifully on the Sphagnum of the moors, particularly in Cumberland.† A big spider (*Epeira diadema*) spreads its snare among the heather, and may now and then be seen to deal in a particularly artful fashion with a wasp or other large insect which may have blundered into the web. The spider cuts the threads away till the struggling insect dangles; cautiously on outstretched leg holds out and attaches a new thread, and then sets the wasp spinning. The silken thread, paid out from the spinneret, soon binds the victim into a helpless mummy.‡ I have never found gossamer so abundant as on the verges of the moor.

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* 'The Etiology and Pathology of Grouse Disease, &c.' (1892).

† Shaw (1806) quoted by R. Blanchard in 'Ann. Soc. Ent. Fr.' tom. lxv, p. 681 (1896).

‡ Blackwall's 'Spiders,' vol. ii. p. 359.

In our day the Yorkshire moor harbours no quadrupeds, and the grassy hills none but small quadrupeds. It was not always so. At Raygill, a few miles from us across the moors, a collection of bones was discovered a few years ago in quarrying. A deep fissure in the rock had been choked ages before with stones and clay. This fissure was cut across by the working face of the quarry. Many bones were brought out of it, bones of the ox and roebuck among the rest. But mixed up with these were teeth and bones of quadrupeds now altogether extinct or no longer found in Britain, such as the straight-tusked elephant (*E. antiquus*), the hippopotamus, a southern rhinoceros (*R. leptorhinus*), the cave hyæna, and the European bison. The Irish elk is often dug up in Yorkshire, the reindeer and the true elk now and then. Not very long ago these and other large quadrupeds grazed or hunted a country which can now show no quadruped bigger than a fox.

It is evident that the moors, valleys and plains of Yorkshire have been depopulated in comparatively recent times. The disappearance of so many conspicuous species is commonly attributed to the glacial period, but I think that the action of man has been still more influential. The extinct animals are such as man hunts for profit or for his own safety. Many of them, among others the cave bear, Machairodus, Irish elk, mammoth, and straight-tusked elephant, are known to have lasted into the human period. That so many of them were last seen in the company of man is some proof that he was concerned in their death.

Central Europe, before man appeared within its borders, or while men were still few, little resembled the Europe which we know. Much of it was covered with woods, morasses or wastes, and inhabited by animals and plants, of which some ranged into the Arctic circle; others to the Mediterranean, Africa and India. The worst lands of all—cold, wet, and wind-swept—had doubtless then, as now, the greatest proportion of Arctic species. But it is likely that the passage from the bleak hills to the more fertile valleys and plains was not then so abrupt as at present. All was alike undrained and unenclosed; and what we know of the distribution of life in Pleistocene Europe shows us that a large proportion of our European animals and plants are not restricted by nature within narrow limits of latitude or climate. Species which are now isolated, at least in Central Europe, occupying moors or other special tracts, and surrounded by a population with which they have little in common, were formerly continuous over vast areas. In the early days of man in Europe many plants, birds and quadrupeds which are now almost exclusively Arctic may well have ranged over nearly the whole of Europe.

As men gradually rooted themselves in what are now the most populous countries of the world, the fauna and flora underwent sweeping changes. The forests were cleared, and trees of imported species planted here and there. The land was drained, and fenced,

and tilled. During the long attack of man upon wild nature many quadrupeds, a few birds, some insects and some plants are known to have perished altogether. Others have probably disappeared without notice Certain large and formidable quadrupeds, though they still survive, are no longer found in Europe, but only in the deserts of the south or the unpeopled northern wastes. Thus the lion, which within the historic period ranged over Greece and Syria, and the grizzly bear, which was once an inhabitant of Yorkshire, have disappeared from every part of Europe. Tillage and fencing have checked the seasonal migrations of the reindeer and the lemming. Useful animals have been imported, chiefly from the south or from Asia. Useful plants have been introduced from ancient centres of civilisation, and common farm weeds have managed to come in along with them. Many species of both kinds are southern, many eastern, none are Arctic. In our day the cultivated lands of Europe are largely occupied by southern or eastern forms, and the wastes appear by contrast with the imported population more Arctic than they really are. Even the wastes are shrinking visibly. The fens are nearly gone, and we shall soon have only a few scattered moors left to show what sort of vegetation covered a great part of Europe in the days of choked rivers and unfenced land. The moors themselves cannot resist the determined attack of civilised man. Thousands of acres which used to grow heather are now pastures or meadows.

What we call the Arctic fauna and flora of to-day is apparently only the remnant of an assemblage of species varying in hardiness, which once extended from the Arctic circle almost to the Mediterranean. If climate and soil alone entered into the question, it is likely that the so-called Arctic fauna and flora might still maintain itself in many parts of Central Europe. This Arctic (or ancient European) flora includes many plants which are capable of withstanding extreme physical conditions. Some thrive both on peat and on sand, in bogs and on loose gravel. They may range from sea level to a height of several thousand feet. They can endure a summer glare which blisters the skin, and also the sharpest cold known upon this planet. Some can subsist on soil which contains no ordinary ingredient of plant food in appreciable quantity. Such plants survive in particular places, even in Britain, less because of peculiarly appropriate surroundings, or of anything which the microscope reveals, than because they can live where other plants perish. Ling, crowberry and the rest are like the Eskimo, who dwell in the far north, not because they choose cold and hunger and gloom, but because there only can they escape the competition of more gifted races. The last defences of the old flora are now being broken down; it is slowly giving way to the social grasses, the weeds of commerce, and the broad-leaved herbs of the meadow, pasture and hedge-row. The scale has been turned, as I think, not so much by climatic or geographical changes, as by the acts of man.

Every lover of the moors would be glad to know that they bid

fair to be handed down to our children and our children's children without diminution or impoverishment. The reclaiming of the moors is now checked, though not arrested, and some large tracts are reserved as open spaces. But the impoverishment of the moors goes on apace. The gamekeeper's gun destroys much. Enemies yet more deadly are the collectors who call themselves naturalists, and the dealers who serve them. A botanical exchange club has lately exterminated the yellow Gagea, which used to grow within a mile of my house. Whenever a kingfisher shows itself, young men come from the towns eager to slay it in the name of science. No knowledge worth having is brought to us by such naturalists as these; their collecting means mere destruction, or at most the compilation of some dismal list. If the selfish love of possessing takes hold of any man, let him gratify it by collecting postage-stamps, and not make hay of our plants and mummies of our animals. The naturalist should aspire to study live nature, and should make it his boast that he leaves as much behind him as he found.

[L. C. M.]

WEEKLY EVENING MEETING,

Friday, March 4, 1898.

SIR WILLIAM CROOKES, F.R.S. Vice-President, in the Chair.

PROFESSOR T. E. THORPE, LL.D. F.R.S. *M.R.I.*

Some Recent Results of Physico-Chemical Inquiry.

THE lecturer gave an account of the main results of an investigation on the relations between the viscosity (internal friction) of liquids and their chemical nature which had occupied the late Mr. J. W. Rodger and himself during several years. He pointed out, in the first place, that the many attempts which had been made since Hermann Kopp directed attention to the connection which exists between the molecular weights of substances and their densities, to establish similar relationships between the magnitudes of other physical constants and chemical composition, had rendered it highly probable that all physical constants are to be regarded as functions of the chemical nature of molecules, and that the variations in their magnitude observed in passing from substance to substance are to be attributed to changes in chemical composition. As yet, however, all endeavours to connect the chemical nature of liquids with their viscosity have been only partially successful, although it is obvious from the work of Graham, Rellstab, Pribram and Handl, and Gartenmeister, that such a connection ought to be discoverable.

Thus it was known that an increment of CH_2 in a homologous series is in general accompanied by an increase in viscosity, and that the increase is greater when the increment of CH_2 takes place in an alcohol radicle than when it takes place in an acid radicle. Metameric bodies have, in general, different viscosity values, and these are nearer together the nearer the boiling points of the liquids. Substances containing double-linked carbon are more viscous than those of equal molecular weight containing single-linked carbon. The substitution in a molecule of Cl, Br, I and NO_2 for H in all cases increases the viscosity of the substance. This increase is smallest on the introduction of Cl, and increases on the introduction of Br, I, and NO_2 and in the order given. The absolute amount of the increase depends not only upon the nature of the substituting radicle but also upon its position in the molecule. Of two isomeric esters that possesses the greater viscosity which contains the higher alcoholic radicle. The ester containing the normal radicle has always a greater viscosity than the iso-compound, and this obtains no matter

whether the isomerism is in the alcohol or the acid radicle. The normal aldehydes have invariably a greater viscosity than the iso-compounds, whilst the alcohols have a greater viscosity than the corresponding aldehydes and ketones. The introduction of the hydroxyl group into the molecule greatly increases the viscosity of the liquid. This is strikingly illustrated by the instances of propyl alcohol, propylene glycol and glycerin. Indeed the high viscosity of solutions of carbohydrates, e.g. the sugars, gums, &c., is probably dependent on the relatively numerous hydroxyl groups in the molecule. The manner in which the hydroxyl group is combined seems, however, to have considerable influence on the viscosity. Thus in the cases of the isomeric substances, benzyl alcohol and metacresol, it is found that in the first-named substance, in which the hydroxyl group occurs in the side chain, the viscosity is very much less than that of the second, in which the hydroxyl group is attached to a carbon atom in the benzene ring.

Whilst the broad fact of a connection between the viscosity of a liquid and the chemical nature of its molecules is established, it cannot be said that the numerical results hitherto obtained afford any accurate means of determining the quantitative character of this connection. This is owing partly to the imperfection of observational methods, and partly to the uncertainty of the basis of comparison. It seems futile to expect that any definite stoichiometric relations should become evident by comparing observations taken at one and the same temperature. Hitherto few attempts have been made to ascertain the influence of temperature upon viscosity, and hence the law of the variation is unknown. It seemed therefore, obvious, that in order to investigate the subject with reasonable hope of discovering stoichiometric relations, one essential point was to ascertain more precisely the influence of temperature on viscosity, and then to compare the results under conditions which have been found to be suitable in similar investigations in chemical physics. Unfortunately, the accurate determination of absolute coefficients of viscosity is beset with difficulties, both in the theory and practice of the methods which can be employed. Moreover, it is quite possible that even if accurate values of the coefficients of viscosity were obtained, their relationships to chemical composition might not be simple. Viscosity is, no doubt, the nett result of at least two distinct causes. When a liquid flows, during the actual collision or contact of its molecules a true friction-like force is called into play which opposes the movement, whilst at the same time molecular attractions exercise a resistance to the forces which tend to move one molecule past another.

After indicating the meaning of viscosity and the principles involved in measuring it, the lecturer proceeded to point out how the coefficient of viscosity may be defined. It is the force which is necessary to maintain the movement of a layer of unit area past another of the same area with a velocity numerically equal to the distance between the layers when the space between them is oc-

tinuously filled with the viscous substance. He then described the different modes of measuring viscosity, and explained the general principle of the method and the features of the particular apparatus employed in the investigation made by Mr. Rodger and himself. The principle was that of Poiseuille, and consisted in observing the time required for a definite volume of liquid under a definite pressure to pass through a capillary tube of known size, the temperature being known and kept constant during the interval. The actual apparatus, however, differed in many important features from any previously designed for the same purpose, and admitted of the determination, in absolute measure, of the coefficient for a temperature range from 0° up to the ordinary boiling point of the liquid. In most of the instruments used by previous observers, the liquid, after passing through the capillary, was allowed to escape, and hence the apparatus had to be recharged before another observation could be made. In the newer form, the time spent in recharging was saved, by arranging that in all the observations on any one liquid the same sample could be used repeatedly; and further economy in time was obtained by arranging that observations could be taken while the liquid was flowing in either direction through the capillary tube, and that while an observation was in progress and liquid was leaving one portion of the instrument, it was entering another portion and getting into position for a fresh observation. It was also desirable to avoid the use of corks or caoutchouc in such parts as would be in contact with the liquid, and it was therefore necessary that the instrument should be made entirely of glass.

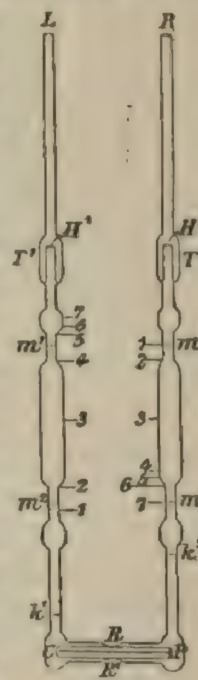

Fig. 1.

The form of apparatus designed to meet these requirements is shown in Fig. 1; it may be termed a glischrometer. It consists of two upright limbs L and R (left and right), connected near their lower ends by a cross piece. Within the cross piece is the capillary tube O P, the bore of which is about ·008 centimetres radius, and the thickness of the wall about 2 millimetres, the internal radius of the cross piece being a millimetre or so greater than the external radius of the capillary. At the zone R, R¹, the walls of the cross piece are constricted and made continuous with those of the capillary: the latter is thus gripped at its middle portion and held axially within the cross piece. Care is of course taken that the bore of the capillary is in no wise disturbed during the process of sealing.

On one side of each limb of the instrument three fine horizontal

lines were etched, m^1, m^2, k^1, on the left limb; m^3, m^4, k^2, on the right limb. The volumes of the limbs between m^1 and m^2 and between m^3 and m^4 were carefully determined; these represent the volumes of liquid which flow through the capillary. The time taken by the level of the liquid to pass from the upper to the lower of either of these pairs of marks is the time observed in the experiments. The limb is constricted in the vicinity of the marks, in order to give sharpness in noting the coincidence of the meniscus with the mark. The shape of the limb between the marks was made cylindrical rather than spherical, in order that the contained liquid might the more readily acquire the temperature of the bath in which the glischrometer was placed during an observation.

It will be seen from the figure that the upper ends of the limbs H^1, H^2 terminate within the glass traps T^1, T^2. These traps admit of slight adjustments of the volumes of liquid contained in the limbs, and their use is connected with that of the marks k^1 and k^2. During an experiment the levels of liquid in the two limbs are continually altering. The object of these marks and traps is to ensure that at the beginning of any observation in a particular limb the effective head of the liquid contained in the glischrometer shall be constant and shall be known. Let us suppose that an observation is to be made in the right limb; the liquid level in the left limb is just brought into coincidence with the mark k^1, when any excess of liquid will flow over into the trap T^2; hence the effective head of liquid extends from H^2 to k^1, and is thus known. A similar proceeding is carried out for the left limb observations, using the mark k^2 and trap T^1. The marks k^1 and k^2 are placed by trial in such positions that the volume from k^1 to H^2 is almost equal to, but slightly greater than, that from k^2 to H^1. The volumes k^1 H^2 and k^2 H^1 are the working volumes of liquid used in the observations.

The general arrangement of the whole apparatus is shown in Fig. 2. A bath B, which for observations at temperatures below 100° contains water, and for higher temperatures glycerin, is supported on an iron stand which is placed on a table in front of a window.* The bath is divided into two compartments. The inner compartment is provided back and front with plate glass walls; the rest of the bath is made of brass. The outer compartment bounds the inner at the sides and underneath, and is fitted with a tap for adjusting the quantity of liquid which it contains. The brass framework carrying the glischrometer, and thermometer T, can be lowered into vertical slots in the lateral walls of the inner compartment; when thus situated the glischrometer occupies a central position in the bath. The walls of both compartments are provided with guides, along which move stirrers consisting of brass plates pierced with holes, which are attached to suitable rods and cross pieces, and are worked by a small water-motor W M.

* In practice two baths were used, one containing water, the other glycerin.

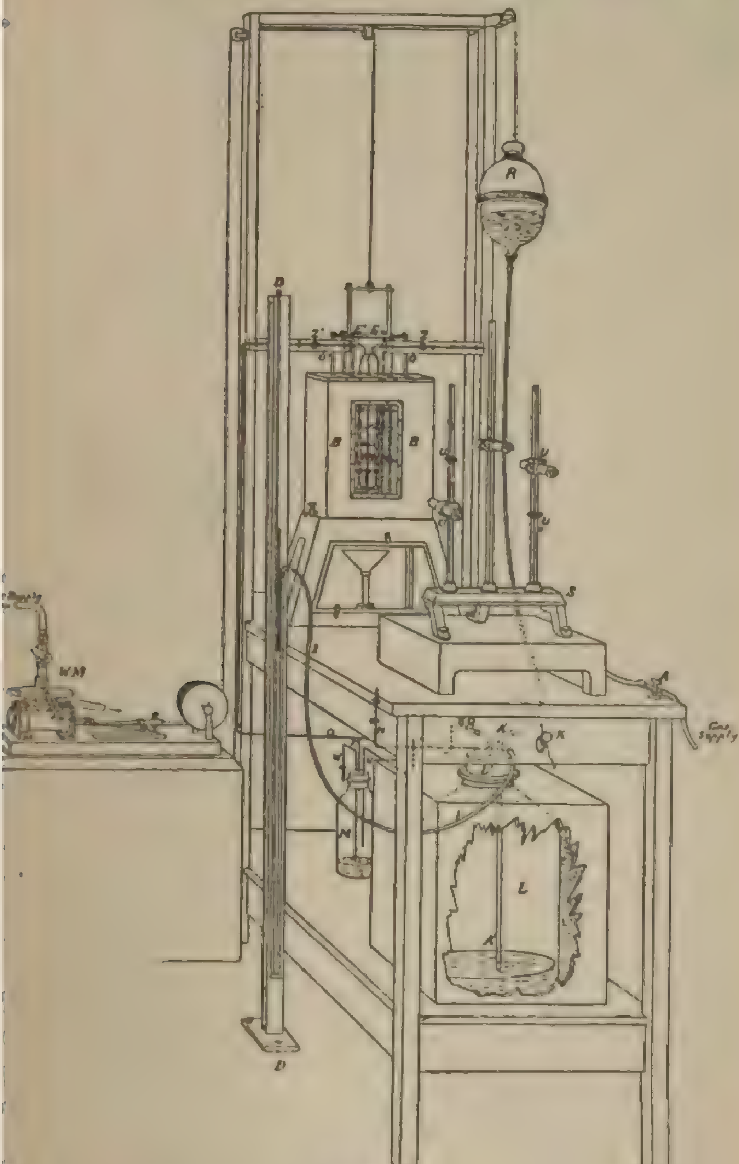

FIG. 2.

The rubber tube E connects the right limb of the glischrometer with the glass tube O, in which is inserted the three-way cock Z. In the same way E′ connects the left limb of the glischrometer with the tube O′ fitted with the three-way cock Z′. At P, O and O′ are united by a T piece which leads to the bottle M containing a quantity of sulphuric acid, which can be abstracted or replaced by means of the syphon W. The acid serves to dry air in its passage from the reservoir L to the glischrometer. When hygroscopic liquids are being experimented upon, the exit tubes of the three-way cocks are provided with small tubes filled with calcium chloride to prevent access of atmospheric moisture to the glischrometer. In this way it is insured that dry air only is in contact with the liquid under examination.

By means of the tube N, which extends from within a few millimetres of the surface of the acid in M to a centimetre or so below the cork L′, and which is fitted with the cock Q, the air in M may be put into communication with the large air reservoir L. This consists of a glass bottle of about 30 litres capacity, encased in a wooden box, and surrounded with sawdust to prevent excessive fluctuation of temperature. A glass tube A′, which reaches to within 5 millimetres, of the bottom of L, is connected, as shown, by india-rubber tubing with the water reservoir R. The air in L is compressed by raising the water reservoir, the height of which can be regulated by a cord leading by a system of pulleys to the stud X, in close proximity to the observer, and to the water manometer D D which indicates the pressure set up in the confined air space. The manometer is connected with the air reservoir by the tube I I, which has a common termination with the tube N.

After describing the method of making a viscosity observation, the lecturer proceeded to indicate how the coefficients of viscosity for the particular temperatures were deduced from the time and pressure of flow, and the constants of the glischrometer.

The coefficient of viscosity η may be found from the expression—

$$\eta = \pi R^4 t p / 8 l v - \rho V / 8 \pi l t,$$

in which R is the radius of the capillary tube and l its length, and V the volume of the liquid of density ρ passing through in time t and under pressure p. The negative term of the formula gives the measure of the correction for the kinetic energy imparted to the liquid, as deduced by Couette and Finkener.

With a view of tracing the influence of homology, substitution, isomerism, molecular complexity, and, generally speaking, of changes in the composition and constitution of chemical compounds upon viscosity, a scheme of work was drawn up which involved the determination in absolute measure of the viscosity of between 80 and 90 liquids at all temperatures between 0° (except in cases where the liquid solidified at that temperature) and their respective boiling points.

This list is as follows:—

Water	H_2O.
Bromine	Br_2.
Nitrogen peroxide	N_2O_4.

Paraffins and Unsaturated Fatty Hydrocarbons.

Pentane	$CH_3.(CH_2)_3.CH_3$.
Isopentane	$(CH_3)_2CH.CH_2.CH_3$.
Hexane	$CH_3.(CH_2)_4.CH_3$.
Isohexane	$(CH_3)_2CH.(CH_2)_2.CH_3$.
Heptane	$CH_3.(CH_2)_5.CH_3$.
Isoheptane	$(CH_3)_2CH.(CH_2)_3.CH_3$.
Octane	$CH_3.(CH_2)_6.CH_3$.
Trimethyl Ethylene (β-isoamylene)	$(CH_3)_2C:CH.CH_3$.
Isoprene (Pentine)	C_5H_8.
Diallyl (Hexine)	$CH_2:CH.(CH_2)_2.CH:CH_2$.

Iodides.

Methyl iodide	CH_3I.
Ethyl iodide	$CH_3.CH_2I$.
Propyl iodide	$CH_3.CH_2.CH_2I$.
Isopropyl iodide	$(CH_3)_2CHI$.
Isobutyl iodide	$(CH_3)_2CH.CH_2I$.
Allyl iodide	$CH_2:CH.CH_2I$.

Bromides.

Ethyl bromide	$CH_3.CH_2Br$.
Propyl bromide	$CH_3.CH_2.CH_2Br$.
Isopropyl bromide	$(CH_3)_2CHBr$.
Isobutyl bromide	$(CH_3)_2CH.CH_2Br$.
Allyl bromide	$CH_2:CH.CH_2Br$.
Ethylene bromide	$CH_2Br.CH_2Br$.
Propylene bromide	$CH_3.CHBr.CH_2Br$.
Isobutylene bromide	$(CH_3)_2CBr.CH_2Br$.
Acetylene bromide	$CHBr:CHBr$.

Chlorides.

Propyl chloride	$CH_3.CH_2.CH_2Cl$.
Isopropyl chloride	$(CH_3)_2CHCl$.
Isobutyl chloride	$(CH_3)_2CH.CH_2Cl$.
Allyl chloride	$CH_2:CH.CH_2Cl$.
Methylene chloride (Dichlormethane)	CH_2Cl_2.
Ethylene chloride	$CH_2Cl.CH_2Cl$.
Ethylidene chloride	$CH_3.CHCl_2$.
Chloroform (Trichlormethane)	$CHCl_3$.
Carbon tetrachloride (Tetrachlormethane)	CCl_4.
Carbon dichloride (Tetrachlorethylene)	$CCl_2:CCl_2$.

Sulphur Compounds.

Carbon bisulphide	CS_2.
Methyl sulphide	$(CH_3)_2S$
Ethyl sulphide	$(CH_3.CH_2)_2S$
Thiophen	$CH:CH.S.CH:CH$

Acetaldehyde and Ketones.

Acetaldehyde	$CH_3.COH$.
Dimethyl ketone	$CH_3.CO.CH_3$.
Methyl ethyl ketone	$CH_3.CH_2.CO.CH_3$.
Diethyl ketone	$CH_3.CH_2.CO.CH_2.CH_3$.
Methyl propyl ketone	$CH_3.(CH_2)_2.CO.CH_3$.

Acids.

Formic acid	$H.COOH$.
Acetic acid	$CH_3.COOH$.
Propionic acid	$CH_3.CH_2.COOH$.
Butyric acid	$CH_3.(CH_2)_2.COOH$.
Isobutyric acid	$(CH_3)_2CH.COOH$.

Oxides (Anhydrides).

Acetic anhydride (Acetyl oxide) ..	$(CH_3.CO)_2O$.
Propionic anhydride (Propionyl oxide)	$(CH_3.CH_2.CO)_2O$.

Aromatic Hydrocarbons.

Benzene	C_6H_6.
Toluene (Methyl benzene)	$C_6H_5.CH_3$.
Ethyl benzene	$C_6H_5.C_2H_5$.
Ortho-xylene	$C_6H_4(CH_3)_2(1:2)$.
Meta-xylene	$C_6H_4(CH_3)_2(1:3)$.
Para-xylene	$C_6H_4(CH_3)_2(1:4)$.

Alcohols.

Methyl alcohol	CH_3OH.
Ethyl alcohol	$CH_3.CH_2OH$.
Propyl alcohol	$CH_3.CH_2.CH_2OH$.
Isopropyl alcohol	$(CH_3)_2CHOH$.
Butyl alcohol	$CH_3.(CH_2)_2.CH_2OH$.
Isobutyl alcohol	$(CH_3)_2CH.CH_2OH$.
Trimethyl carbinol	$(CH_3)_3COH$.
Amyl alcohol (active)	$CH_3.CH_2.CH(CH_3).CH_2OH$.
Amyl alcohol (inactive)	$(CH_3)_2CH.CH_2.CH_2OH$.
Dimethyl ethyl carbinol	$(CH_3)_2C(OH).CH_2.CH_3$.
Allyl alcohol	$CH_2:CH.CH_2OH$.

Esters.

Methyl formate	$H.COOCH_3$.
Ethyl formate	$H.COOCH_2.CH_3$.
Propyl formate	$H.COOCH_2.CH_2.CH_3$.
Mthyl acetate	$CH_3.COOCH_3$.
Ethyl acetate	$CH_3.COOCH_2.CH_3$.
Propyl acetate	$CH_3.COOCH_2.CH_2.CH_3$.
Methyl propionate	$CH_3.CH_2.COOCH_3$.
Ethyl propionate	$CH_3.CH_2.COOCH_2.CH_3$.
Methyl butyrate	$CH_3.CH_2CH_2.COOCH_3$.
Methyl isobutyrate	$(CH_3)_2CH.COOCH_3$.

Ethers.

Ethyl ether	$CH_3.CH_2.O.CH_2.CH_3$.
Methyl propyl ether	$CH_3.O.CH_2.CH_2.CH_3$.
Ethyl propyl ether	$CH_3.CH_2.O.CH_2.CH_2.CH_3$.
Dipropyl ether	$CH_3.CH_2.CH_2.O.CH_2.CH_2.CH_3$.
Methyl isobutyl ether	$CH_3.O.CH_2.CH(CH_3)_2$.
Ethyl isobutyl ether	$CH_3.CH_2.O.CH_2.CH(CH_3)_2$.

In speaking of the results of the observations on these substances the lecturer drew special attention to the case of water, more particularly as regards the effect of temperature in altering its viscosity. The following table shows the viscosity of water in absolute measures at temperatures between 0° and 100° C.

Temperature.	Viscosity.	Temperature.	Viscosity.	Temperature.	Viscosity.
°		°		°	
0	·01778	35	·00720	70	·00406
5	·015095	40	·006535	75	·003795
10	·013025	45	·00597	80	·00356
15	·011335	50	·005475	85	·00335
20	·010015	55	·005055	90	·003155
25	·00891	60	·00468	95	·002985
30	·007975	65	·004355	100	·00283

The results of these observations are graphically represented in Fig. 3, in which viscosity coefficients are ordinates and temperatures are abscissæ.

A special series of observations was made in order to ascertain if, as inferred by Moritz, water had a maximum viscosity in the neighbourhood of 4°, but no indication was given of any anomalous change in the rate of variation between 0° to 8°, and the lecturer pointed out the bearing of this fact upon the supposition that water at low temperature is a solution of ice, richer and richer in ice as it is more and more cooled.

The so-called anomaly of water possessing a point of maximum density remote from its point of congelation, must be connected with its other physical properties, and observation shows this to be the case. Water, like all other liquids, is compressible, but whereas in the case of all other liquids the compressibility increases with the temperature, it is found that water at low temperature is more compressible than at high temperatures. It has also been shown that water is "anomalous" in respect to its behaviour when heated under pressure. The degree to which it expands for a given interval of temperature steadily increases with the pressure, and especially at low temperatures, contrary to what is usually observed. The viscosity of water is also affected by pressure. It has been shown by Warburg and Sachs, and also by Rontgen, that water at ordinary temperatures

becomes more mobile when subjected to pressure: in other words, its viscosity is lowered by pressure. This is a very striking fact, and so far as observation has gone it is without a parallel. Benzene, ether, liquid carbon dioxide, all become more viscous under the

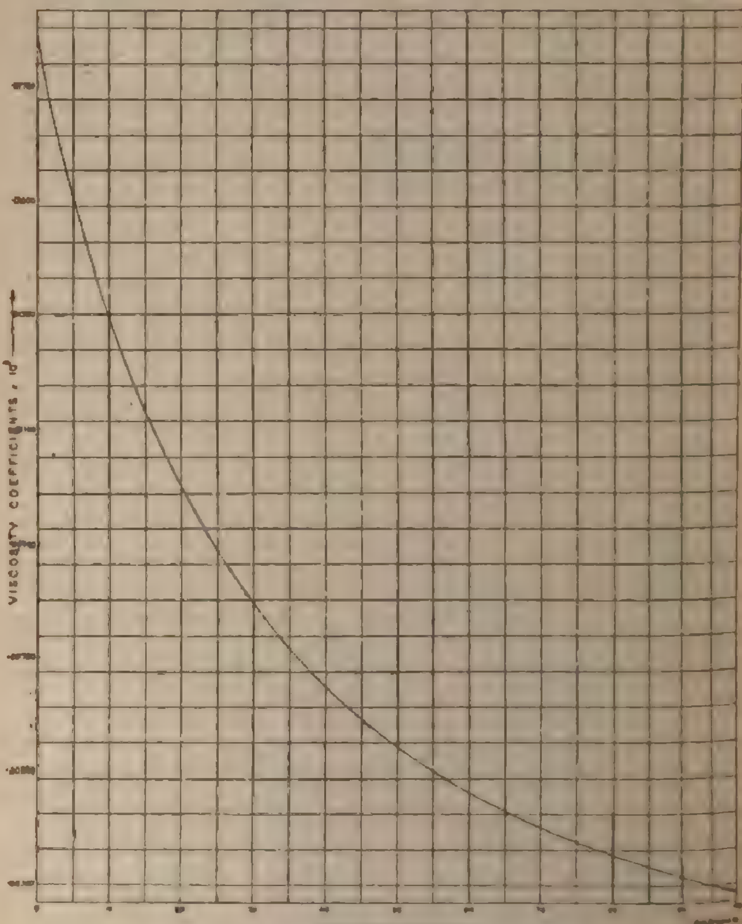

FIG. 3.—Viscosity of Water between 0° and 100°.

influence of great pressure. Now Professor Röntgen has pointed out that these "anomalies" may be explained on the assumption that water at ordinary temperatures is an aggregation of two distinct kinds of molecules, one of which has the properties we associate with

ice. The proportionate amount of these "ice-molecules" depends, under ordinary conditions, upon the temperature. On heating they become fewer and fewer; on cooling they become more numerous. We may regard water at any particular temperature as a saturated solution of such molecules; when cooled below its ordinary solidifying point it is a supersaturated solution of such molecules, and of course behaves under such conditions like any other supersaturated solution.

Now any circumstance which effects the transformation of the ice-molecules into the other kind of molecules should be attended by a contraction of volume. When water is heated from 0° upwards, we have two distinct volume changes—expansion of the water as such, and the destruction or transformation of the ice-molecules with consequent diminution of volume. Up to 4° the diminution due to the transformation of the ice-molecules is greater than the expansion, and the nett result is contraction. After 4° the ice-molecules become fewer and fewer, and the degree of expansion gradually gains upon that of the diminution in volume due to the alteration of the ice-molecules; and thence the degree of contraction becomes less and less, until the nett result is an increase of volume and the water seems to behave like any other liquid on heating. It does not, however, follow that all the so-called ice-molecules will have disappeared, even at above 8°, for the two distinct sets of molecules may co-exist, but of course in gradually diminishing ratio as the temperature rises.

It is easy to see how this assumption, which is but an extended form of a very old idea, may serve to explain the "anomalies" above referred to. Take the case of compressibility of water at low temperatures. It is unnecessary to remind a Royal Institution audience that ice, even at low temperatures, may be converted into water by pressure; the classical experiments of Faraday and Tyndall are admirable illustrations of that fact. Now the more ice we thus convert into water the greater the contraction. A given increase of pressure at a low temperature causes a greater contraction than at a higher temperature, because at the lower temperature there are more ice-molecules to be changed. The diminution of volume under compression is like the increase of volume by temperature, made up of two parts, viz. (1) the real compressibility of the water; and (2) the diminution attending the transformation of the ice-molecules. Probably the water-molecules, as such, behave like other molecules—they contract under pressure, and to a gradually smaller extent as the pressure is increased; it is only the effect of the increased pressure in changing the ice-molecules, with consequent diminution of volume, that makes the *apparent* compressibility greater, and thus gives rise to the "anomaly." It should follow, therefore, that at some point of temperature above the freezing point of water there should be a minimum point of compressibility, just as there is a minimum volume. Experiment shows that such a minimum point exists at about 50°.

The fact, discovered by Amagat, that water under great pressure is more expansible by heat than at ordinary pressure, may also be equally well explained on this hypothesis. Increasing temperature, as we have seen, works in two directions on the volume of water—but as yet nothing is exactly known of the effect of pressure upon the volume-change per degree of temperature of an aggregate consisting solely of one kind of water-molecules; but the probability is that such an aggregate of molecules would behave like a gas. The anomaly found by Amagat gradually disappears as the pressure is increased. This finds its explanation in the fact that with gradually increasing pressure the number of ice-molecules becomes less. Amagat also found that the anomaly became less marked as the temperature was increased; this also is explained by the circumstance that as the temperature increases the number of the ice-molecules diminishes.

The same hypothesis explains the fact that under pressure the temperature of the point of maximum density becomes lower, and it also affords a reason for the circumstance that the freezing point of water becomes lowered by pressure.

It has been observed that water at low temperatures becomes colder when subjected to pressure, which may be explained by the fact that in order to convert ice-molecules into molecules of the second kind, heat is required, which can only be furnished by the compressed liquid.

As regards the influence of pressure on viscosity, we have only to assume, as analogy indicates, that the greater the number of ice-molecules in solution the more viscous becomes the liquid. If we add soluble matter to water, its viscosity increases. Sea water is more viscous than pure water, and the greater the amount of salt in solution the greater becomes the viscosity. If by pressure we diminish, for any particular temperature, the number of ice-molecules in solution, it must follow that we diminish the viscosity, which is what is observed.

Now, in the light of Professor Röntgen's explanation, the behaviour of water is no longer "anomalous." Its normal properties are exactly similar to those of any other liquid. The so-called anomalies are simply due to the circumstance that the "solid" form of water is specifically lighter than the liquid form. The peculiar form of the curve showing the relation between viscosity and temperature in the case of water at low temperatures, arises from the progressive and rapid increase of the number of the ice-molecules. In this special particular water is not peculiar. Studies on surface energy, on vapour pressures and densities, and on optical characters, have shown that this hypothesis of molecular complexes is well founded, and it is remarkable that many liquids, especially hydroxyl combinations, in which there is reason to assume the existence of such complexes, also exhibit curves of viscosity very similar in character to that shown by water.

The mathematical expression of the relation of the viscosity of

liquids to temperature has engaged the attention of many physicists from the time of Poiseuille, but, on the whole, no empirical formula reproduces the observed values better than that of Slotte, which may be written in the shape—

$$\eta = C / (a + t)^n.$$

In order to determine the value of the constants two values of η, viz., η_1 and η_3, are chosen, which correspond respectively with the temperatures t_1 and t_3; a third value of η, viz. η_2, is then found from the equation $\eta_2 = \sqrt{\eta_1 \eta_3}$, and the temperature t_2 corresponding with this value η_2 is found graphically, and a and n are deduced from the equation—

$$a = \frac{t_2^2 - t_1 t^3}{t_1 + t_3 - 2t_2} \quad n = \frac{\log \eta_1 - \log \eta_3}{\log (a + t_3) - \log (a + t_1)}.$$

Writing the formula in the shape $\eta = C / (1 + b\,t)^n$, where C is the viscosity coefficient at 0°, the experimental results in the case of the whole series of liquids may be accurately represented by formulæ of the Slotte type by means of the following constants.

CONSTANTS IN SLOTTE'S FORMULA, $\eta = C/(1 + b\,t)^n$.

—	C.	*b*.	*n*.
Pentane	·002827	·006039	1·7295
Hexane	·003965	·005279	2·1264
Heptane	·005180	·005551	2·1879
Octane	·007025	·006873	2·0290
Isopentane	·002724	·008435	1·2901
Isohexane	·003713	·004777	2·3237
Isoheptane	·004767	·005541	2·1633
Isoprene	·002600	·006944	1·4433
Amylene	·002534	·005341	1·7855
Diallyl	·003388	·005780	1·9340
Methyl iodide	·005940	·007444	1·4329
Ethyl iodide	·007190	·006352	1·7520
Propyl iodide	·009372	·007308	1·7483
Isopropyl iodide	·008783	·006665	1·9161
Isobutyl iodide	·011620	·009186	1·6577
Allyl iodide	·009296	·007933	1·6592
Ethyl bromide	·004776	·007212	1·4749
Propyl bromide	·006448	·006421	1·8282
Isopropyl bromide	·006044	·005916	2·0166
Isobutyl bromide	·008234	·006187	2·1547
Allyl bromide	·006190	·006895	1·7075

CONSTANTS IN SLOTTE'S FORMULA, $\eta = C/(1 + b\,t)^n$—*continued.*

—	C.	b.	n.
Ethylene bromide	·024579	·012375	1·6222
Propylene bromide	·023005	·011267	1·7075
Isobutylene bromide	·033209	·013227	1·7988
Acetylene bromide	·012307	·008905	1·5032
Bromine	·012535	·008935	1·4077
Propyl chloride	·004349	·004917	2·2453
Isopropyl chloride	·004012	·007485	1·5819
Isobutyl chloride	·005842	·007048	1·8706
Allyl chloride	·004059	·006366	1·7459
Ethylene chloride	·011269	·009033	1·6640
Ethylidene chloride	·006205	·007575	1·6761
Methylene chloride	·005857	·007759	1·4408
Chloroform	·007006	·006316	1·8196
Carbon tetrachloride	·013466	·010521	1·7121
Carbon dichloride	·01139	·007925	1·6325
Carbon bisulphide	·004294	·005021	1·6328
Methyl sulphide	·003538	·005871	1·6981
Ethyl sulphide	·005589	·006705	1·8175
Thiophen	·008708	·009445	1·6078
Dimethyl ketone	·003949	·004783	2·2244
Methyl ethyl ketone	·005383	·007177	1·7895
Methyl propyl ketone	·006464	·007259	1·8248
Diethyl ketone	·005919	·006818	1·8626
Acetaldehyde	·002671	·003495	2·7550
Formic acid	·029280	·016723	1·7164
Acetic acid	·016867	·008912	2·0491
Propionic acid	·015199	·009130	1·8840
Butyric acid	·022747	·010586	1·7020
Isobutyric acid	·018872	·009557	2·0059
Acetic anhydride	·012416	·010298	1·6851
Propionic anhydride	·016071	·011763	1·7049
Ethyl ether	·002864	·007332	1·4644

CONSTANTS IN SLOTTE'S FORMULA, $\eta = C/(1 + b\,t)^n$—*continued.*

—	C.	b.	n.
Benzene	·009055	·011963	1·5554
Toluene	·007684	·008850	1·6522
Ethyl benzene	·008745	·008218	1·7616
Ortho-xylene	·011029	·010379	1·6386
Meta-xylene	·008019	·008646	1·6400
Para-xylene	·008457	·008494	1·7326
Water—			
0° to 8°	·017793	·017208	1·9944
0° to 100°	·017944	·023121	1·5423
Methyl alcohol	·008083	·006100	2·6793
Ethyl alcohol	·017753	·004770	4·3731
Propyl alcohol	·038610	·007366	3·9188
Butyl alcohol—			
0° to 52°	·051986	·007194	4·2452
52° to 114°	·056959	·010869	3·2150
Isopropyl alcohol—			
0° to 40°	·045588	·007057	4·9635
40° to 78°	·048651	·011593	3·4079
Isobutyl alcohol—			
0° to 38°	·080547	·010840	3·6978
38° to 75°	·085365	·011527	3·6708
75° to 105°	·094725	·015838	3·0537
Inactive amyl alcohol—			
0° to 40°	·085358	·008488	4·3249
40° to 80°	·093782	·012520	3·3395
80° to 128°	·152470	·026540	2·4618
Active amyl alcohol—			
0° to 35°	·111716	·009851	4·3736
35° to 73°	·124788	·015463	3·2542
73° to 124°	·147676	·127583	2·0050
Trimethyl carbinol—			
20° to 50°	·135060	·128156	1·8232
50° to 77°	1·755458	·196967	2·0143
Dimethyl ethyl carbinol—			
0° to 27°	·142538	·020868	3·2080
27° to 63°	·154021	·027019	2·7578
63° to 95°	·131901	·026082	2·6610
Allyl alcohol	·021736	·009139	2·7925
Nitrogen peroxide	·005267	·007098	1·7319
Methyl formate	·001301	·014655	0·8325
Ethyl formate	·005048	·007197	1·7006
Propyl formate	·006679	·007179	1·9154

CONSTANTS IN SLOTTE'S FORMULA $\eta = C/(1 + b\,t)^n$—*continued.*

—	C.	b.	n.
Methyl acetate	·004781	·006472	1·8636
Ethyl acetate	·005783	·007384	1·8268
Propyl acetate	·007706	·007983	1·8372
Methyl propionate	·005816	·006820	1·8972
Ethyl propionate	·006928	·007468	1·8914
Methyl butyrate	·007587	·008081	1·8375
Methyl isobutyrate	·006720	·007144	1·9405
Diethyl ether	·002864	·007332	1·4644
Methyl propyl ether	·003077	·006809	1·5863
Ethyl propyl ether	·003969	·005454	2·1454
Dipropyl ether	·005401	·006740	1·9734
Methyl isobutyl ether	·003813	·005737	2·0109
Ethyl isobutyl ether	·004826	·006549	1·9733

Slotte's formula gives the best results in the case of observed viscosity curves in which the slope varies but little with the temperature. As regards the relation between the chemical nature of the substances and the magnitude of their temperature coefficients, it is evident that—

(*a*) From the mode in which the constants n and b are derived, their individual values cannot be expected to be simply related to chemical nature.

(*b*) For the majority of the liquids the formula—

$$\eta = C/(1 + \beta t + \gamma t^2)$$

obtained from Slotte's formula by neglecting terms in the denominator involving higher powers of t than t^2, closely expresses the effect of temperature on viscosity, and in the formula the magnitudes of the coefficients β and η are found to be definitely related to the molecular weight and constitution of the substances, except in the case of liquids which, like water and the alcohols, contain molecular aggregate.

In order to obtain quantitative relationships between viscosity and chemical nature, and to compare one group of substances with another, it is necessary to fix upon particular temperatures, and to obtain and compare the values corresponding with those temperatures. The first point to decide was at what temperatures viscosities should be compared. Inasmuch as the viscosity curves, even in the same family of substances, cross one another, it is obvious that quantitative relationships obtained at any single temperature of comparison, as has usually been done, can have no pretensions to generality. Following the method of Kopp, temperature of the boiling point may be considered as a comparable temperature, or we may adopt the

method indicated by Van der Waals; or, lastly, we may compare the viscosity values at the temperatures of equal slope, or at temperatures at which $d\eta / dt$ is the same for the different liquids—that is, points at which temperature is exercising the same effect on viscosity.

Now, no matter which of these modes of comparison be instituted, definite general relations are apparent. Thus, if we compare the viscosity coefficients at the boiling points, it is found that as a homologous series is ascended the coefficients, as a rule, diminish. Of corresponding compounds, the one having the highest theoretical molecular has the highest coefficient. Normal propyl compounds have higher values than allyl compounds, and an iso-compound has a larger coefficient than a normal compound. In the case of other metameric substances, branching of the atomic chain and the symmetry of the molecule influence the magnitudes of the coefficients, the ortho-position in the case of aromatic compounds having a more marked effect than either the meta- or para-positions. There are, however, certain significant exceptions to the universality of these rules, but these are in all probability dependent upon differences in molecular complexity, as there is independent reason for believing that the anomalous liquids contain molecular aggregates. Very similar, although less definite, relationships are obtained at corresponding temperatures obtained by the method of Van der Waals, and these are still more obvious when the comparisons are made at temperatures of equal slope.

The attempt has been to ascertain if molecular viscosity can be expressed as the sum of partial effects which may be ascribed to the atoms and to the modes of atom linkage which occur in the molecule, and it has been found possible to obtain values for particular elements and groups, and to trace the special influence of the iso-grouping, of ring grouping, and of double linkage, upon the viscosity of a liquid in such manner as to obtain a very fair agreement between the observed and calculated value. Fundamental viscosity constants have thus been obtained for the various elements, and it is possible to assign a quantitative value to specific differences in molecular arrangement. Thus the fundamental viscosity constants at temperatures of equal slope may, for a particular slope, be expressed as follows:—

FUNDAMENTAL VISCOSITY CONSTANTS.

Hydrogen		H	44·5
Carbon		C	31
Hydroxyl-oxygen	C—O—H	$\diagdown\diagup$ O	166
Ether-oxygen	C—O—C	O<	58
Carbonyl-oxygen	C=O	‖ O	198

FUNDAMENTAL VISCOSITY CONSTANTS—*continued.*

Sulphur	C—S—C	$\diagdown\diagup$ S	246
Chlorine (in monochlorides)		Cl	256
Chlorine (in dichlorides)		Cl′	244
Bromine (in monobromides)		Br	372
Bromine (in dibromides)		Br′	361
Iodine		I	499
Iso grouping		<	− 21
Double linkage		(=)	48
Ring-grouping		(O)	244

The following tables show the numbers calculated by means of these constants, together with those actually observed in a number of cases:—

—	Observed.	Calculated.	Difference per cent.
Pentane	687	689	−0·3
Hexane	818	809	1·1
Heptane	931	929	0·2
Octane	1035	1049	−1·3
Isopentane	663	668	−0·7
Isohexane	799	788	1·4
Isoheptane	908	908	0·0
Isoprene	620	607	2·1
Diallyl	728	729	−0·1
Methyl iodide	638	664	−4·0
Ethyl iodide	778	784	−0·8
Propyl iodide	903	904	−0·1
Isopropyl iodide	878	883	−0·6
Isobutyl iodide	1010	1003	0·7
Allyl iodide	864	866	−0·2
Ethyl bromide	663	657	0·9
Propyl bromide	774	777	−0·4
Isopropyl bromide	750	756	−0·8
Isobutyl bromide	877	876	0·1

—	Observed.	Calculated.	Difference per cent.
Allyl bromide	734	739	−0·7
Ethylene bromide	973	962	1·1
Propylene bromide	1068	1082	−1·3
Isobutylene bromide	1171	1181	−0·9
Acetylene bromide	932	921	1·2
Propyl chloride	658	661	−0·4
Isopropyl chloride	644	640	0·6
Isobutyl chloride	760	760	0·0
Allyl chloride	617	623	−1·0
Ethylene chloride	737	728	1·2
Methylene chloride	600	600	0·0
Methyl sulphide	578	575	0·5
Ethyl sulphide	812	815	−0·3
Dimethyl ketone	572	558	2·4
Methyl ethyl ketone	671	678	−1·0
Methyl propyl ketone	796	798	−0·2
Diethyl ketone	785	798	−1·6
Acetaldehyde	448	438	2·2
Formic acid	456	484	−6·1
Acetic acid	593	604	−1·8
Propionic acid	742	724	2·4
Butyric acid	842	844	−0·2
Isobutyric acid	843	823	2·4
Acetic anhydride	838	845	−0·8
Propionic anhydride	1036	1085	−4·7
Ethyl ether	635	627	1·3
Benzene	688	697	−1·3
Toluene	821	814	0·8
Ethyl benzene	939	934	0·5
Ortho-xylene	954	934	2·1
Meta-xylene	939	934	0·5
Para-xylene	923	934	−1·2

These general results are, it should be stated, independent of the magnitude of the slope: no matter what particular value be selected, the relations are made obvious. Of course, in the actual comparison, such a value of the slope was selected as would comprehend the greatest number of observed cases.

In conclusion it may be pointed out that a comprehensive view of the physico-chemical relationships of a series of substances can only

be obtained by studying the variation of the physical property over as wide a range of temperature as possible; that the graphical or algebraical representation of the results so obtained will indicate whether particular members of a series are exceptional in behaviour as compared with their congeners; and if such exceptional behaviour occurs it may be detected either in the viscosity-magnitude or the temperature, no matter whether we use the boiling point, a corresponding temperature, or a temperature of equal slope as the condition of comparison.

[T. E. T.]

GENERAL MONTHLY MEETING.

Monday, March 7, 1898.

SIR JAMES CRICHTON-BROWNE, M.D. LL.D. F.R.S.
Treasurer and Vice-President, in the Chair.

Miss Cecilia Ash,
Mrs. Henry C. A. Baynes,
Miss Mary E. Bevington,
The Hon. Edith M. Boscawen,
Miss Alice M. Burton,
The Rev. J. J. Coxhead, M.A.
Alfred Charles Cronin, Esq.
Ralph Collingwood Forster, Esq.
William Garnett, Esq. M.A. D.C.L.
Herbert Godsal, Esq.
Alexander H. Goschen, Esq.
Major-General Coleridge Grove, C.B.
Arthur Humbert, Esq.
Joseph Kincaid, Esq. M.A. M. Inst. C.E.
Captain William N. Lister,
Lazare M. Lowenstein, Esq.
George Wharton Marriott, Esq.
Mrs. E. R. Merton,
Thomas Middlemore, Esq.
Bertram Savile Ogle, Esq. J.P.
Paris Eugene Singer, Esq.
George Paul Taylor, Esq.
John Thornton, Esq.
Sir Arthur Spencer Wells, Bart.

were elected Members of the Royal Institution.

The Special Thanks of the Members were returned for the following Donation to the Fund for the Promotion of Experimental Research at Low Temperatures:—

Mr. Hugh Leonard £50

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Meteorological Office—Meteorological Observations at Stations of the Second Order for 1894. 4to. 1897.
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Editors—American Journal of Science for Feb. 1898. 8vo.
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Editors—continued.
Brewers' Journal for Feb. 1898. 8vo.
Chemical News for Feb. 1898. 4to.
Chemist and Druggist for Feb. 1898. 8vo.
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Electrical Engineer for Feb. 1898. fol.
Electrical Engineering for Feb. 1898. 8vo.
Electrical Review for Feb. 1898. 8vo.
Electricity for Feb. 1898. 8vo.
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Engineering for Feb. 1898. fol.
Homœopathic Review for Feb. 1898. 8vo.
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Abhandlungen, Band XXIV. Nos. 2, 3. 8vo. 1898.
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Berichte, 1897, No. 2. 8vo. 1898.
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WEEKLY EVENING MEETING,

Friday, March 11, 1898.

SIR FREDERICK BRAMWELL, BART. D.C.L. LL.D. F.R.S. Honorary Secretary and Vice-President, in the Chair.

WALTER FREWEN LORD, Esq.

"Marked Unexplored."

THE small area of unexplored history that I shall ask your leav open up this evening is that curious backwater of Mediterran history which I have called Murat's dream. It was an early atte to unify Italy, and was defeated by Lord William Bentinck and Lo Philippe (afterwards King of the French) when Duc d'Orleans.

In order to facilitate my exposition of this highly complica period I will ask your attention to these four maps. The first rep sents Murat's dream; the second represents what actually happe to Italy when that dream ceased to be even an aspiration; the th represents Italy at the present moment: I will speak of the fo map presently. As regards the first map I need hardly remind y that after the battle of Austerlitz the ancient kingdom of the T Sicilies was conferred upon Joseph Bonaparte by his brother Na leon. This was a simple operation in so far as the mainl dominions were concerned; but Sicily, being an island, and p tected by the British fleet, was beyond Napoleon's reach, and ne passed out of the hands of the Neapolitan Bourbons. Joseph, wh presented to the throne of Spain, was succeeded by Joachim Mu Napoleon's brother-in-law, and at the time that our story opens M was *de facto* King of Naples. To that kingdom he had recently ad the States of the Church. He was in military occupation of Grand Duchy of Tuscany. His brother-in-law, Prince Borghes not a warlike or an ambitious person—was in occupation of Piedm the King of Sardinia having retired to the island from which he h his title. Sardinia was at this epoch, all that remained to present Royal House of Italy. It is obvious that this is the larg homogeneous dominion actually and potentially (for there wo have been no difficulty about Lucca, Parma and Modena) ever car out in Italy since the fall of the Roman Empire until the year 18

Murat proposed to make this a permanent settlement: leav Ferdinand of Bourbon in Sicily, the House of Savoy in Sardi England in Corsica, and Austria in her dominions of Northern It England and Austria assented to this plan; and before we con

consider how a scheme so powerfully supported was not carried out, we must first ask why England and Austria (neither power yielding to the other in hatred of Napoleon and his family) came to sanction it.

It was a question of military expediency. On the 28th of October, 1813, the Allied Armies halted on the right bank of the Rhine. But they durst pursue Napoleon no further; and waited for their left wing to swing round and take Napoleon in the rear. But their left wing could not swing round. Marshal Bellogarde, commanding the Austrian army, refused to move a man under existing circumstances. He openly stated that the Allies must come to a compromise. Somehow or other Murat must be detached from Napoleon's cause in order to break up the deadlock in Italy.

The fourth map. The deadlock in Italy was caused in this way. Eugene Beauharnais' army of 40,000 men practically held in check the Austrian army of 70,000, because Bellegarde was compelled to detach an army corps to watch Murat, who in his turn could do nothing because he was between Bellegarde and Bentinck. If Murat could only be won over to the cause of the Allies, they would command 120,000 men to Beauharnais' 40,000, and France could easily be invaded by way of the Riviera. Murat deserted the Emperor, and threw in his cause with the Allies; his price being his own definite and official recognition as King of Naples, while he on his part consented to recognise Ferdinand as King of Sicily.

Murat's conduct has been variously described. We shall see, presently, what Bentinck thought of it. M. Thiers records that Napoleon said that he had made a great mistake in making Murat a king, as he now thought only of his own kingdom and France came second. Murat himself stated that he was now an Italian, and thought only of the interest of Italy. The Austrians thought that Murat meant to make the most for himself out of the situation, that his defection might be useful to them, and that, further, Murat had excellent grounds for dissatisfaction with his brother-in-law's interfering and imperious behaviour.

Be that as it may, Murat quitted the Grand Army after a violent quarrel with the Emperor, and betook himself to Italy with the object of unifying it in the manner I have briefly sketched.

At this time Lord William Bentinck was Commander-in-Chief and Ambassador Extraordinary in Sicily. He commanded about 30,000 men. Bellegarde was the Austrian Commander-in-Chief, Count Mier was the Austrian Chargé d'Affaires, and Count Neipperg was the Austrian Ambassador Extraordinary charged with the execution of the Treaty of Alliance and Recognition. Lord William Bentinck was charged with the same duty on the part of England.

Lord William Bentinck received his instructions early in January 1814 from Lord Castlereagh. This is the temper in which he received them. "I was always afraid that Count Neipperg would be overreached by that Italian court" (meaning Naples). "The conditions of this treaty are altogether impolitic, inexpedient and

unnecessary. Upon Murat no reliance can ever be placed. But this treaty creates not only a rival but a master perhaps in Italy" (which is exactly what it was intended to do). "When the Viceroy" (Eugene Beauharnais) "is driven back to the Alps the Italians will certainly gravitate towards Murat. But if the British protection and assistance had happened to be within their reach, that great floating force would certainly have ranged under their standard. The national energy would then have been roused, like Spain and Germany, in honour of national independence, and this great people, instead of being the instrument of the ambitions of one military tyrant or another, or, as formerly, the despicable slaves of a set of miserable petty princes, they would have become a powerful barrier both against Austria and France, and the peace and happiness of the world would receive a great additional security—but I fear the hour is gone by. It is lamentable also to see superior rewards showered upon a man whose whole life has been crime" (this means Murat), "who has been the intimate and active partner of all Bonaparte's wickedness, and whose last act of treachery to his benefactor has been the result of necessity. This treaty is a sad violation of all public and private principle."

I am sure that you will be grateful to me when I say that that is the only one of Lord William Bentinck's despatches that I shall read to you.

I apprehend that it is open to an ambassador to have his private opinion on his instructions; but when his views are of this violent character there are only two courses that he can pursue with self-respect and honesty: the first is, do what Benjamin Keene did when he was directed to surrender Gibraltar to Spain. He rent his garments in rage and mortification—and then did what he was told. The second is to do what Gilbert Elliot did when he was ordered to carry on the government of Corsica under impossible conditions. He asked that he might be replaced immediately; but if any value was placed upon his services, the conditions of his charge must be altered as he indicated. Bentinck took neither of these courses. He used his instructions to defeat the plans of the Cabinet. Thus in sending Mr. Graham, his private secretary, to Naples, ostensibly to sign the treaty, the terms of which had been already settled between England and Austria, he directed him to use his intimacy with the Neapolitan court, in order to obtain a passport to the Austrian headquarters. Such a passport was courteously granted to him, of course under the impression that it was being granted to a man who was at work on the treaty. Not at all. "You will use the armistice as a means of getting to the headquarters and informing the authorities *in secret* that I am about to occupy Corsica with 10,000 foot, 400 horse and 30 guns," and to concert this landing with them.

In due course the King of Naples' envoys, Colonel Barthemy, an A.D.C. of King Joachim, and Baron d'Aspern of Count Neipperg's suite, arrived at Palermo to do their work. Bentinck "refused to

compromise himself in any manner." "Refused to compromise himself," by obeying the orders of his sovereign.

Mr. Graham, on our side, arrived at Naples on the 5th of January, was conveyed in a royal carriage to the Duc de Gallo's, where he met Count Neipperg and Menz. They naturally supposed that Graham had come to sign the treaty on Bentinck's behalf; but when it was presented to him, Graham said that he had no instructions. The Austrians stared at him, and naturally wondered what in that case he had come to Naples for. They did not suspect Bentinck's perfidy.

After a few days of dining and fêting, Mr. Graham had another interview with Count Neipperg. Count Neipperg was completely bewildered at Graham's attitude. The question, he said, had been settled by Lord Aberdeen and Prince Metternich, acting under the orders of their sovereigns, and neither he nor Lord William Bentinck, still less Mr. Graham, could pretend to any discretion in the matter. They were merely agents. Graham was a loyal private secretary, and struggled hard in an impossible situation. At length he dropped a word in favour of King Ferdinand, and Neipperg flashed out at him, "It was absurd," he said, "that a useless monarch should stand in the way of the peace of Europe; and Austria," he went on, was quite prepared to force Ferdinand to renounce Naples if he did not do so of free will.

"A useless monarch" is a remarkable expression when applied to a Bourbon sovereign married to an Austrian archduchess, and applied, too, by the ambassador of the Austrian Emperor. I think it shows how determined Austria was to establish the throne of Murat. For the rest the epithet is entirely in place. Never was there a more useless monarch than Ferdinand of Naples.

Neipperg summoned up the resolve of his court in these words: "Wherever we can find a soldier to oppose to the French armies, we shall buy him at any cost," and "King Joachim must now have a better military frontier." That is a well-known diplomatic phrase, and, of course, implied a large addition to his territory. Thus the intentions of Austria were manifest. Murat, on his side, by the mouth of the Duke of Campochiaro, stated plainly that his determination was to be the leader of United Italy; that in that cause he had no desire for any ally except England: with himself on land and England in alliance at sea, he said, United Italy was a certainty. He was so well aware, he added, of the hopelessness of ever rivalling England at sea, that he was ready to hand over all his ships to England at once. So near as this was Italy to being unified in the year 1814.

Graham, on his part, would say nothing definite, listened to everyone, reported to Bentinck, and even went so far in dissimulation as to arrange an imaginary campaign, with King Joachim commanding the centre of the Allied Army, and having Bellegarde on his right wing and Bentinck on his left. So loyal was he in a disloyal cause. He then extracted the passport for the Neapolitan Ministry for

Foreign Affairs, and made his way to Geneva, the headquarters of the Allies, ostensibly to forward the plans of Murat, really to thwart them. Thus Bentinck had managed to waste a fortnight, and England was still unpledged.

On the 7th of January, 1814, the treaty between Austria and Naples was signed by the Duke of Gallo for King Joachim and by Adam, Count Neipperg, for the Emperor. The secret articles bound Austria to obtain the recognition of King Joachim by England, and to compel King Ferdinand of Sicily (by force if necessary) to acknowledge that Naples had passed away from him for ever. The next day Count Neipperg wrote to Bentinck and remonstrated at the delay. He urged all the arguments that he could think of (and what a strange notion of English discipline he must have formed when he found that he had to coax a lieutenant-general into obeying his sovereign's orders), and wound up by reminding Bentinck of the very serious nature of the European crisis. If it turned out badly, he urged, the world would hold Neipperg and Bentinck to be responsible.

Three weeks later, on the 30th of January, Bentinck gave some signs of life. He wrote a despatch to Castlereagh, complaining without the slightest grounds, so far as I have been able to discover, of an "apparent want of good faith" on the part of Austria; and adding, "I am aware that Murat wishes to make every possible parade and demonstration of a good understanding with Great Britain, as the most effectual means of quieting the discontent existing both among his subjects and his army." Note the discourteous expression "Murat," instead of the "King of Naples." This is only an exaggerated instance of Bentinck's habitual attitude towards those with whom he was dealing. You would gather from his letters that he was the only honest man in Italy. "In point of fair dealing, I consider Prince Metternich and King Murat to be nearly on a level."

Having pushed sheer inertia so far as it was possible to push it without running the risk of being recalled, Bentinck now proceeded in a leisurely way to take action; with how much intention that it should be effective we may suppose when he writes, "I feel considerable embarrassment in what manner I should act." Considerable embarrassment! With his instructions on the table in front of him! He began by saying that he could not possibly go to Naples except *incognito*. What an extraordinary condition for an ambassador to make; and added that he could not set foot in Naples until he was definitely assured on that point, as he was in the embarrassing situation of being the ambassador of a government that so far had not recognised the King of Naples. When the whole point of his instructions was to recognise him, and that immediately!

What adds a touch of grim humour to the situation, is Bentinck's habit of writing officially of his "straightforwardness," his "uprightness," on one occasion of his "known frankness."

At last, on the 6th of February, this man of known frankness

made his way to Naples and wrote to Lord Castlereagh that the Duc de Gallo and Count Neipperg were most pressing for him to sign, but that he would not, because no reliance was to be placed upon Murat. However, he went so far as to sign an armistice, which was all the Allies could get out of him. He then returned to Palermo, and took up the routine of administration there, leaving the Austrians and Neapolitans gazing at each other in mute amazement at finding so irresponsible a person in so responsible a situation.

In Palermo he found a despatch from Lord Castlereagh, directing him to inform the Crown Prince that it was out of the question for the Royal family of Sicily to hope any more for the restoration of Naples, but that Great Britain would see that they were properly compensated. The Crown Prince was invited to choose, in order of preference, whatever addition to Sicily he would like instead of Naples. He might choose from this list, Poland, Lombardy, Saxony, Sardinia, Corsica, the Ionian Islands, or (oddly enough) the West Indian Islands. Thus the intentions of England were no less plain than those of Austria.

Bentinck seems by this time to have felt that something more was expected of him than writing declamatory despatches, abusing alike the cabinet of the Prince Regent, the Austrians and the French. So he made a great display of zeal and energy, resulting (as such displays mostly do) in nothing. He sailed from Palermo on the 28th of February, reached Naples on the 2nd of March and made his way by land to Leghorn, which place he reached on the 8th. Here Filangieri, a messenger from King Joachim, reached him, but he would not compromise himself, hurried on to Reggio, which he reached on the 15th, and ultimately made his way to Verona by the 22nd. Let me remind you that this is just three months after orders for the *immediate* conclusion of the treaty with Murat had been issued. On the road he favoured the cabinet with some comments on their policy. "All parties," he wrote, "agree in one view, viz. that of augmenting as much as possible Murat's power, and of uniting Italy under his standard." "A stand should be made at once against these views of ambition." Verona was the Austrian head-quarters. Here Bentinck met Bellegarde, and, after his usual fashion, made a violent attack upon his probity. "I found the Marshal anxious to believe to be true that which he knew to be false." But Bellegarde would not be bullied, and he civilly, but quite firmly, reminded Bentinck of his government's instructions to keep on good terms with Murat. To be lectured was more than Bentinck could stand from anybody, so he broke up the council of war that he had called, and betook himself to Bologna in a huff. Here he drew up instructions to Sir Robert Wilson to proceed at once to the head-quarters of the King of Naples and present his ultimatum. And here I must ask you to consider once more that Bentinck was not empowered to make an ultimatum at all: his instructions were not to seek a quarrel, but to cement a peace. The particular point that

he chose to join issue over was the occupation of Tuscany. Murat was in possession; Bentinck said that Murat ought to withdraw his army and hand over the country to England. Bellegarde said that, as a middle course, the best thing to do would be to summon the destined occupant of the Tuscan throne—the Grand Duke of Würzburg—so that neither English nor Neapolitans should occupy the country.

Murat offered to share Tuscany with Bentinck, or to allow him to occupy Via Reggio and Lucca Genoa and Pisa, thus commanding all the military roads, *or* (if Bentinck would sign the treaty) to evacuate Tuscany altogether. A more conciliatory temper it would be impossible to show.

The utter futility of the whole squabble is not realised unless we keep clearly in our minds that the object of the alliance was for both armies to get out of Tuscany as soon as possible and cross the frontier into France. But Bentinck only wanted to pick a quarrel, and he did it this time most effectually. I wish that I could read you his instructions to Sir Robert Wilson. They would show, better than any words of mine could do, that he intended the negotiation to fail. I will quote, however, two or three sentences of his secret instructions to Wilson. "I will not hear of any interference." Interference! between allies in a common cause. "An immediate decision must be the *sine quâ non* of my remaining with the British expedition." This, after three months' delay for which he alone was responsible!

With these instructions, Sir Robert Wilson interviewed the Duc de Gallo, the Foreign Minister of Naples. Gallo made the offers that I have already mentioned, and then introduced Wilson to a private audience with the King. In the midst of the interview Gallo entered with—I was going to say—a letter, but a communication from Lord William Bentinck to the King. It was written in the third person, severely lecturing the King, and couched in the most arrogant language. The King read it silently until he came to the word "disloyal," when he laid the letter down, stared at Wilson repeating the word, and then taking the letter up read it through to the end, read it a second time, handed it silently to Gallo, and signified that the audience was at an end.

The next day the Duc de Gallo sent a line to Lord William Bentinck, simply informing him that his language and bearing was not in accordance with Lord Castlereagh's instructions, and declining to hold any further communications with him. For the future, the Duke said, the Neapolitan court would communicate direct with the British cabinet. On the 2nd of April, Bentinck reported the interview to Lord Castlereagh, adding "I have resolved to be no party to a system of weak and timid policy, which, in my judgment, promises no material present advantage, and certainly none to counterbalance the dangerous effects of Murat's power and ambition." And Bentinck was drawing pay to the amount of 14,000*l.* a year for the express

purpose of carrying out that policy. That does not strike one as being conspicuously straightforward or honourable conduct.

"The negotiations having failed," he wound up, "I return to-day to Leghorn." I think it would have been more in accordance with Bentinck's "known frankness," if he had written "in spite of every possible concession on the part of the Austrians and the court of Naples, I have contrived to make the negotiations fail."

He betook himself to Palermo, gathered up his forces, despatched a small expedition under Colonel Montresor to reduce Corsica, landed on the Riviera on his own account, and on the 18th of April, Genoa surrendered to the British army.

If forgiveness be a kingly virtue, there have been few monarchs of more truly royal nature than Joachim Murat, King of Naples.

Bentinck had been Murat's evil genius from first to last. He had thwarted his grand design of unifying Italy, and condescended even to such petty impertinences as wearing the violet cockade of the Neapolitan Bourbons in Murat's presence, and punctiliously calling him Monseigneur instead of Sire or your Majesty. How did Murat revenge himself? Five days after the capture of Genoa, Murat wrote to Bentinck congratulating him on his success. He could never, he said, forget the wounding expression that Bentinck had permitted himself to use towards himself as King, but as one soldier to another he begged Bentinck's acceptance of a sword, in commemoration of the capture of Genoa. As there was not time to have one of suitable magnificence prepared, he begged Bentinck's acceptance of his own. The sword of Murat, the greatest cavalry leader that ever lived, was a present that monarchs might have coveted, a most gracious gift, most graciously bestowed. How did Bentinck receive it? I think there is no doubt that if he had not been roundly rebuked by Lord Castlereagh for his misbehaviour, he would have declined it. This is what he wrote home:

"It is a severe violence to my feelings to incur any degree of obligation to an individual whom I so entirely despise. But having hitherto adopted, according to the best of my humble judgment, a line of conduct towards that personage which your lordship has not approved, I feel it to be my duty not to betray any appearance of a spirit of animosity which can do no good, and may perhaps be interpreted by so suspicious a mind to higher authority." Suspicious is the last thing that Murat was; and as to "higher authority," Bentinck need not have been alarmed: nobody supposed that there were two men in England so rude as Lord William Bentinck.

He concluded his despatch by hoping that the Prince Regent would allow him to present him with Murat's sword as a curiosity.

I have said hard things of Lord William Bentinck. What did the Austrians say of him?

Bellegarde looked on him as a kind of lunatic, hurrying up and down Italy, for ever active and never achieving anything. Count Mier said the most damaging thing ever said of him, damaging in

its self-restraint. He said that he did not see how England could expect Italy to be pacified, unless she would send out a man who would pay some attention to his instructions. But it is not so much with Bentinck's personality that I would occupy you, as with his policy. Now the keynote of Bentinck's policy was implacable hostility to Murat because he was an adventurer, and unfaltering support of the Bourbon Ferdinand because he was a legitimate monarch. And yet, when Murat had fallen and Ferdinand was once more enthroned at Naples, Ferdinand was not grateful for a restoration which was almost entirely Bentinck's work. On the contrary, when Bentinck proposed to winter at Naples, Ferdinand conveyed to him a strong hint that he would do better to stop away. When—Bentinck-like—he braved the hint, the King sent him his passports. When Bentinck hesitated to use them, the King intimated that he would have him arrested and turned out of Naples by armed force. All that is not consistent, not natural. What explanation does the historian give of so contradictory a state of things? The most exhaustive historian of this period is an Austrian, who naturally takes the harshest view of Lord William Bentinck because he bullied Maria Caroline, of Sicily, who was an Austrian archduchess by birth. He says that if Bentinck's conduct at this epoch has the inconsequence of a lunatic's action, it is because all turns upon some secret spring of action. "Bentinck," he says, "wanted Sicily for himself. See how that explains everything. It explains that mysterious clause in the Sicilian constitution by which the complete separation of Naples from Sicily was decreed. With this in his mind, Bentinck naturally did not want to leave Murat in Naples, because that would have entailed the necessity of leaving Ferdinand in Sicily, where Bentinck wanted to rule himself. Nothing less than so great an ambition could have caused even Bentinck to deliberately violate his instructions for not merely a week or so, but for four months. Finally, it explains Ferdinand's hatred for his benefactor." It does, and most satisfactorily—if we could only bring ourselves to believe anything so outrageously incredible.

At the time when this conjecture was published, it could have been no more than a conjecture; for the papers disclosing the actual state of affairs were not accessible to the public.

My compliments to the Austrian for his insight. For, ladies and gentlemen, I present you with the astounding conclusion, that the outrageously incredible is nothing less on this occasion than the truth. To annex Sicily to England and rule the Island himself as Viceroy is precisely what Lord William Bentinck was aiming at. That, and not pious wrath, was the secret of his hatred of Murat; that, and not attachment to the cause of a legitimate sovereign, was the reason for his championing the cause of Ferdinand.

On the 5th of May, 1814, he received from Lord Castlereagh the explicit command to officially disavow to the Crown Prince of Sicily any such plan either of his own or of the British Government. In

acknowledging the receipt of his orders he poured out his usual volume of abuse of everybody concerned. In partial justification of himself, but yet with a fine inconsistency, he wrote, "Hated though Murat is, he is not so detested as the old King." "Badly as I think of the Crown Prince, I cannot believe that he has broken my confidence." "Still worse as I think of the King, I can hardly believe it even of him." In receiving Bentinck's official disclaimer the Crown Prince wrote that he had never breathed a word on the subject to any one, and that he had severely scolded Prince Castelcicala.

Prince Castelcicala, the Neapolitan ambassador, whose romantic and resounding name accords somewhat oddly with the high respectability of Great Cumberland Place, where his Embassy was, had demanded Bentinck's immediate recall as the only satisfactory protest against his iniquitous plan of buying half the kingdom to which he was accredited. In this coil it is evident that some one is telling the thing which is not. The person who was saying the thing that is not would appear to have been the Crown Prince of Sicily. The facts are as follows.

On the 3rd of December, 1813, about a month before our story opens, Lord William Bentinck had written to the Crown Prince and laid before him the plan of surrendering Sicily to England. Sicily, he wrote, had never paid Naples; the island could not rule itself, and would not consent to be ruled by Naples. England was the only power who could manage the government of Sicily. As to compensation, why, money was no object. Or, if territory was preferred, perhaps King Ferdinand would like the States of the Church. England could have no objection to his taking them. Perhaps not: but Ferdinand might have some objection to accepting them. All serious adjectives are out of place when applied to that incomparable fribble; but the least flighty part of his character was, perhaps, his attachment to the Church. So that, apart from the unprincipled nature of the communication, I know not which to marvel at most, the brutality of offering to place the King of Sicily on the Pension List of the Treasury, or the ineptitude of proposing to dower an ardent Catholic with the plunder of the Holy See. The Crown Prince replied guardedly, and made some allusions to Bentinck's instructions. "Instructions?" Bentinck rejoined, "he had none:" the Crown Prince must not give the proposal a second thought. It was only "the phantasm of his own disordered brain," a "sogno filosofico," a "castle in Spain," "le rêve d'un voyageur."

From the way the correspondence runs it appears to me plain that the Crown Prince did not believe Bentinck when he said that he had no instructions and was acting on his own initiative. He gave the question a week's thought, and then transmitted copies of the correspondence to Castelcicala; who acted as we have seen, adding dry comments. In the unparalleled circumstances, he said, of an ambassador proposing to buy the country to which he was accredited, and doing so without his sovereign's instructions, it was not sufficient for

him to say that the idea was only a philosophic dream. If Lord William Bentinck, he added, is subject to dreams of this kind he is not a fit person to be accredited to my master's court. His demand for Bentinck's recall was not acceded to; but Bentinck soon after resigned his post, and so passes from our history, where he figures as Murat's evil genius. In that capacity he was succeeded by Louis Philippe, who was even now hastening to Paris, and whom we must follow in his efforts to overthrow the last Bonaparte throne left in Europe.

For we have now arrived at June 1814; the Emperor is installed at Elba, and Louis XVIII. is on the throne of France. The first rumours of the Congress of Vienna are in the air, and the watchwords of that Congress are to be Legitimacy and Restoration. Hence the extremely awkward position of the Allied Powers with regard to Murat, who certainly was not a legitimate monarch in this sense, and at whose gates there resided a legitimate monarch in the person of Ferdinand of Sicily, who claimed to be also Ferdinand of Naples. Nevertheless the most ardent champion of legitimacy, the Emperor of Austria, had in fact recognised Murat, and had undertaken to engage England to recognise him also. These promises had been made under the stress of military exigencies, as I have endeavoured to make plain. But Austria was loyal to them; and it seemed that Murat was to be made the solitary exception to the rule "Legitimacy and Restoration," and that one Bonaparte kingdom would survive the general wreck. Thus all that Bentinck had achieved by his perfidy and disobedience was to postpone the fulfilment of Murat's dream. We shall see this if we follow Louis Philippe through his interviews with various notables throughout the year 1814.

Louis Philippe, Duke of Orleans, had married, under the protection of British ships and bayonets, Maria Amelia, daughter of Maria Caroline, Queen of Sicily, and Ferdinand her husband. He was destined to seek the same protection for himself and his aged wife in their flight from France in 1848, and to die, as he had wedded, in an island—exile, and under the British flag. He now betook himself to Paris in order to do the best he could for his father-in-law, and to overturn, if possible, the throne of Murat. He met with a cold reception. First, the Emperor of Austria: "Tell your father-in-law that he must give up all idea of returning to Naples. It is out of the question for him to think of it." The Emperor of Russia was even more firm: "Tell your father-in-law that peoples are no longer to be ruled by holding out a hand to be kissed. Unless he can make up his mind to a really liberal and constitutional form of government, he must give up all idea of regaining the kingdom of Naples."

Seeing that Ferdinand was at this moment occupied in plundering and persecuting every upholder of the constitution who had not already fled the country, the Emperor's words were not very encouraging. But the vanity and tenacity of Ferdinand were of that colossal stamp that almost exalts petty failings into greatness. On

hearing of the Russian Emperor's advice, he said: "The Emperor knows nothing about it. My return is longed for as if I were the Messiah. As for constitutions, why doesn't the Emperor grant one to Russia, since he is so ready with his advice to me?"

Brave words; but words brought him no nearer to moving Murat. Murat, a fiery and impulsive man, was playing his game with great skill. He merely sat steady under his treaty obligations, and called upon the contracting powers to fill theirs.

Louis Philippe now approached Louis XVIII. Surely his kinsman the King of France would help him. Perhaps the son of Egalité Orléans was not a very welcome figure to the brother of Louis XVI. Anyhow the King of France received him with reserve. King Ferdinand, he said, had all his sympathy, and he would instruct M. de Talleyrand to urge legitimacy and restoration at the Congress of Vienna with all possible force. He even went so far as to say that he would never recognise Murat himself. There was an amusing passage of arms between the two monarchs at about this period. The official gazetteer, when it appeared, contained—in accordance with this resolve of Louis XVIII.—the following entry:

Naples, see Sicily, kingdom of.

Murat, not to be behindhand, published the official gazetteer of Naples with this entry:

France, see Elba, island of.

All of which brave doings brought Louis Philippe no nearer to turning Murat off the throne of Naples.

Baffled in Paris, he now turned to London, and craved from Louis XVIII. a line of introduction to the Prince Regent. "No," said the King, "I can't do that; the Prince would show the letter to his ministers, and it would become an official document, but you may give H.R.H. this message. Ask him if he remembers that Knight of the Garter whom he received sitting." This was all the letter of introduction that Louis Philippe brought to London. It seems to have reference to some incident for which the Prince Regent owed reparation, for he received the Duke graciously enough. But he held out no more hope than the other kings. "Your father-in-law has played his cards badly." "Votre beau-père a mal mené sa barque," he said. "Our engagements with Murat must be maintained." "England has no engagements with Murat," said the Duke. But the Prince was silent, and then he added, "I can't think what the Allies meant by stuffing Napoleon into the Island of Elba, just outside Murat's gates." This was a most unpleasant line for the Prince's thoughts to take, for it led to the conclusion that if another exile were found for Napoleon, Murat would do no harm where he was. So the Duke hastened to turn the conversation: "Let your Royal Highness put yourself at the head of the movement," he said, "and do for Naples what you have already done for France."

On this appeal, vague and grandiose, the Prince Regent shook hands with the Duke, and rang his bell for Lord Liverpool and

Lord Castlereagh, who were in attendance. He presented these nobles to the Duke, and referred the matter to them, glad to escape unpledged from so tenacious a negotiator.

Lord Castlereagh had a cold; a bad cold; a very bad cold indeed. Lord Castlereagh was deeply grieved at being unable to pay his respects to His Royal Highness the Duc d'Orleans. He was most distressed at being unable even to receive His Royal Highness in bed. The fact was that Lord Castlereagh was going to Vienna in the Autumn, and had no mind to discuss the situation with this pertinacious young man. Lord Liverpool, however, was not going to Vienna, and was not of an anxious temper. He had a long interview with the Duc d'Orleans, and took the best step towards making matters clear by saying at once—

Firstly, that Austria is bound to Murat;

Secondly, that England and Russia, having had notice of the treaty, and having approved of it, were equally bound, and that it was useless for the Duke to deny the fact: a fact it remained;

But thirdly (and I think this must have been ironical), France and Spain remained unpledged, and might do what they liked in the matter.

The Duke fenced a little, but Lord Liverpool drove his conclusions home. If his advice were asked, he said, he would not recommend the alliance of two Bourbon kings, with the object of restoring a third; that a French army entering Italy would produce a very bad impression; and that if Louis XVIII. allied himself with Ferdinand in order to attack Murat, of course the feeling of England towards Sicily would undergo a considerable change. There was a marked menace in the last warning, and Louis Philippe shifted his ground again. "Confess, my Lord," he said, "that you hum and haw because you are all afraid of Murat." Lord Liverpool laughed, there was something in that. "But how would your Royal Highness set to work if you wanted to get rid of Murat"? "I would set Lord William Bentinck at him," said Louis Philippe boldly. Whereat Lord Liverpool grew very grave: Lord William, he said, had been far too hasty with Murat, and had given him very just grounds of complaint. So far Louis Philippe had not scored a point, and now Lord Liverpool tried to reason him out of his position. Even if we turned out Murat, he argued, there was no compensation possible for him; there was no other throne that we could offer. "Why a throne? then why not money"? "By all means, if he would take it." "Oh, he would take it fast enough if the British fleet were in the Bay of Naples." "But then who is to pay it"? "Why of course, my Lord, those powers who have guaranteed Murat's throne." That was the only point that Louis Philippe scored off Lord Liverpool. He now waited on Prince Metternich, and opened up with his remark that Murat was not to be depended on. But then, rejoined Metternich, no more is your father-in-law, you must wait for the Congress. The Duc d'Orleans had been so pertinacious that Lord Castlereagh's cold

had had time to recover, and the Duke, encouraged perhaps by the incident, interviewed him and pressed for an immediate decision. But Lord Castlereagh was not so easily squeezable as Louis Philippe imagined. An immediate decision is quite out of the question, he said; "your Royal Highness must wait, like all of us, for the Congress."

"Je ne pus rien gagner," he sighed.

And yet, at the moment when he was complaining that he could make no way, he had in fact won his cause. Ferdinand, by himself, was a negligible quantity in his own cause. The sovereigns of Europe held him as a incumbrance in their cause. They were fighting the cause of monarchy, and he was a disgrace to the cause of monarchy. They were fighting the cause of legitimacy, and Ferdinand was the incarnation of all the qualities that made the word legitimacy an abomination in the ears of the peoples.

If it had not been for Bentinck and Louis Philippe, Ferdinand would never have returned to Naples.

Bentinck's conduct was highly improper, but, as a matter of fact, it did prevent the definite recognition of Murat. Louis Philippe's adroitness and pertinacity produced the general impression that Murat was rather a nuisance than otherwise. The result was that when the Dukes of Gallo and Campochiaro claimed admittance to the Congress of Vienna as Murat's representatives, it was refused to them.

Talleyrand, the plenipotentiary of Louis XVIII., tried to push his advantage further. But Metternich was firm. "I will never," he said, "advise my master to repudiate the treaty with Murat. It was made in an hour of stress when we had need of his help, and I will be no party to repudiating it now. But," he added, "you know Murat's temper. He has so far exhibited great self-restraint. Sooner or later he will make a slip, and we shall profit by that."

I am glad that my time has drawn so near to its close, and that I can do no more than hurry through the last year of Murat's life. Prince Metternich was quite right, Murat did make a slip, and the Austrians did take advantage of it. They entered his territory, he was defeated in battle and fled. Ferdinand, the Messiah as he called himself, returned to his faithful Neapolitans, and Murat wandered in exile. His private fortune of twelve millions of francs had been spent in maintaining the royal state of Naples. All that he carried into exile with him was a handful of gold pieces and some diamonds.

At last, when at the end of his resources, there came a helping hand from Austria. The Emperor created him Count of Lipona, and granted him a passport to Austrian dominions: doubtless a provision would have followed. It came too late. That very morning he had completed his preparations for a last desperate attempt. "The die is cast," he cried, as with the patent of Count of Lipona in his pocket, he set sail for Calabria, bent on a struggle for the throne of Naples. He had miscalculated. There was no rising in his favour. He was taken prisoner, tried by a Court Martial, of which nearly

every member had been decorated by his own hands, condemned and shot.

"As an act of justice or an act of policy his punishment is equally to be justified," wrote Bentinck's successor, as a comment on the tragedy. Perhaps: only when one remembers 1848 and 1859, 1866 and 1870, when one remembers the long agony through which Italy had to pass before she attained that measure of unity that Murat was endeavouring to win for her in 1815, our only consolation for Murat's death must be the reflection that the Red Cross of Savoy now waves over the Peninsula from end to end.*

* The discourse was illustrated by four maps.

[W. F. L.]

WEEKLY EVENING MEETING,

Friday, March 18, 1898.

Sir Frederick Bramwell, Bart. D.C.L. LL.D. F.R.S.
Hon. Secretary and Vice-President, in the Chair.

James Mansergh, Esq. V.P. Inst. C.E. F.G.S. *M.R.I.*

The Bringing of Water to Birmingham from the Welsh Mountains.

The city of Birmingham has an area of 12,365 acres; and the parliamentary limits within which the Corporation are bound to supply water extend to 83,221 acres, or 130 square miles—an area 10 per cent. in excess of that of the County of London. This district varies considerably in elevation, being 270 feet above sea level in the north-east corner, and rising to 800 feet in the south-west. As compared with this, the highest part of Hampstead Heath, in the north-west of London, is 450 feet. The population within the limits at the time of census taking in 1891 was 647,972, and is believed to be now over 700,000. The water is at present obtained from five local streams, and from six wells sunk in the New Red Sandstone which underlies the city and its neighbourhood.

In 1890 I was called in to investigate the whole question of the future of the water undertaking. My advice to the committee, put shortly, was—

1. That the water obtainable from the local streams, flowing as they do through populous districts, would go on constantly increasing in impurity, and the greatest care would have to be exercised in order to ensure its safety for domestic use.

2. That the addition to their resources by any impounding works which could be constructed on these streams, or by sinking more wells, would carry them on for only a comparatively few years, at the end of which time they would inevitably have to go much further afield, and the money they had spent would be practically lost.

3. That the distant unpolluted sources, at sufficient elevation to supply Birmingham by gravitation, were comparatively few, and that if their acquisition were delayed even for a few years only, the chances were that they would have been secured by some other community, possibly London.

This advice was accepted, the result being that a Bill was promoted in Parliament in the Session of 1892, by which the Corporation sought powers to utilise the waters of the rivers Elan and Claerwen flowing from an area of 71 square miles in the counties

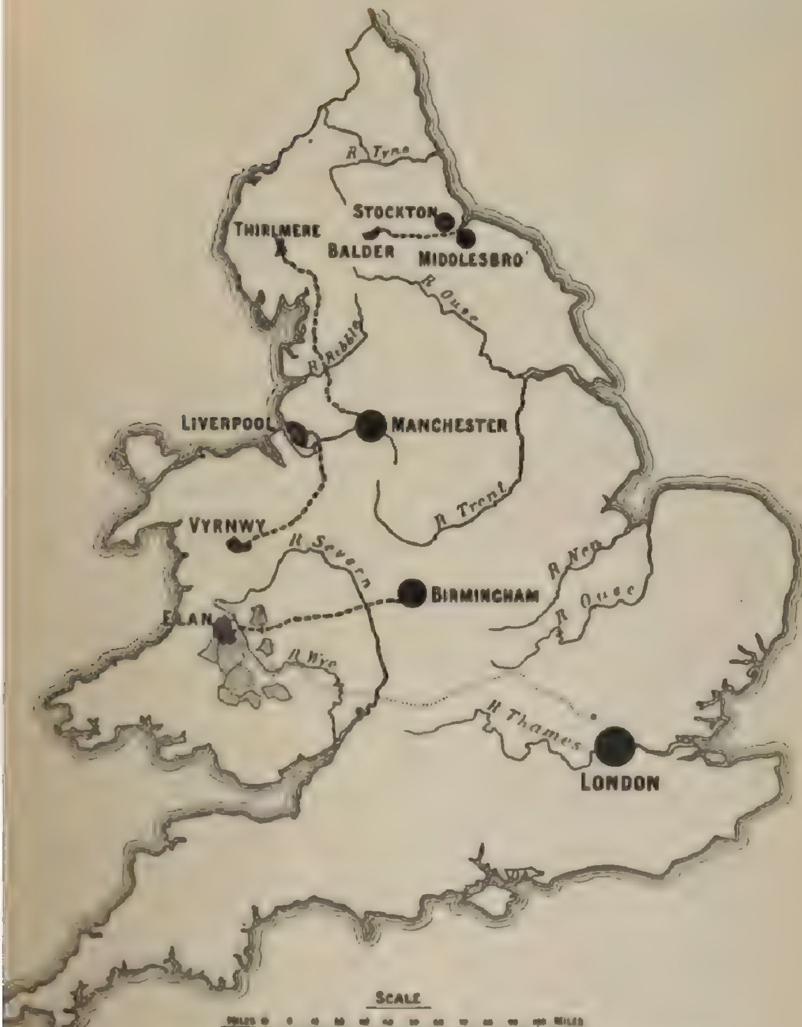
of Radnor, Brecon and Cardigan. These rivers are tributaries of the Wye, which, passing through Radnor, Brecon, Hereford, Monmouth and Gloucester, joins the Severn near Chepstow.

Diagram No. 1, being a map of England, shows the relative positions of Birmingham and the Elan shed, with the aqueduct (80 miles). It also shows the Stockton and Middlesbrough (35 miles), the Manchester Thirlmere (100 miles), and the Liverpool Vyrnwy (66 miles), schemes all executed; and in addition the Welsh scheme for London (170 miles), projected by my friend Sir Alexander Binnie.

In order to obtain complete control of the drainage area, and thus secure the water from pollution, the Corporation asked Parliament to allow them to acquire the whole of it by purchase, a proposition which induced the opposition of the landowners, the Commoners and the Commons Preservation Society. The Bill was also opposed by a number of property owners upon the line of aqueduct, by a small section of Birmingham ratepayers, by the Corporation of Hereford, and by the London County Council; the ground of the last-mentioned opposition being that the source of supply was an exceptionally good one, that therefore the Council might some day like to get hold of it, and that Birmingham ought to wait until London had made up its mind. We were most effectively assisted in combating this opposition by your worthy Honorary Secretary, Sir Frederick Bramwell, who had been engaged in the Liverpool fight twelve years previously, and was able to testify that a similar objection was made at that time by the Metropolitan Board of Works to the taking of the waters of the Vyrnwy to the great Lancashire seaport, and to show that the London water question was no further advanced in 1892 than it was in 1880. This London contention was met by setting out in detail the many streams in the Welsh mountains which were available for the Metropolis, but too low for Birmingham; streams which, when provided with proper storage reservoirs, were competent to supply nearly 500 million gallons a day without touching the Elan and Claerwen.

In addition to these oppositions we had of course to fight—as happens in all water Bills of this class—the question of the amount of compensation water to be paid to the river for the right to divert the water authorised to be taken for the supply of Birmingham. In the case of works established upon the rivers of Lancashire and Yorkshire, whose waters are utilised for manufacturing purposes nearly up to their sources, this is a serious question, but fortunately in the whole course of the Wye, and the Elan below the point of abstraction, there is not a single case of such utilisation even for driving the wheel of a corn mill. This did not, however, prevent most exorbitant claims being set up by riparian owners on account of their fishing rights—not, however, by the net-fishers in the lower reaches who make their livelihood out of the fishing, but by sportsmen who handle a rod for diversion. In the Bill as deposited we had proposed that the quantity of compensation water should be $2\frac{1}{2}$

DIAGRAM No. 1.



Map of England and Wales, showing the Manchester Thirlmere, Liverpool Vyrnwy, Stockton and Middlesborough, and the Birmingham Elan Schemes; also the Proposed Welsh Scheme for London.

millions of gallons per day: the rod fishers demanded *forty millio* They were assisted by the Wye Fishery Board, and, in the bac ground, by the officials of the Board of Trade who administer t Salmon Fisheries Acts; and ultimately a compromise was come to which the quantity was fixed at 27 million gallons a day. Since t works have been in course of construction we have had the oppo tunity of measuring the flow of the river at the spot where t 27 millions will have to be discharged, and have found that in ve dry weather it falls to something under 4½ millions, so that t quantity passing down will, so soon as any water is taken Birmingham, be increased at such point *six-fold*. Of course t capability of so benefiting the river is due to the storing of flo waters in the reservoirs to be constructed.

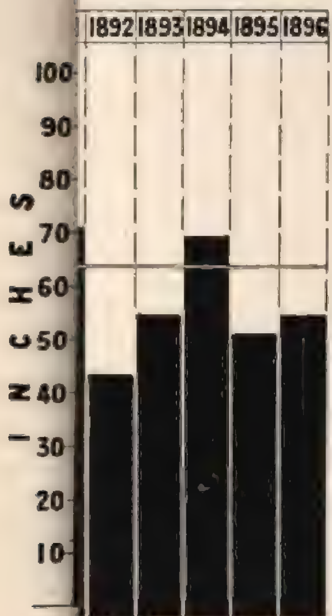
Another incidental benefit arising out of this impounding will the reduction in the volume and violence of destructive floods in t river below. The amount of compensation water in these cases is fairly well recognised proportion of the water collectable from t watershed area, that is to say, where the water is used for trade manufacturing purposes the proportion is *one-third*, and where the are only ordinary riparian—including fishing rights—it is about o *fourth*. The quantity of water collectable is ascertained from the si of the gathering ground and the rainfall upon it less the evaporati and the volume of water inevitably overflowing from the reservoirs times of flood. Thus the area we are here dealing with, was dete mined by accurately marking upon the plans the parting lines watershed boundaries after careful examination, and in some ca instrumental levelling upon the ground. By measurement from t plans the area was found to be 45,562 acres, the first factor in t calculation. The area is shown by a photograph of a model of t watershed (Diagram No. 2).

The model was made on a scale of 6 inches to a mile, that 880 feet to an inch *horizontal*, and 300 feet vertical, and upon it t reservoirs are represented as made and filled with water. The rai fall might have been a much more difficult thing to determine th the area, *but* that very fortunately the Lord of the Manor, Mr. Rob Lewis Lloyd, and his father before him had kept a rain-gauge regu larly from the year 1871 onwards, at the family mansion of Nan gwillt, in the lower part of the Elan Valley, and at a spot on t watershed area to be appropriated.

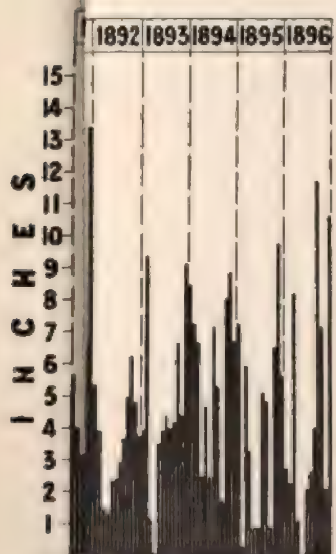
So soon as it seemed probable that the matter would be proceed with, several other rain-gauges were erected at several points up the shed, with the assistance of Mr. Symons, the last, and a mo worthy gold medallist of the Society of Arts. Then, by a comparis of these with the long-term gauge at Nant-gwillt and others in th surrounding country, it was decided that the mean annual fall of long series of years upon the watershed might be taken at abo 68 inches, and the average of three consecutive dry years at 55 inche —this latter being the figure always used in these estimations—

DIAGRAM No. 2.

DIAGRAM No. 3.



1892
1893
1894
1895
1896
INCHES
100
90
80
70
60
50
40
30
20
10



1892
1893
1894
1895
1896
INCHES
15
14
13
12
11
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9
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7
6
5
4
3
2
1

first suggested by the veteran waterworks engineer and hydrologist, the late Mr. Hawksley. The greatest rainfall was in 1872, viz. 93·86 inches; and the least was in 1892, viz. 43·44 inches.

Diagram No. 3 shows the rainfall. The upper one gives the yearly fall at Nant-gwillt from 1871 to 1896, with the mean of that term, and also the mean of the lowest three consecutive dry years. The lower one shows the monthly fall at the same place for the same period. It is very usual to take 14 inches as the amount of evaporation, but, in order to be on the safe side and to allow amply for loss by overflow, 19 inches were deducted from the 55, leaving 36 as collectable by means of the reservoirs intended to be constructed. Taking the mean of three consecutive dry years, the rainfall in one year upon the watershed area would be equivalent to 252,495,491 tons of water, of which 63,950,823 would be lost by absorption or evaporaiton, and 23,154,608 tons by overflow, leaving 165,390,060 tons as collectable in the reservoirs. In a year of maximum rain like 1872, the total quantity falling upon the watershed would be 431,116,756 tons, and the volume overflowing from the reservoirs into the river would be correspondingly increased. Further observations since the Bill was in Parliament have satisfied me that we may calculate on obtaining from the works 75 million gallons a day for supply, in addition to the 27 millions for compensation.

Considered geologically the whole of the watershed area consists of rocks of Lower Silurian age, principally inferior slates, but in parts of very hard grits and conglomerates. It is the presence of thick bands of the latter stretching across the Elan, at a place called Caban Côch, and resisting degradation, which has determined the position of the contraction in the sides of the valley and rendered it so eminently suitable for the location of a barrier dam. At this spot the bed of the river is 700 feet above Ordnance datum, the bottom of the valley being about 200 feet wide, and at 120 feet higher, only 600 feet. Immediately above this contraction the valley widens out into a broad "*flat*," and 1540 yards higher up, the river Claerwen joins the Elan on its right bank.

These conditions pointed unmistakably to the Caban as the site of the lowest *dam*, and consequently determined the area of gathering ground to be utilised.

The height of the wall to be built was after much consideration fixed at 122 feet above the bed of the river, and the contents of the reservoir behind it will be nearly 8000 million gallons. As compared with the height of this wall above the river, the Vyrnwy (Liverpool works) is 85 feet, and the Thirlmere (Manchester works) 50 feet. The river Elan has in the part affected by this dam a rise of 30 feet in a mile, so that the 122-feet barrier backs the water up that valley 4 miles, and up the Claerwen, which is somewhat steeper, about 2½ miles. The length of drought which it was deemed advisable to guard against was fixed at 180 days, and consequently the total storage to be provided was nearly 18,000 million gallons, or 10,000

millions more than the Caban Côch reservoir would contain. For the purpose of selecting the positions of other reservoirs than the Caban higher up, the two valleys were levelled and closely contoured to above the highest possible site on each, and by this means the exact positions of five others were determined, *giving the greatest impounding capacity with the least amount of structural work.*

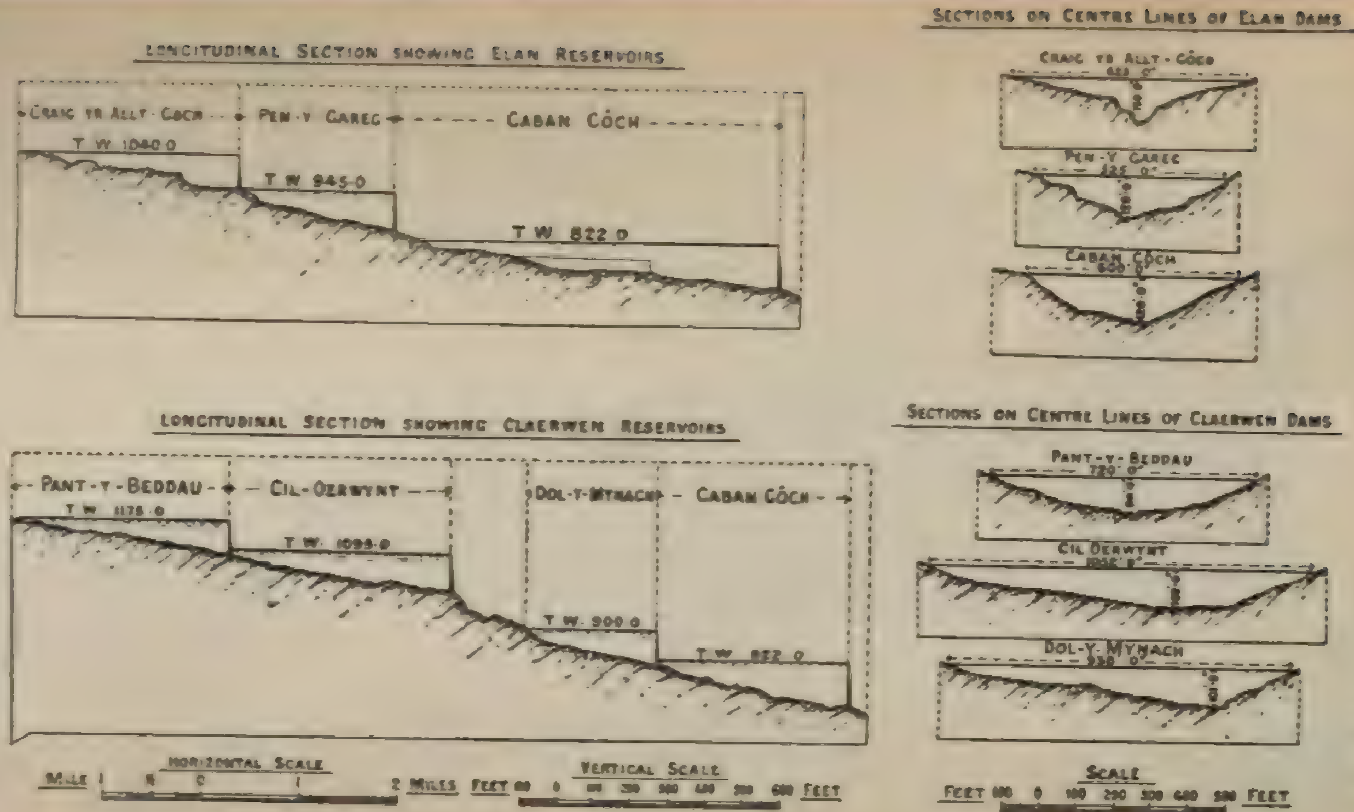
On Diagram No. 4 are given longitudinal sections of the two valleys, showing that on the Elan, above the Caban, there is to be a dam at Pen-gareg, and another at Craig-yr-allt-goch, and on the Claerwen at Dol-y-mynach, Cil-oerwynt and Pant-y-beddau. In the order named, their respective heights are 128 feet, 120, 101, 108, and 98; and the reservoir capacities 1330, 2000, 1680, 8150, and 1940 million gallons respectively.

A unique feature in the scheme is the provision of what has been called a *submerged dam*, to be built across the Caban Côch reservoir at a point nearly a mile and a half above the main wall, and called Caregddu, its precise function being to hold the water up behind it high enough to charge the aqueduct conveying the water to Birmingham. It is described as submerged, because until the water has been lowered 40 feet it will be drowned and out of sight. The necessity for this device comes about in the following way, viz. at the Birmingham end of the aqueduct the water is to be delivered into a large service reservoir at Frankley, about 6 miles from the centre of the city, whose top water will be 603 feet above O.D. From the commencement of the aqueduct in the side of the Caban reservoir to Frankley is a distance of nearly 74 miles, and in this length the fall required to convey the water is in round figures 170 feet, so that the invert of the aqueduct at its inlet will be 770 feet above O.D. or 70 feet higher than the bed of the river at Caban Côch. Now the water must of necessity never fall below this inlet, or the aqueduct could not be charged, and therefore the submerged dam is to have its crest at 782 O.D. being high enough to fill the aqueduct; the cross section of which will be described later on.

Diagram No. 5 explains this more fully. A is the main Caban wall, built at a spot where the bed of the river is, as before stated, 700 feet above O.D., its crest being 822; B is the submerged wall, with a crest level of 782; C is the entrance to the aqueduct, with its invert level at 770.

When the reservoir is full, the layer of water between 822 and 782, 40 feet thick and having a surface area of 500 acres, contains 4585 million gallons. Now suppose very little water were coming down the rivers in a time of great drought, 27 million gallons have still to be sent out for compensation every day at A, and dealing with the first instalment, another 27 millions have to go down the aqueduct to Birmingham; then this combined draught of 54 millions would draw down the water from 822 to 782 in about 80 days. The quantity of water below 782 between Caban Côch at A and Carregddu at B is 2565 million gallons, and would therefore suffice to pay the com-

DIAGRAM No 4.



LONGITUDINAL SECTION SHOWING ELAN RESERVOIRS
CRAIG YR ALLT GOCH
PEN-Y GAREG
CABAN CÔCH
T W 1040 0
T W 945 0
T W 822 0
SECTIONS ON CENTRE LINES OF ELAN DAMS
CRAIG YR ALLT-GOCH
PEN-Y GAREG
CABAN CÔCH
600' 0"
LONGITUDINAL SECTION SHOWING CLAERWEN RESERVOIRS
PANT-Y-BEDDAU
CIL-OERWYNT
DOL-Y-MYNACH
CABAN CÔCH
T W 1175 0
T W 1095 0
T W 900 0
T W 822 0
SECTIONS ON CENTRE LINES OF CLAERWEN DAMS
PANT-Y-BEDDAU
720' 0"
CIL OERWYNT
DOL-Y-MYNACH
HORIZONTAL SCALE
MILE
2 MILES
VERTICAL SCALE
FEET
FEET
SCALE
FEET
FEET

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CABA DAM.

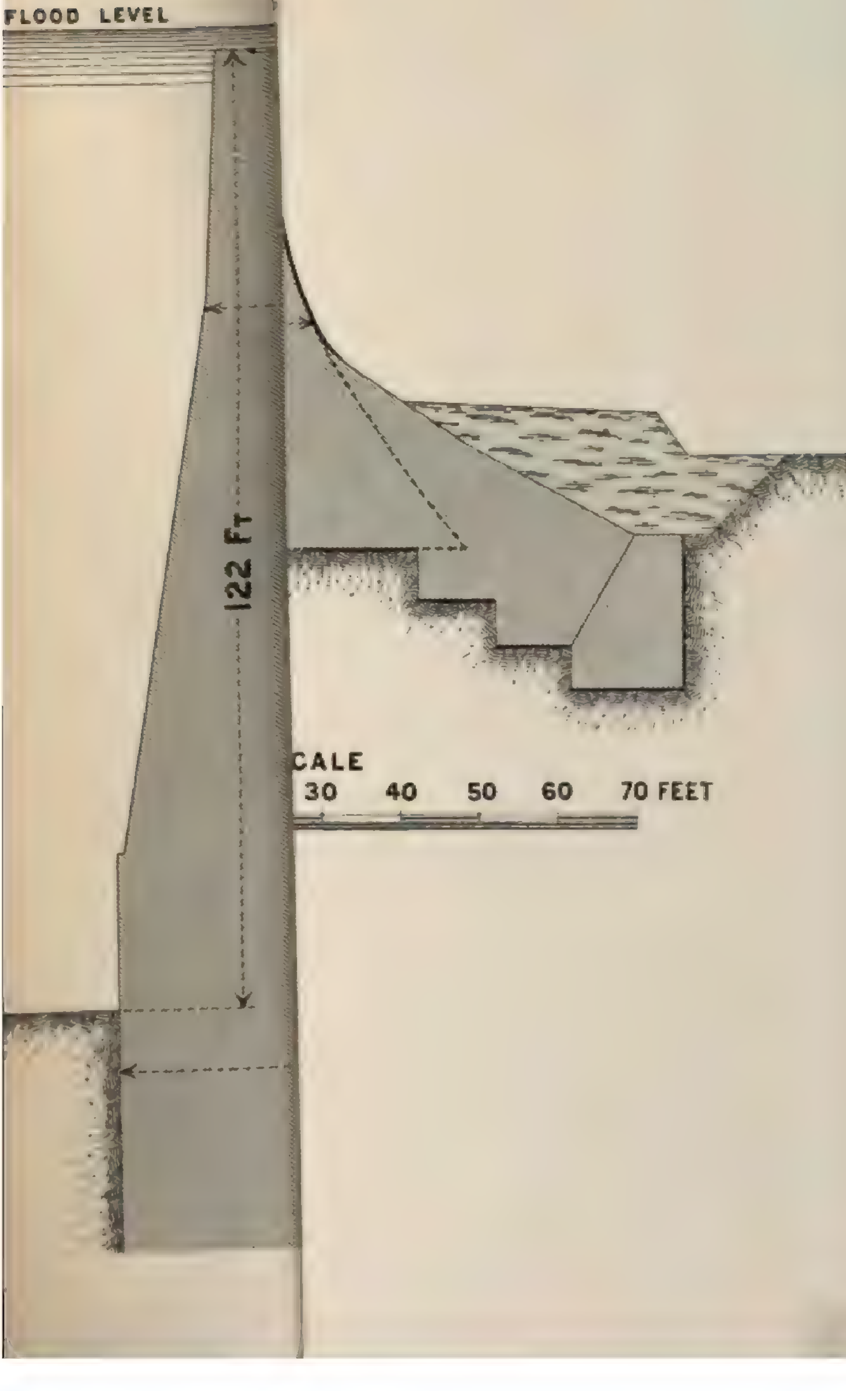
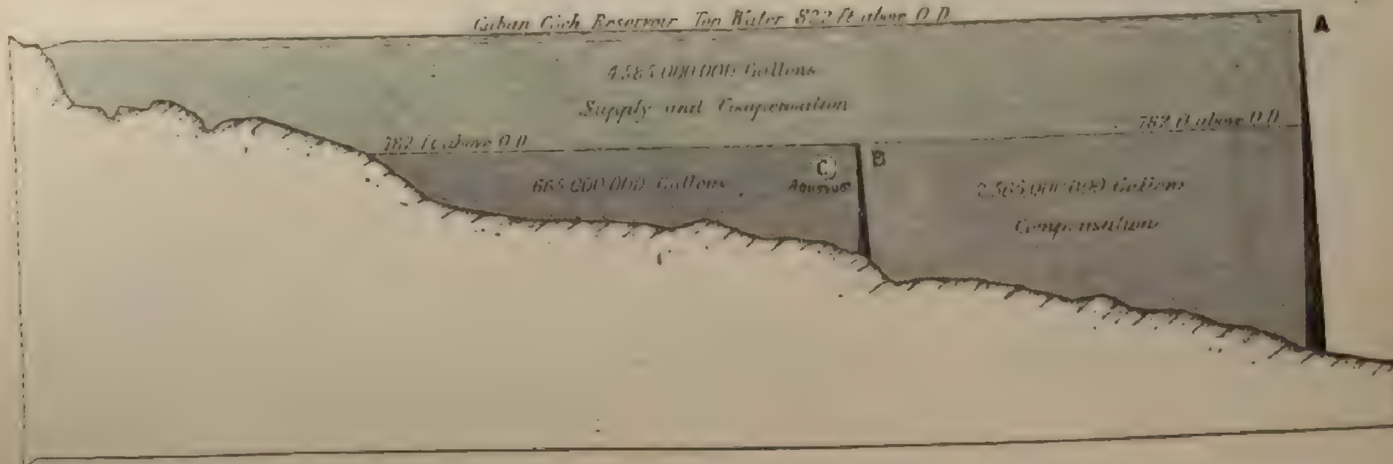
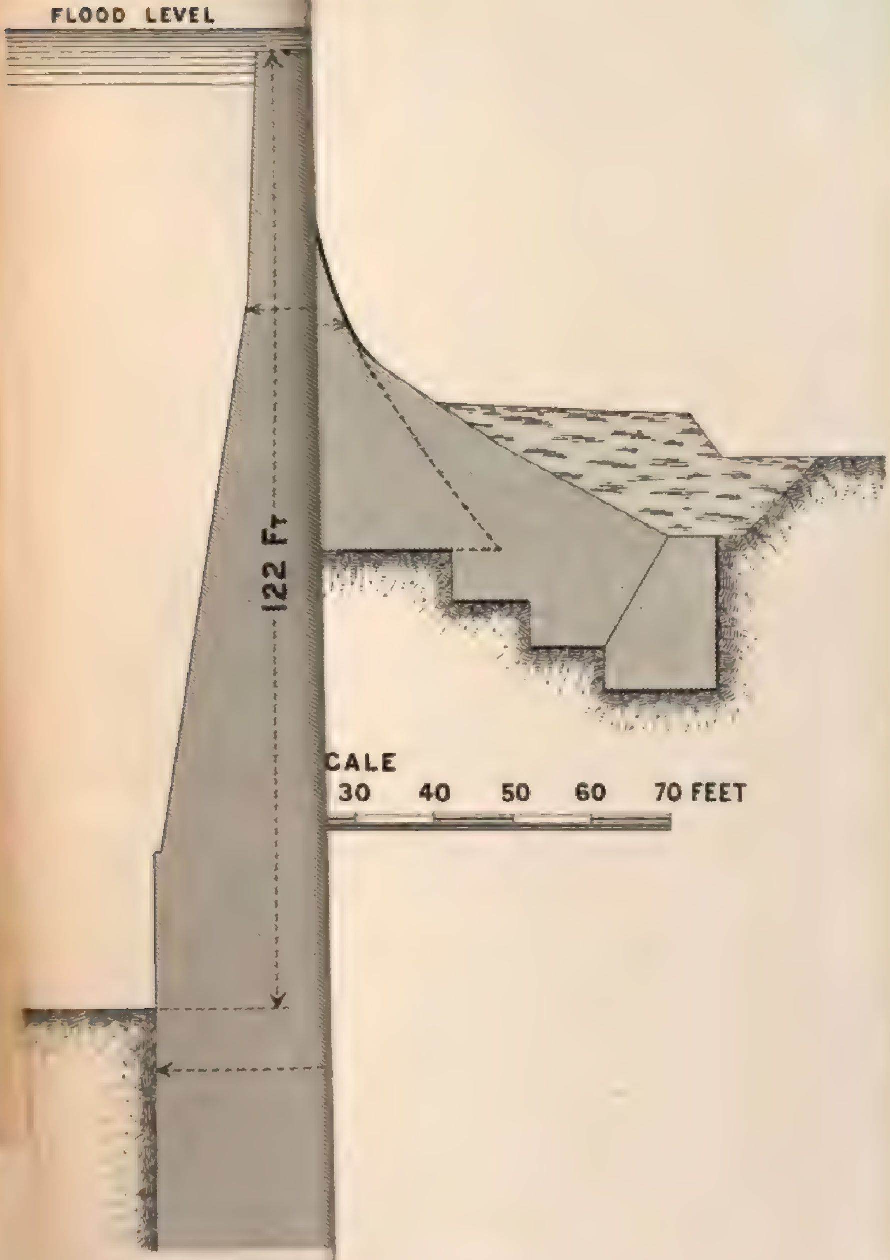


DIAGRAM No. 5.

DIAGRAM SHOWING FUNCTION OF SUBMERGED DAM



CABA DAM.



pensation water for another 100 days. In this way a drought of 180 days is provided for, the water for supply during the 100 days coming from the Pen-gareg and Craig-gôch reservoirs, higher up the Elan. They hold together 3330 million gallons, and are therefore fully competent to ensure this.

The water darkly shaded on the diagram above the submerged dam and below 782, cannot of course be counted as effective storage, as it cannot be drawn down without leaving the aqueduct inlet high and dry, but it will of course be in no sense stagnant, because the quantity going to Birmingham must always be running through it. When the second and following instalments are required for supply, the reservoirs on the Claerwen will have to be made in succession as required, and the addition of the water obtainable from them will enable the 40 feet "slice" between 822 and 782, which they will always be repleting, to maintain the increased delivery by way of the aqueduct and the compensation as before, leaving the 2565 millions below 782 for the last 100 days of the drought. In order to delay as long as possible the making of the Claerwen reservoirs, a tunnel 1¼ mile long is to be driven from the Dol-y-mynach reservoir on that river to above the submerged dam, so that its *natural unstored* waters can be used for supply, the respective levels at each end admitting of this being done comfortably.

In this country hundreds of impounding reservoirs have been constructed for the storage of water for canal purposes and for town supply, and a very large majority of these have banks of earth supporting an internal wall of puddled clay, which forms the watertight part of the barrier.

There are still only very few stone dams of any great size in England, although many are to be found on the Continent of Europe. The Elan and Claerwen valleys were, however, peculiarly adapted for such structures, the dam sites being all on rock practically to the surface, and plenty of stone for building at no great distance, the material for earth banks being, on the other hand, deficient.

It may be interesting to show a cross-section of one of these stone dams, and on Diagram No. 6 you have the Caban Côch which we are now building, and alongside it that of the Bouzey dam, near Epinal, in France, which failed about three years ago with very disastrous consequences. I invite you to compare these two profiles, and note the relative thickness of the walls at the same depth below the water surface, which, of course, determines the pressure. In this dam (Bouzey) the line of stress, instead of falling within the middle third of the profile, as it ought to do, was very much nearer the down-stream face at the point of failure; the weight of the structure was under 130 lbs. per cube foot, and neither the stone nor the mortar of which it was built was of good quality. The failure was no doubt due to the fact that when the reservoir was full the water face of the wall at the point of fracture, owing to the improper form of cross-section, was subjected to a tensile strain which the material was not competent to bear.

This strain Professor Unwin has calculated at a ton and a quarter p square foot, which was sufficient to make a horizontal tear or re along the back of the wall. Once this was made the water would ent it, and, acting upwards as a wedge, widen the rent and ultimatel overturn the part of the wall above, cutting it right across verticall at each end of the disturbed part, a length of about 190 yards.

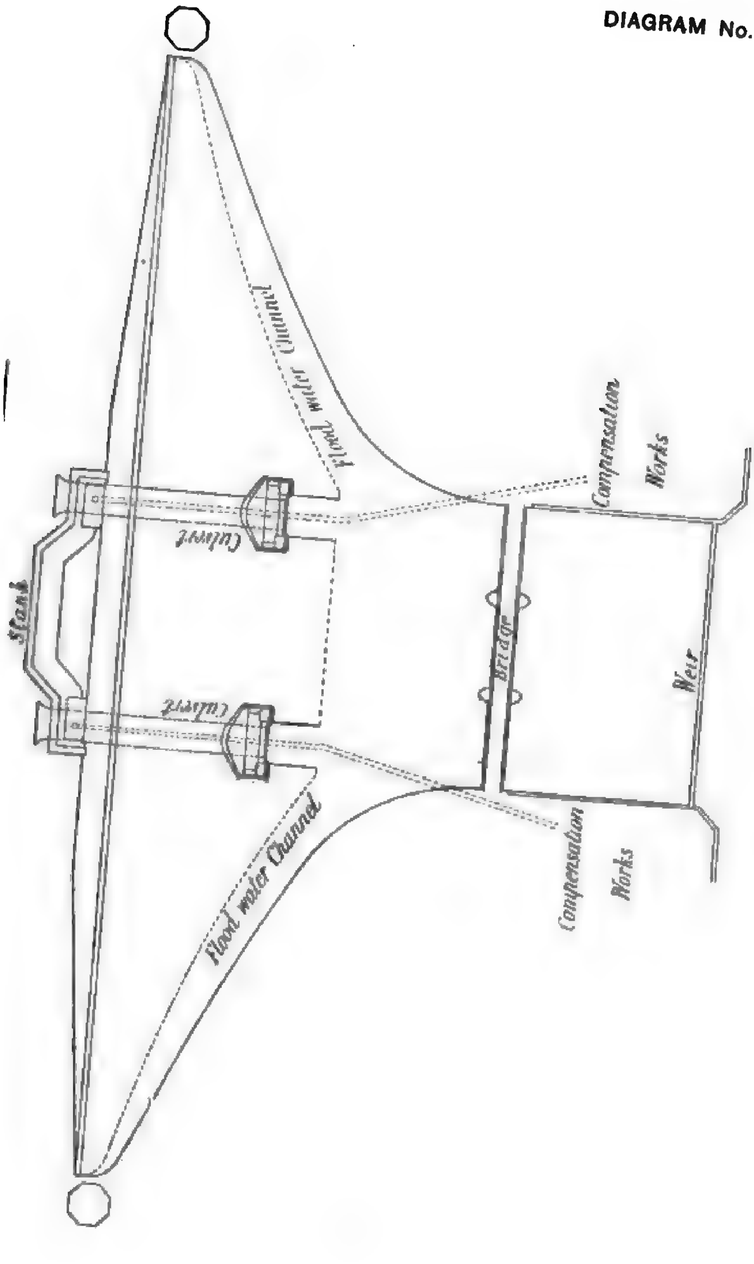
The structure of all the walls in the Elan Valley will be identic in character; they are being formed of blocks of stone (*plums* as th men call them) practically unhewn, varying from 5 or 6 cwt. to many tons in weight, built so as to avoid horizontal bedding plane but with good vertical bonding, and embedded in and surrounded b a matrix of high-class Portland cement concrete. Both the *up* an *down* stream faces are being finished with heavy broken-coursed an rock-faced grit or conglomerate blocks closely jointed. The ston weighs about 170 lbs. per cube foot and the concrete about 146, an we are aiming at getting a little more than half the mass of *plums*, that the finished weight of the dams shall be as nearly as possibl 160 lbs. per cube foot. The design of the walls is such that n effective tensile strain can ever come upon their water faces, but if i did, the structures as put together will resist a tensile strain of a least 12 tons per square foot. When the Caban reservoir is full th total water pressure against the exposed face of the dam will be abou 60,000 tons. The work is being so built that there shall be n interstices in it, and that each dam when finished shall be to al intents and purposes a monolith, only removable by some great con vulsion of nature. Without reckoning anything for the cohesibility of the structure, but only considering the *weight*, the factor of safety against overturning (as did the Bouzey) is from 3½ to 4.

The drainage area above Caban Côch is by far the largest tha has been hitherto dealt with in this country in constructing works o this character. Deducting the reservoirs, the Manchester Thirlmer area is 11,000 acres, the Liverpool Vyrnwy 22,000, and this is 44,00 The provision to be made for passing flood waters during the execu tion of the works is consequently a very important matter. At th Caban it is quite within the range of probability that at the ver height of a flood 700,000 cube feet a minute may have to be deal with.

Diagram No. 7 is an outline drawing showing the way in which we are arranging for the passage of such a flood during constructio and how it will be disposed of when it comes afterwards with al reservoirs full.

First of all we cleared out of the bed of the river *on* and for som distance *below* the base of the wall a very great number of larg boulders and some rocks *in situ* in order to enable the water to ru freely away. We then erected a concrete and timber stank on th Breconshire side of the river to exclude the water, and thus allow o the excavation for the foundation of that end of the wall being go out and the base of the wall and the Brecon culvert built. This has

DIAGRAM No. 7.



Flood water Channel
Culvert
Stank
Culvert
Bridge
Weir
Compensation Works
Flood water Channel
Compensation Works

all been done, the wall having been carried up to 730 O.D., or 30 feet above the bed of the river, the water passing meanwhile along the left side of its old course. We have now completed a similar stank on the Radnor side, and are getting out the foundation inside of it, and the building of the wall and the Radnor culvert will follow in due course. Then a stank of concrete will be erected up to the level of 730, abutting against the wall at the upper and inner end of each culvert. This stank being finished, we shall be in a position to impound water behind it to the extent of 240 million gallons, and to charge the two culverts (which are 16 feet in diameter) under a head over the centre of 22 feet, and this combined storage and power of discharge through the culverts will enable us to pass a maximum flood without interfering with the conduct of the works. The excavation for the foundation of the central part of the wall can then be got out, and the wall be built between the two ends (which are being finished with vertical joints, dovetailed in plan) up to 730, after which the remaining 92 feet in height of the wall can be erected without further trouble

When the wall has been finished to its full height the inlet ends of the two culverts will be closed. Whilst they are performing their function of passing the river in its normal state, and during floods, they are fitted with cast-iron trumpets or bell-mouthed inlets to facilitate the entrance of the water. At the proper time these castings will be removed, and the face-plate to which they are attached will then become the seating of a steel caisson, which will be lowered into its place by means of guides previously fixed and drawn home so as to form a watertight junction by bolts inside. These doors or caissons are competent to bear the pressure due to a full reservoir, viz. *about* 560 *tons*, and under their protection the pipes with their valves will be laid in the culverts for conveying the compensation water to the measuring chambers outside. Afterwards each of the caissons will be reinforced by a mass of concrete and brickwork inside the culvert, so that there may be no risk of the perfect and permanent soundness and watertightness of the "*stop*." In connection with the measuring apparatus there will be self-recording gauges and testing chambers, and turbines driven by the compensation water actuating accumulator pumps for working the hydraulic valves and dynamos for electric lighting. With a full reservoir the passing of the 27 million gallons a day of compensation water will give about 650 horse-power, gross. When the reservoir is full the water will overflow the whole 600-foot length of the wall, unimpeded in any way, and at the time of a high flood the depth will be about 3 feet on the crest. This will be a magnificent sight, which I hope some of us may live to see. On each side of the valley a channel lined with masonry and concrete will be constructed in front of the ends of the main wall to conduct the water harmlessly down and train it into the main channel of the river, which will be enclosed within masonry side walls 150 feet apart.

GRAM No. 8.

At the dams higher up the river, similar means are being provided for the passing of flood waters, modified, of course, to meet the circumstances of each case. The Craig-gôch dam is to be built on a curve in plan, all the other main dams being straight, and will have a roadway carried over it on arches. The submerged dam will also have a road over it, and as upon it must be laid a railway for the conveyance of materials up the Claerwen Valley, its ends must be built on practicable railway curves.

Before closing this much-condensed description of the general scheme and the works in the valley, I should like to say that out of the 45,562 acres of the collecting area probably 40,000 consist of open mountain pasture or moor land carrying not more than one small sheep per acre. Diagram No. 8 gives a very fair idea of the general character. This is the country just above the upper end of the Craig-gôch reservoir.

In the lower parts of the valleys there is some cultivated land, which will for the most part be occupied by the reservoirs, roads and railways, the small farmsteads being submerged and all trees and fences being removed below top water level of the reservoirs. Practically the whole area will be expropriated; only the cottages of the very few shepherds needed, being left. The old manor house of Nant-gwillt will be drowned, as also Cwm Elan House, for some time the residence of Shelley, and the very small Nant-gwillt Church and a Baptist chapel, from the grave yard of which the remains of between 60 and 70 bodies have been removed and reinterred near a new chapel erected below Caban Côch.

Aqueduct.

I will now shortly describe the course and mode of construction of the aqueduct (Diagram No. 9). As has already been stated, the aqueduct commences in the side of the Caban Côch reservoir above the submerged or Caregddu dam, and terminates in the Frankley service reservoir, nearly 74 miles distant. At its inlet there will be a tower containing the controlling valves and simple screens to keep out floating matters. The aqueduct goes immediately into tunnel, a mile and a quarter in length, through the Foel, and emerges on the side of the hill about 800 yards below the Caban dam. At about 4½ miles it crosses over the Mid Wales Railway where that line is in tunnel, and at 5 miles under the river Wye, a little south of the small town of Rhayader. At 10 miles it passes the village of Nantmel, and at 17 goes under the Central Wales Railway at Dolau, where it enters a tunnel 4¼ miles long. At 26 miles it is just south of Knighton, that point being at the east end of another tunnel 2½ miles long. At 35 miles it crosses over the river Teme, south of Leintwardine, then runs along Bringwood Chase to just south of Ludlow, where it again crosses the Teme. At 52½ miles it is half a mile north of Cleobury Mortimer, and at 58 miles it crosses over the river Severn 3 miles

DIAGRAM No 9

CROSS SECTION OF AQUEDUCT

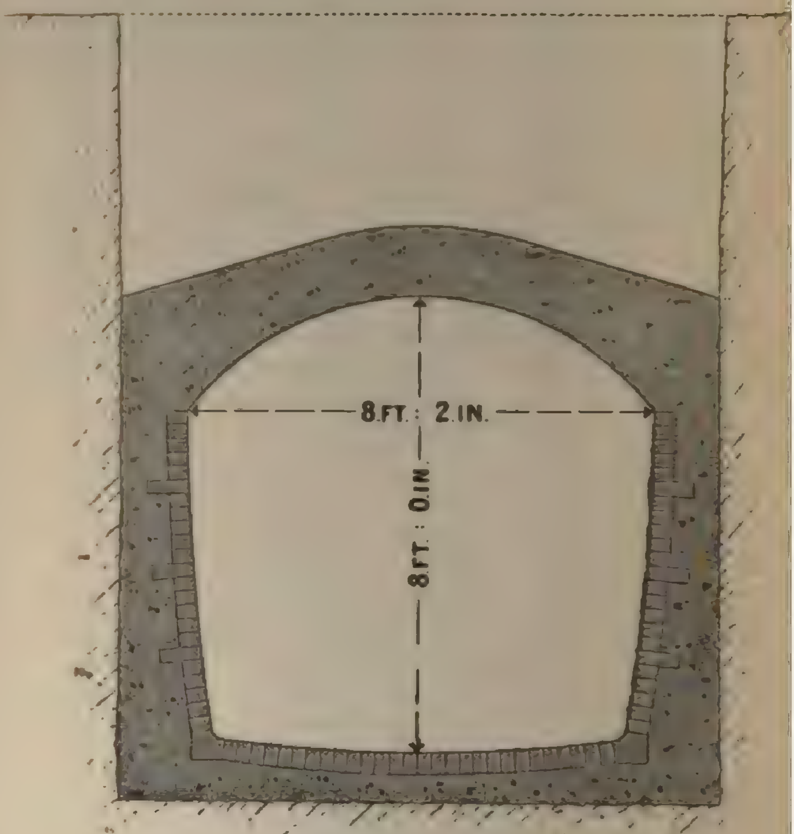


DIAGRAM No. 10.

DIAGRAM No. 11.

AQUEDUCT ABOVE GROUND, NEAR KNIGHTON.

north of Bewdley, where the pressure in the pipes will be about 240 lbs. on the square inch. At 63½ miles it is just north of Wolverley, and at 68 close to Hagley, reaching the intended Frankley reservoir at 73 miles 54 chains.

In addition to the two railways above mentioned the aqueduct crosses the Shrewsbury and Hereford Railway at 42 miles 10 chains, the Severn Valley Railway at 58 miles 54 chains, the Stafford and Worcester Canal at 62 miles 70 chains, and the Halesowen and Bromsgrove Railway at 72 miles 5 chains. In its course it also crosses the rivers Rea and Stour, and the Teme a third time.

There are altogether—

	13½	miles of tunnel;
	23	miles of cut and cover; and
	37¼	miles of iron and steel pipes crossing valleys under pressure.
Total	73¾	miles.

The meaning of "*cut and cover*" is that the aqueduct is laid in ground approximately parallel to and slightly higher than the hydraulic gradient line, so that an open trench may be cut, the aqueduct built in it, and the ground filled in and restored over it to its original condition. In *tunnel* and *cut and cover* the structure consists of blue brick lining on a concrete backing so far as the invert and side wall are concerned, the arch being of concrete only.

Diagram No. 10 shows the cut and cover conduit in construction, and Diagram No. 11 the aqueduct as built across narrow valleys.

This conduit is laid almost throughout with a fall of 1 in 4000, or about 16 inches in a mile, the exception being in the long tunnels, which have slightly better gradients. It will carry, running something under full, 75 million gallons a day, and the first instalment of 27 million gallons a day will flow about 3 feet deep and with a speed of 150 feet a minute, taking about 44 hours in its passage from the Elan to Birmingham. In crossing valleys below the hydraulic gradient line the aqueduct will consist at first of two 42-inch cast-iron or steel pipes, with a fall of 3 feet in a mile, or 1 in 1760. As the demand for water increases, a third, fourth, fifth and sixth pipe of similar size will be laid.

The service reservoir at Frankley is to be divided into two equal parts, each holding 100 million gallons. The surface water area will be 25 acres, and the depth 30 feet. The side walls will be of concrete faced with blue brickwork, a skin of asphalt coming between them and being laid also on the concrete floor. Below this reservoir will be built a series of filter-beds, sufficient at all times to efficiently filter all the water that is required. From a pure water tank below the filters the gravitation mains will start into the district, and from it will be pumped such water as is wanted for a high fringe of sparsely populated country too high to be commanded by gravitation.

HOUSING OF WORK

I should like now to be allowed t
arrangements which have been made by
with whom I am in constant touch, for t
being of the workpeople engaged on th
and their families. At my recommen
mined to undertake the construction of th
works in the valley under the direct
staff, and without the intervention of
time nor place either to defend or apolo
it to say that up to the present time th
satisfaction. Having thus decided, the
people were to be kept together in cl
and it was answered by the erection
Cöch, with sufficient accommodation f
houses are of wood, and are built of d
grades; thus. there are huts for officia
schoolmaster, for gangers, for marri
lodgers. It has not been unusual on
four men into such a hut, sleeping in
where work was going on day and nig
occasions when these beds have not ha
say the least of it, is *not nice*. The
from me to sanction the erection of t
the larger eight men sleep in one larg
own separate cubicle and single bed.

Water is laid on under pressure thr
age system is as good as can be made
There is also a canteen, where good bee
had at certain hours and under strict re
and older children, with one male an
rooms being used on Sundays for reli
a large recreation hall with gymnasiu
tion. and a circulating library, and
theatrical entertainments, and this las
are *baths* and wash-houses, and a gen
the village, and another for infectious
side. The baths are, of course, patro
afternoon and Sunday morning. Whe
one charge, viz. a penny. It was soo
account had to be taken of different gr
tramp labourer, *who is not a proud ma*
navvy demanded to pay more so as to
on a still higher platform. Now, the
towel costs a penny. Ditto, with two to
ditto, and high-class toilet soap, two-

ladies' days, but into the particulars of their prejudices I have not ventured to inquire.

To keep out infectious diseases there is also a "doss house" on the opposite side of the river to the village, where men tramping in search of work are taken in. On admission they are made to have a warm bath and their clothes are disinfected, and for a week they sleep here, working with others, and are under the supervision of the doctor, before being allowed to take up their quarters in the village. These arrangements have hitherto been successful, and whilst two years ago small-pox was epidemic in many parts of South Wales, and especially on some large public works, we escaped.

In the rest of this description of the village, I am quoting from a lecture, delivered in Birmingham on several occasions with great success, by Mr. E. A. Lees, the highly esteemed Secretary of the Water Committee.

"The village is on the opposite side of the river to the road, and access is given to it by a suspension bridge constructed across the river by the Corporation. The position of the village, in that it has to be approached by this bridge, and that it is erected on private ground to which there is no public right of way, is fortunate, in that the Corporation thereby have the means of exercising a beneficent supervision which would be impossible were it, in the ordinary sense of the word, a public place. Nor is the supervision of the Corporation merely nominal. No strangers are allowed in the village without permission. Every tradesman who wishes to deliver goods is required to furnish himself with a pass, on which somewhat stringent regulations are laid down. For instance, the owner undertakes he will not deliver any intoxicating drinks within the village; and the Sunday quiet and rest of the inhabitants are protected by a regulation that, with the exception of milk, no goods shall be delivered or sold on that day; and these regulations are not a dead letter, for at the end of the bridge on the village side a gate is situate, at which the bridge-keeper is constantly in attendance, and examines the contents of every cart before it is allowed to proceed.

"Fire hydrants are fixed on the water mains throughout, fire extinguishing appliances are provided at convenient points, and in the middle of the village there is a small fire station surmounted by a fire bell. This is the rendezvous of the fire brigade, some members of whom are on duty every evening. The village is perambulated throughout the night by two watchmen. All of the huts are moreover inspected weekly by the village superintendent, with a view to the removal of all refuse, and the prevention of the use of oil lamps of dangerous type, and other articles likely to occasion an outbreak of fire.

"The village day school is placed under the Education Department, the school managers being the Chairman of the Water Committee with three officials, two of whom are resident at the works and one in Birmingham. The buildings are certified by the Department

as sufficient for the accommodation of 168 scholars. At first, considerable difficulty was experienced in bringing the navvy children under the discipline of regular instruction, but now good progress is being made, and, at the last examination by the Government Inspector, the school earned the highest possible grant.

"I must now refer to the canteen: To this institution a special interest attaches, as we have here an experiment embodying some of the suggestions thrown out for the regulation of the liquor traffic. In point of fact, the canteen is a municipal public house, and is, I think, the only instance of the kind in the United Kingdom. On the question of the drink traffic there were the three proverbial courses open to the Water Committee:—

"1. To do nothing, and allow any enterprising publicans who could obtain licenses to set up their establishments and conduct their trade in the usual manner.

"2. To attempt to prohibit the traffic altogether.

"3. To undertake the provision of beer for the use of the community, but under such regulations as should render it least hurtful.

"The objection to the first course is obvious. The navvies—in common, alas, with many others—readily yield to temptations to drink when they have the means of gratifying the appetite; and during the summer months, when regularity in the gangs is of the utmost importance, and at the same time when earnings are highest, there would be the greatest likelihood of the demoralising and disastrous effects of drunkenness asserting themselves.

"To the second course the objection was none the less marked. The people, rightly or wrongly, will have their beer, and without facilities to obtain it in a legitimate manner, they would decline the place altogether or resort to illicit means to supply themselves. It was held, therefore, to be impolitic to attempt prohibition, and I think it would have been unwise to prohibit altogether the sale of beer.

"The third alternative course, then, was that adopted, namely, to provide beer under stringent regulations. The canteen is placed in charge of a manager, in whose name the license stands. The manager has no interest in the sale of the drink; his salary is fixed, and is sufficiently liberal to command the services of a thoroughly reliable and respectable man. The points against which he must guard himself are, incivility to customers on the part of himself or his assistants, lack of cleanliness in the house and drinking vessels, adulteration of the liquors, selling *out of hours*, and disorder and drunkenness on the part of his customers. If he is able to avoid offence in all these respects he is thought no worse of if the takings fall off, and no better of if they increase. To promote the objects in view, stringent regulations have been enacted; and the regulations are not merely printed and hung on the walls, but are actually enforced. The sale of drink is refused to men who show signs of having had enough, or who have already been supplied up to the stipulated limit. No women or children are permitted in the bar.

Even in the out-door department no woman under 21 years of age is served, and no boy under 16. The house is closed every night at nine o'clock, and the inspection and co-operation of the police are courted in every way. Every effort is made to sell a thoroughly wholesome and pure beer. A regular system of sample taking and testing is carried out, samples being taken without notice from time to time and forwarded to Birmingham for analysis in cases marked with numbers only, so that the analyst cannot tell from what brewers the beers are purchased.

"Now as to the social results. While we cannot say that by our attempt to regulate the drink traffic we have created a 'Utopia,' we may fairly say, and indeed we claim, that we have reduced the evil results of drinking to a minimum, taking into consideration the fact that on the opposite side of the river, within half a mile of the village, another public house exists, which is conducted on the usual lines. Persons qualified to judge speak in the highest terms of the results of the experiment.

"One of the declared bases of the Elan village canteen is that the profits are devoted to the social well-being of the community. First, the whole of the cost of the day school, beyond the Government grant, and including the cost of the building, is provided from the canteen profits; in other words, the profits take the place of what in an ordinary community would be the School Board rate. Second, the cost of erecting and maintaining the public hall, with the library, gymnasium and reading room, is provided from the same source; recreation grounds for the workmen and clerical staff, the deficit on the bath house, and occasional help to charitable institutions, are all defrayed from the canteen profit."

The men are taken up the valley from the village early in the morning and brought back after their work in railway carriages, so as to save time and their exposure in open trucks, and the children from the upper works huts are brought down to school and returned home in the same way; with this ride in view the parents have no trouble in getting them away to their lessons.

[J. M.]

WEEKLY EVENING MEETING,

Friday, March 25, 1898.

Sir James Crichton-Browne, Treasurer and Vice-President, in the Chair.

The Very Rev. the Dean of Canterbury, D.D. F.R.S.

Canterbury Cathedral.

(Abstract.)

The Friday Evening Lecture was delivered by the Very Rev. Dean of Canterbury, who, at the request of the President, took as subject "Canterbury Cathedral." After speaking of the difficulty steering between the Scylla and Charybdis of saying too little or much, in dealing with the story of a cathedral which had been clo connected with the history of England for thirteen centuries, lecturer touched, first, on points of interest connected with Mer Lane and Christchurch Gate, and the ancient and famous Ki School. He spoke of the many styles of architecture still visibl the cathedral—Roman and Saxon, Early and Late Norman, Decora Early and Late Perpendicular, and modern—which mark the cha of a thousand years. To show how completely the cloisters ar Professor Willis called them, "a perfect museum of mediæval ar tecture," he showed a slide and photograph of the Martyrdom d where Edward I. was married to Margaret of France. The Nor door, by which Becket entered, was superseded by the Early Eng triple arcade of 1290, overlaid about 1400 by the fan-shaped sh and groins of Prior Chillenden, into which has been inserted Perpendicular door of Archbishop Morton, about 1490. He gave a very rapid sketch of the main events in the history of structure, which was burnt down (by the Danes) in 1011, and ag burnt down in 1067 and 1174, amid the wild emotion of the peo described by Gervase, who witnessed it. After describing how it rebuilt by William of Sens and William the Englishman, and later additions of Archbishops Simon of Sudbury, Arundel, Cour and Morton, he spoke of the cloisters, and described the daily lif a mediæval monk, the hardships of which sufficed to account for immense size of the infirmary, of which the ruins still remain. an illustration of some of the memorable scenes for which cathedral is famous, Dr. Farrar rapidly described, from origi sources, the circumstances which attended the murder of Bec This was illustrated by a reproduction of the ancient painting, n mainly obliterated, on the tomb of Henry IV. After alluding to Becket pilgrims and the relics, and the famous visits of vari

sovereigns, and of Erasmus and Dean Colet, he described the memorable penance of Henry II. in the crypt, which was also illustrated by an ancient picture. He then mentioned the discovery of the stone coffin in the nave a few years ago, and gave very strong reasons for his own belief that it contains the genuine remains of the murdered Archbishop. Attention was next turned to the refuge offered in the crypt to the Walloons and Huguenots, whose French service is still continued in the Black Prince's Chantry every Sunday afternoon. The ravages committed by Culmer ("Blue Dick") and the Puritans in 1642 were next described, and the lecture concluded with a swift glance at the recent events in the cathedral history—the burial of Archbishop Benson, the *first* prelate of the Reformed English Church to be buried in his own cathedral after an interspace of 338 years, since the death of Cardinal Pole; the enthronement of Archbishop Temple; the visit of their Royal Highnesses the Prince and Princess of Wales with their family, and a circle of illustrious Englishmen, in 1897; and the thirteenth centenary visit of all the English-speaking bishops of the Empire, and of Cardinals Vaughan and Derrand, the Archbishop of Trebizond, the Duke of Norfolk, and other illustrious Roman Catholic prelates and laymen.

The lecture was illustrated throughout with fine lantern slides and large photographs of the cathedral buildings.

GENERAL MONTHLY MEETING,

Monday, April 4, 1898.

Sir James Crichton-Browne, M.D. LL.D. F.R.S. Treasurer and Vice-President, in the Chair.

Miss Sarah Brisco,
Frank Clowes, Esq. D.Sc. F.C.S.
Sherard Cowper-Coles, Esq.
James E. Horne, Esq. M.A.
Stephen Miall, Esq. LL.D. B.Sc.
Cecil David Mocatta, Esq.
Ernest George Mocatta, Esq.

were elected Members of the Royal Institution.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Secretary of State for India—Review of Education in Bengal (1892-93 to 1896-97). fol. 1897.

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Bimetallist for March, 1898.
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Education for March, 1898. 8vo.
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WEEKLY EVENING MEETING,

Friday, April 29, 1898.

BASIL WOODD SMITH, ESQ. F.R.A.S. F.S.A. Vice-President, in the Chair.

PROFESSOR ANDREW GRAY, M.A. LL.D. F.R.S.

Magneto-Optic Rotation and its Explanation by a Gyrostatic Medium.

THE action of magnetism on the propagation of light in a transparent medium has been rightly regarded as one of the most beautiful of Faraday's great scientific discoveries. Like most important discoveries it was no result of accidental observation, but was the outcome of long and patient inquiry. Guided by a conviction that (to quote his own words) "the various forms under which the forces of matter are made manifest have one common origin," he made many attempts to discover a relation between light and electricity, but for very long with negative results. Still, however, retaining a strong persuasion that his view was correct, and that some such relation must exist, he was undiscouraged, and only proceeded to search for it more strictly and carefully than ever. At last, as he himself says, he "succeeded in *magnetising and electrifying a ray of light, and in illuminating a magnetic line of force.*"

Faraday pictured the space round a magnet as permeated by what he called lines of force; these he regarded as no mere mathematical abstractions, but as having a real physical existence represented by a change of state of the medium brought about by the introduction of the magnet. That there is such a medium surrounding a magnet we take for granted. The lines of force are shown by the directions which the small elongated pieces of iron we have in iron filings take when sprinkled on a smooth horizontal surface surrounding a horizontal bar magnet, as in the experiment I here make. [*Experiment to show field of bar magnet by iron filings.*]

The arrangement of these lines of force depends upon the nature of the magnet producing them. If the magnet be of horse-shoe shape the lines are crowded into the space between the poles; and if the pole faces be close together and have their opposed surfaces flat and parallel, the lines of force pass straight across from one surface to the other in the manner shown in the diagram before you. [*Diagram of field between flat pole faces.*]

The physical existence of these lines of force was demonstrated for a number of different media by the discovery of Faraday to which

I have already referred, and on which almost all the later work
the relation of magnetism to light has been founded. I am permit
by the kindness of the authorities of this Institution to exhibit h
the very apparatus which Faraday himself employed, though for
various experiments I have to make it is necessary to actually
another set of instruments. [*Apparatus shown.*] Before repeat
Faraday's experiment, let me describe shortly what I propose to
and the effect to be observed.

A beam of plane polarised light is produced by passing white li
from this electric lamp through a Nicol's prism. To understand
nature of plane polarised light, look for a moment at this other d

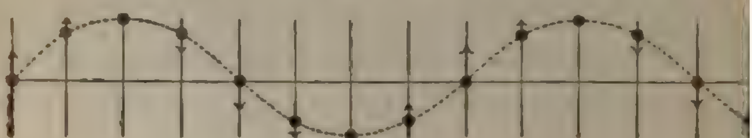

Fig. 1.

gram (Fig. 1). It represents a series of particles displaced in a cert
regular manner to different distances from the mean or equilibri
positions they originally had along a straight line. They are movi
in the directions shown by the arrows and with velocities dependi
on their positions, as indicated by the lengths of the arrows. Supp
a certain interval of time to elapse. The particles will have mo
in that time to the positions shown in this other diagram (Fig. 2),

Fig. 2.

the same sheet. It will be seen that the velocities as well as t
positions of the particles have altered, but that the configuratio
the same as would be given by the former diagram moved thro
a certain distance to the left.

Thus an observer looking at the particles and regarding their c
figuration would see that configuration apparently move to the l
and this, it is very carefully to be noted, is a result of the transve
motions of the individual particles. In another interval of time eq
to the former, the arrangement of particles will appear to have mo
a further distance of the same amount towards the left.

This transverse motion of the particles, thus shown displaced fr

their equilibrium positions, represents the vibration of the medium which is the vehicle of light, and the right to left motion of the configuration of particles is the wave motion resulting from that vibration. I do not say that the medium is thus made up of discrete particles, or that the different portions of it vibrate in this manner, but there is undoubtedly a directed quantity transverse to the direction in which the wave is travelling, the value of which at different points may be represented by the displacements of the particles, and which varies in the same manner, and results, as here shown, in the propagation of a wave of the quantity concerned.

In fact we have here a representation of a wave of plane polarised light. The directions of vibration are right lines parallel at all points along the wave. Ordinary light consists of vibrations the directions of which are not parallel if rectilinear, and each vibration is therefore capable of being resolved into two in directions at right angles to one another. The Nicol's prism in fact splits a wave of ordinary unpolarised light into two waves, one in which the vibrations are in one plane containing the direction in which the light is travelling, the other in a plane containing the same direction, but at right angles to the former. One of these waves is stopped by the film of Canada balsam in the prism and thrown out of its course, while the other wave is allowed to pass on undisturbed.

If the wave thus allowed to pass by one Nicol's prism be received by another, it is found that there are two positions of the latter in which the wave passes freely through the second prism, and two others in which the wave is stopped. The prism can be turned from one position to another by properly placing it and then turning it round the direction of the ray. It is found that if the prism be thus turned

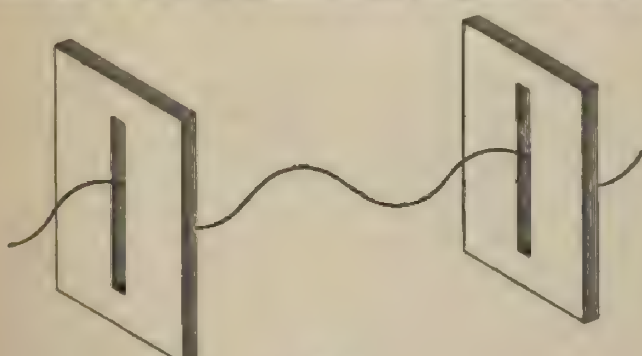

FIG. 3.

from a position in which the light is freely transmitted, we come after turning it through 90° to a position in which the light is stopped, and that if we go on turning through another angle of 90° a position

is reached in which the light is again freely transmitted, and so on, the light being alternately stopped and transmitted by the second prism in successive positions 90° apart.

The mode of passage of the wave by the Nicols when their planes are parallel, and its stoppage when the planes are crossed, are illustrated by this diagram (Fig. 3) of a vibrating cord and two slits. When the slits are parallel, the vibration which is passed by one is passed by the other; when they are crossed, a vibration passed by one is stopped by the other.

Two planes of symmetry of the prisms parallel to the ray, and called their principal planes, are parallel to one another when the light passes through both, and are perpendicular to one another when the light passed by the first is stopped by the second. We shall call the first prism the polarising prism, or the *polariser*, from its effect in producing plane polarised light; the other, the *analyser*. The stoppage of the light in the two positions 180° apart of the second prism, and its passage in the two intermediate positions, show that the light passed by the first prism is plane polarised.

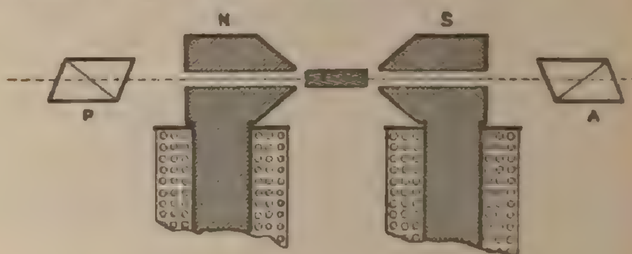

FIG. 4.

Now a beam of plane polarised light is passed through the perforated pole-pieces of this large electro-magnet (Fig. 4), so that the beam travels between the pole-faces along the direction which the lines of force there would have if the magnet were excited by a current. The arrangement of the apparatus is as shown in the diagram. The light is polarised by the prism P, passes through the magnetic field, and then through the analysing prism A, to the screen. As you see, when the second prism is turned round the ray the light on the screen alternately shines out and is extinguished, and you can see also that the angle between the positions of free passage and extinction is 90°.

I now place in the path of the beam this bar of a very remarkable kind of glass, some of the properties of which were investigated by Faraday. It is a very dense kind of lead glass, which may be described as a silicated borate of lead; that is, it contains silica, boric acid and lead oxide. The beam is not disturbed although the light passes through the glass from end to end. I now adjust the analysing prism to very nearly complete extinction, and then excite the magnet.

If the room is sufficiently darkened, I think all will see that when the magnet is excited there is a very perceptible brightening of the dim patch of light on the screen, and that this brightening disappears when the current is removed from the magnet. This is Faraday's discovery.

How are we to describe this result? What effect has been produced by the magnetic field? It is clear that the direction of vibration of the light emerging from the specimen of heavy glass has been changed relatively to the prism so that the light now readily passes. It is found, moreover, that the amount of turning of the direction of vibration round the ray is proportional to the length of the specimen, so that the directions of vibration at different points along the wave within the specimen lie on a helically twisted surface, and may be regarded as represented by the straight rods in the model before you on the table (Fig. 5).

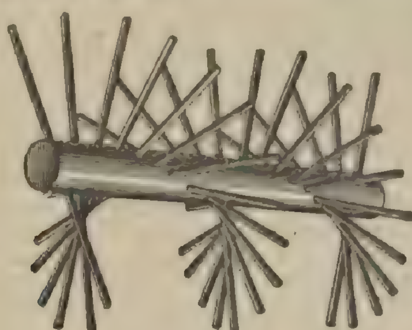

Fig. 5.

It is also found that the amount of the turning depends on the intensity of the magnetic field—is, in fact, simply proportional to that intensity. Hence the turning is proportional to the mean intensity of the field, and to the length of the path in the medium, that is, to the product of these two quantities. It also depends on the nature of the medium. The angle of turning produced by a field of known intensity when the ray passes through bisulphide of carbon has been very carefully measured by Lord Rayleigh, whose results are of great value for other magnetic work.

The law of proportionality of the amount of turning of the plane of polarisation to the intensity of the magnetic field in the space in which the substance is placed, is not, however, to be regarded as established for strongly magnetic substances, such as iron, nickel or cobalt. The matter has not yet been completely worked out, but the turning in such cases seems to be more nearly proportional to the intensity of magnetisation, a different quantity from the intensity of the magnetic field producing the magnetisation. If this law be

found correct, the angle of turning will be proportional to the product of the intensity of magnetisation and to the length of the path; and the angle observed divided by this product will give another constant, which has been called Kundt's constant.

The rotation of the plane of polarisation in strongly magnetised substances was investigated by Kundt, the very eminent head of the Physical Laboratory of the University of Berlin, who died only a year or two ago. Kundt is remembered for many beautiful methods which he introduced into quantitative physical work; but no work he did was more remarkable than that which he performed in magneto-optic rotation when he succeeded in passing a beam of plane polarised light through plates of iron, nickel and cobalt. Such substances, though apparently opaque to light, are not really so when obtained in plates of sufficient thinness. In sufficiently thin films all metals, so far as I know, are transparent, not merely to Röntgen rays, but to ordinary light. Kundt conceived the idea of forming such films of the strongly magnetic metals, so as to investigate their properties as regards magneto-optic rotation. He succeeded in obtaining them by electroplating platinised glass with such thin strata of these metals that light passed through them in sufficient quantity for observation. The rotation produced by the glass and the exceedingly thin film of platinum was determined once for all and allowed for. Kundt obtained the remarkable result that the magnetic rotatory power in iron is so great, that light transmitted through a thickness of one centimetre of iron magnetised to saturation is turned through an angle of over 200,000°, that is, that light passing through a thickness of an inch of iron magnetised to saturation would have its plane of polarisation turned completely round more than a thousand times; in other words, one complete turn would be given by a film less than $\frac{1}{1000}$ of an inch in thickness. A scarcely smaller result has been found by Du Bois for cobalt, and a maximum rotation of rather less than half as much by the same experimenter for nickel.

The direction of turning in all the cases which have so far been specified—that is, Faraday's glass, bisulphide of carbon, iron, nickel and cobalt—is the same as that in which a current of electricity would have to flow round the spires of a coil of wire surrounding the specimen so as to produce the magnetic field. This we call the *positive* direction. There are, however, many substances in which the turning produced by the magnetic field is in the contrary or negative direction: for example, ferrous and ferric salts of iron, chromate and bichromate of potassium, and in fact most compound substances which are feebly magnetic.

Faraday established by his experiments the fact that substances fall into two distinct classes as tested by their behaviour under the influence of magnetic force. For example, an elongated specimen of iron, nickel or cobalt, if freely suspended horizontally between the poles of our electro-magnet, would set itself with its length along the lines of force. On the other hand, a similar specimen of heavy glass, or a

tube filled with bisulphide of carbon, would, if similarly suspended, set itself across the lines of force. The former substances were therefore called by Faraday paramagnetic, the latter diamagnetic.

It might be supposed that diamagnetics would show a turning effect opposed to that found in paramagnetics, but this is not the case. As we have seen, bisulphide of carbon and heavy glass, which are diamagnetics, show a turning in the same direction as that produced in iron—as indeed do most solid, fluid and gaseous diamagnetics. Feebly paramagnetic compound substances, on the other hand, produce negative rotation.

A theory of diamagnetism has been put forward in which the phenomena are explained by supposing that all substances are paramagnetic in reality, but that so-called diamagnetic bodies are less so than the air in which they are immersed when experimented on. Thus the diamagnetic quality is one of the substances relatively to air, in the same kind of way as the apparent levity of a balloon is due to the fact that its total weight has a positive value, but is less than that of the air displaced by the balloon and appendages. Lord Kelvin's dynamical explanation of magneto-optic rotation does not bear out this view of the matter.

Before passing to the dynamical explanation, however, I must very shortly call attention to some remarkable discoveries in this subject made by Dr. John Kerr, of Glasgow. I have here an electro-magnet arranged as in the diagram before you (Fig. 6). The light from the lamp is first plane polarised by the Nicol P, then it is thrown on the piece of silvered glass G, and part of it is thereby reflected through this perforated pole-piece so as to fall normally on the polished point of the other pole-piece. Reflection thus takes place at perpendicular incidence, and the reflected light is received by this second Nicol. When the magnet is unexcited the second Nicol is arranged so as to quench the reflected light. The magnet is then excited, and it is found that the light is faintly restored, showing that an effect on the polarisation of the light has been produced by the magnetisation. It is to be noticed here that the incident and reflected light is in the direction of magnetisation. We shall not pause to make this experiment. It was arranged this morning and successfully carried out; but the effect is slight, and might not be noticeable without precautions,

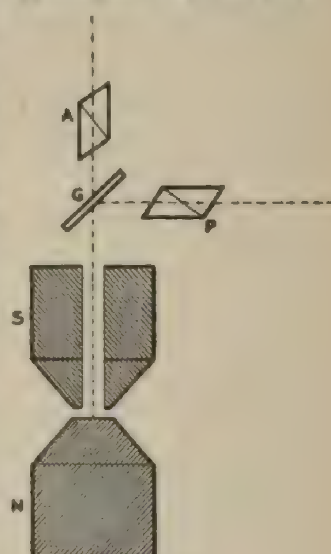

FIG. 6.

which we have hardly time to make, to exclude all extraneous light from the screen.

It would perhaps be incorrect to say that the plane of polarisation has been rotated in this case, as it has been asserted by Righi that the light after reflection is no longer plane polarised, but that there are two components of vibration at right angles to one another, so related that the resultant vibration is not rectilinear but elliptical. There is therefore no position in which the analysing prism can be placed so as to extinguish the reflected light. The transverse component necessary to give the elliptic vibration is, however, in this case, if it exists, very small, and very nearly complete extinction of the beam can be obtained by turning the analysing prism round so as to stop the other component vibration. The angle through which the prism must be turned to effect this is the amount of the apparent rotation. The direction of rotation is reversed by reversing the magnetism of the reflecting pole. Dr. Kerr found that the direction is always that in which the current flows in the coils producing the magnetisation of the pole.

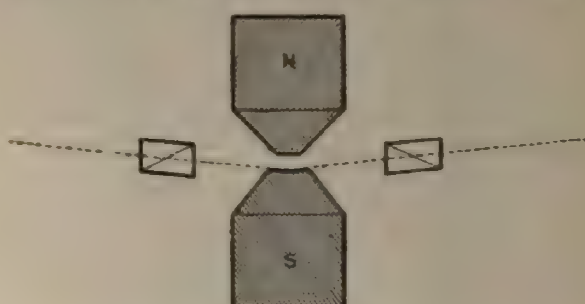

FIG. 7.

Dr Kerr also made experiments with light obliquely incident on a pole-face, with the arrangement of apparatus shown in this other diagram (Fig. 7). He found that the previously plane polarised light was by the reflection rendered slightly elliptically polarised. A slight turning of the analysing Nicol was necessary to place it so as to stop the vibration corresponding to the long axis of the ellipse and so secure imperfect extinction.

These effects are, like those of normal incidence, very small, and they can hardly be shown to an audience.

I must now endeavour to give some slight account of the theories that have been put forward in explanation of magneto-optic rotation. There is an essential distinction between it and what is sometimes called the natural rotation, the plane of polarised light produced by substances, such as solutions of sugar, tartaric acid, quartz, &c., some of which rotate the plane to the right, some to the left. When

light is sent once along a column of any of those substances without any magnetic field, its plane of rotation is rotated just as it is in heavy glass or bisulphide of carbon in a magnetic field. But if the ray, after passing through the column of sugar or quartz, is received on a silvered reflector and sent back again through the column to the starting point, its plane of polarisation is found to be in the same direction as at first. Quite the contrary happens when the rotation is due to the action of a magnetic field. Then the rotation is found to be doubled by the forward and backward passage, and it can be increased to any required degree by sending the ray backward and forward through the substance, as shown in this other diagram (Fig. 8).

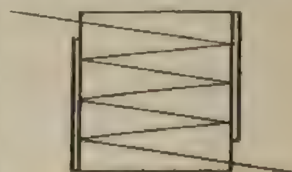

FIG. 8.

Thus the rotations in the two cases are essentially different, and must be brought about by different causes. In fact, as was first, I believe, shown by Lord Kelvin, the annulment of the turning in quartz, and the reinforcement of the turning in a magnetic field, produced by sending the ray back again after reflection at the surface of an optically denser medium, points to a peculiarity of structure of the medium as the cause of the turning of the plane of polarisation in sugar solutions and quartz, and to the existence of rotation in the medium as the cause of the turning in a magnetic field. Think of an elastic solid, highly incompressible and endowed with great elasticity of shape and of the same quality in different directions—a stiff jelly may be taken as an example to fix the ideas. Now let one portion of the jelly have bored into it a very large number of extremely small corkscrew-shaped cavities, having their axes all turned in the same direction. Let another portion have imbedded in it a very large number of extremely small rotating bodies, spinning-tops or gyrostats in fact, and let these be uniformly distributed through the substance, and have their axes all turned in the same direction.

Both portions would transmit a plane polarised wave of transverse vibration travelling in the direction of the axes of the cavities or of the tops with rotation of the plane of polarisation; but in the former case the wave, if reflected and made to travel back, would have the original plane of polarisation restored; in the latter the turning would be doubled by the backward passage.

To understand this it is necessary to enter a little in detail into the analysis of the nature of plane polarised light. As I have already said, the elastic solid theory may not express the facts of light propagation, but only a certain correspondence with the facts. But its use puts this matter in a very clear way. In a ray of plane polarised light each portion of the ether has a motion of vibration in a line at right angles to the ray, and the direction of this line is the

same for each moving particle. The lines of motion and the relativ positions of the particles in a wave are shown in the first diagram abov (Fig. 1). As the motion is kept up at the place of excitation it i propagated out by the elastic resistance of the medium to displace ment, and the configuration of particles travels outwards with the spe of light, traversing a wave-length (represent in the diagram by the distance between tw particles of the row in the same phase of motion in the period of complete to-and-fro motion of particle in its rectilineal path.

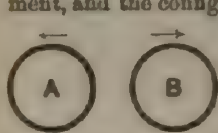

Fig. 9.

Now, a to-and-fro motion such as this can b conceived as made up of two opposite uniform an equal circular motions. Think of two distinct particles moving in th two equal circles A B in this diagram (Fig. 9), with equal unifor speeds in opposite directions. Let each particle be at the top of it

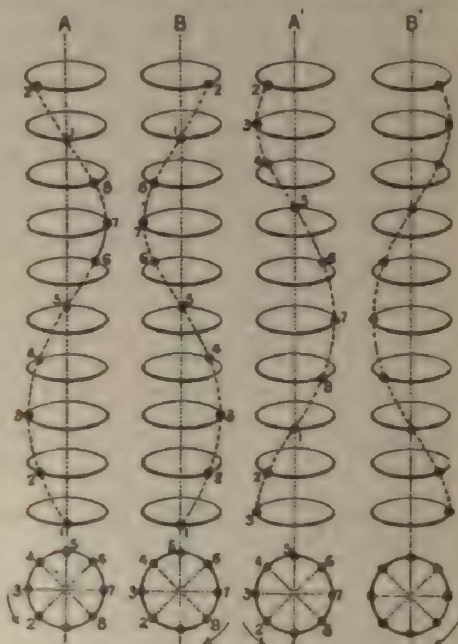

Fig. 10.

circle at the same instant; then at any other instant they will be in similar positions, but one on the right, the other on the left of th vertical diameter of the circle. Thus at that instant each particle i moving downward or upward at the same speed, while with whateve speed one is moving to the left, the other is moving with precisel

that speed towards the right. Imagine, now, these two motions to be united in a single particle. The vertical motions will be added together, the right and left motions will cancel one another, and the particle will have a motion of vibration in the vertical direction of range equal to twice the diameter of the circles, and in the period of the circular motions.

The rate of increase of velocity of the particle at each instant is the resultant obtained by properly adding together the accelerations of the particles in the circular motions, and therefore the force which must act on the particle to cause it to describe the vibratory motion just described, is the resultant of the forces required to give to the two particles the circular motions which have just been considered.

Now, what we have done for any one particle may be conceived of as done for all the particles in a wave. To understand the nature of a wave in this scheme, we must think of a series of particles originally in a straight line in the direction of propagation of the ray, as displaced to positions on a helix surrounding that direction. Fig. A

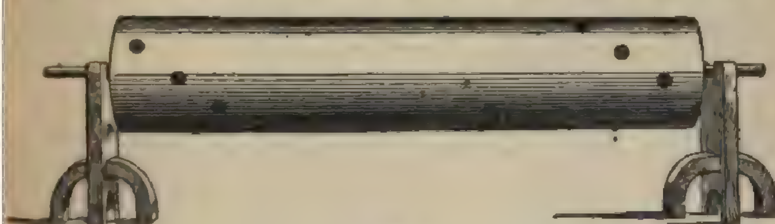

FIG. 11.

of this diagram (Fig. 10), regarded from the lower end, and the black spots on the model before you, show a left-handed helical arrangement. Let these particles be projected with equal speeds in the circular paths represented by the circle at the bottom of Fig. A. On this circle are seen the apparent positions of different particles in the helical arrangement when it is viewed by an eye looking upwards along its axis. This motion is shown by that of the black spots on the surface of the model (Fig. 11), when I set it into rotation about its axis. Let the particles be constrained to continue in motion exactly in this manner. As the model shows, the helical arrangement of the particles is displaced along the cylinder. This is the mode of propagation of a *circularly* polarised wave, which is made up of helical arrangements of particles which were formerly in straight lines parallel to the axis.

The direction of propagation of the wave is clearly from the bottom of the diagram to the top, and from the end of the model towards your left to the other, when the particles have a right-handed motion, and is in the contrary direction when the direction of rotation is reversed. For a right-handed helical arrangement the direction of

propagation for the same direction of motion of the particles is the opposite of that just specified. The direction of propagation remains therefore the same when the direction of motion and the helical arrangement of the particles are both reversed. All this can be made out from the diagram. Fig. B shows part of a right-handed arrangement of particles corresponding to the opposite arrangement of Fig. A; and if the particles have the motions shown at the bottom of the diagram, the propagation will be for both in the same direction from the bottom to the top.

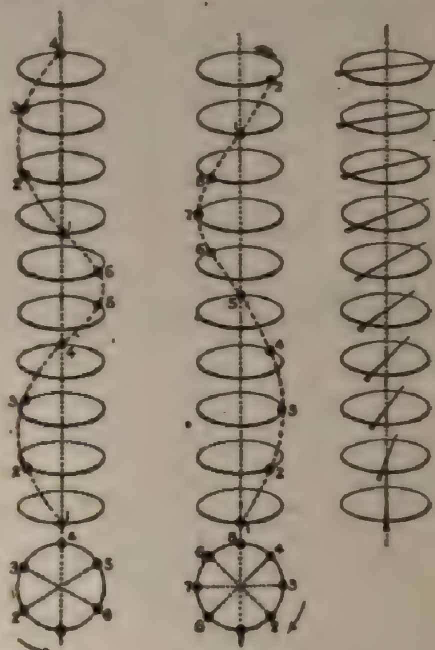

FIG. 12.

In Fig. 10 we suppose the periods equal and also the wave-lengths, the distance along the axis from particle 1 to particle 9. The combination of the circular motions A and B gives rectilinear motion; the combination of the wave motions of Figs. A and B gives a plane polarised wave, the plane of polarisation of which does not change in position. If, however, while the periods were equal, the wave-lengths were unequal, as shown in this other diagram (Fig. 12), the plane of polarisation would rotate, as shown by the lines drawn across the paths in the figure on the right, for the circular motions of particles in the longer wave would gain on those in the shorter.

A little consideration will show that the direction of the resultant rectilinear motion will, in consequence of the unequal speeds of propagation, turn round as the wave advances, and will do so in the direction of motion of the particles in the more quickly travelling wave, generating the screw surface shown in the model I have already exhibited.

We must now consider the forces. The particles moving in the circular paths have accelerations towards the centres of these paths, and forces must be applied to them to produce these accelerations. These forces are applied in the present theory by the action of the medium, and it is the reactions of the part cles on the medium that are properly called the centrifugal forces of the particles. The requisite centreward forces then are supplied by the state of strain into which the medium is thrown by the displacement of parts of it, which form in the undisturbed position a series of straight arrays in the direction of propagation, into these helical arrangements round that direction. The greater these elastic forces the greater the velocity of propagation of the wave.

In an elastic medium these forces depend on the *amount* of the relative displacements of the particles, and will be greater for displacements in the right-hand helical arrangement than for displacements in the opposite direction if the medium has a greater rigidity for right-handed distortion than for left, and the right-handed wave of distortion will be transmitted with greater speed, and *vice versâ*. This is the case of solutions of sugar and tartaric acid, quartz, &c., for which a helical structure has been supposed to exist in the medium.

Taking this case, refer to Figs. A and B of our large diagram (Fig. 10), and let the right-handed wave travel the faster. Let the waves travel up, be reflected at the upper ends, as at the surface of a denser medium, and then travel down again. The reflected waves are those shown in Figs. A′, B′ of the diagram. By the reflection the helical arrangement will be unaltered. But the plane of polarisation, as we have seen, turns round in space in the direction of the motion of the particles in the more quickly moving wave; it therefore turns round in the direction of the hands of a watch as the wave moves in the upward direction in the diagram, and in the opposite direction when the wave is travelling back. Thus the rotation of the plane of polarisation produced in the forward passage is undone in the backward.

It is easy to see that the same thing will take place if the reflection is at the surface of an optically rarer medium, so that the direction of motion of the particles is the same in the reflected as in the direct wave. The helical arrangements, however, are reversed by the reflection, and hence the wave which travelled the more quickly forward travels the more slowly back, and again the turning of the plane of polarisation is annulled by the backward passage. Thus Lord Kelvin's hypothesis of difference of structure completely explains the phenomena.

We pass now to the other case, that of magneto-optic rotation. Let us suppose, to fix the ideas, that the right-handed circular ray travels faster than the other, and that whether direct or reversed. Here, as in the other case, the elastic reaction of the medium on the displaced particles depends only on the distortion, and if there be no structural peculiarity in the medium there must be the same reaction in the particles in both the circular waves which combine to make up the plane polarised one.

Thus the actions on the particles being the same for both waves, and the velocities of propagation being different, the motions concerned in the light propagation cannot be the same. There must in fact be a motion already existing in the medium which, compounded with the motions concerned in light propagation, give two motions which give equal reactions in the medium against the equal elastic forces, applied to the particles in the case of equal helical displacements.

Thus Lord Kelvin supposes that in the medium in the magnetic field there exists a motion capable of being compounded with the luminiferous motion of either circularly polarised beam. The latter is thus only a component of the whole motion.

In the very important paper in which he has set forth his theory Lord Kelvin expresses his strong conviction that his dynamical explanation is the only possible one. If this view be correct, Faraday's magneto-optic discovery affords a demonstration of the reality of Ampère's theory of the ultimate nature of magnetism. For we have only to consider the particles of a magnetised body as electrons or groups of charges of electricity, ultimate as to smallness, rotating about axes on the whole in alignment along the direction of the magnetic force, and with a preponderance of one of the two directions of rotation over the other. Each rotating molecule is an infinitesimal electro-magnet, of which the current distribution is furnished by the system of convection currents constituted by the moving charges.

The subject of magneto-optic rotation has also been considered by Larmor, and two types of theory of these effects have been indicated by him in his report on the 'Action of Magnetism on Light.' One is represented by Lord Kelvin's theory, which is illustrated by Maxwell's chapter on molecular vortices in his 'Electricity and Magnetism.' FitzGerald's paper "On the Electromagnetic Theory of the Reflection and Refraction of Light," in the 'Philosophical Transactions' for 1880, is related to Maxwell's theory, and explains the rotation produced by reflection from the pole of a magnet by means of the addition of a term to the energy of the system. The other theory is also a purely electromagnetic one, and supposes that the effects are due to a kind of æolotropy of the medium set up by the magnetisation, and so attributes them to a change of structure which introduces rotational terms into the equations connecting *electric* displacements and electric forces. This latter theory therefore

regards the magneto-optic rotation as only a secondary effect of the magnetisation, which is not supposed to exert any direct dynamical influence on the transmission of the light-waves.

It is not possible here to enter into the subject of these theories, but I should like to direct attention to a paper by Mr. J. G. Leathem, just published in the 'Philosophical Transactions,' in which the type of theory just referred to has been worked out and compared in its results with the experiments of Sissingh and Zeeman in reflection. These investigators made measurements of the phase and amplitude of the magneto-optic component of the reflected light for various angles of incidence. For both these quantities the theoretical results of Leathem agree very well with the observed values.

Returning now to the gyrostatic medium, between which and the electro-magnetic theory, it is to be remembered, there is a correspondence, we may inquire in what way the gyrostats, when moved by the vibrations of the medium, react upon it, and so affect the velocity of propagation. The motion of a gyrostat is often regarded as mysterious, and it can hardly be fully explained except by mathematical investigation. But the general nature of its action may be made out without much difficulty. First of all, a gyrostat consists of a massive fly-wheel running on bearings attached to a case which more or less completely encloses the wheel. The mass of the wheel consists in the main of a massive rim, which renders as great as possible what is called the moment of momentum of the wheel when rotating about its axis. The diagram (Fig. 13) represents a partial section of the case and fly-wheel of a gyrostat, showing the arrangement of fly-wheel and bearings.

FIG. 13.

Now let the fly-wheel of such a gyrostat be rapidly rotated, and the gyrostat be hung up, as shown in this other diagram (Fig. 14), with the plane of the fly-wheel vertical, and a weight attached to one extremity of the axis. The gyrostat is not tilted over, but begins to turn round the cord by which it is suspended with a slow angular motion which is in the direction of the horizontal arrow if the direction of rotation is that of the circular arrow shown in the case. The same thing is shown by the experiment I now make. I spin this gyrostat, and hang it with the axis of rotation horizontal by passing a loop of cord round one end of the axis so that the weight of the gyrostat itself forms the weight tending to tilt it over about the point of suspension. The

axis of rotation here again remains nearly horizontal, but turns slowly round in a horizontal plane as before.

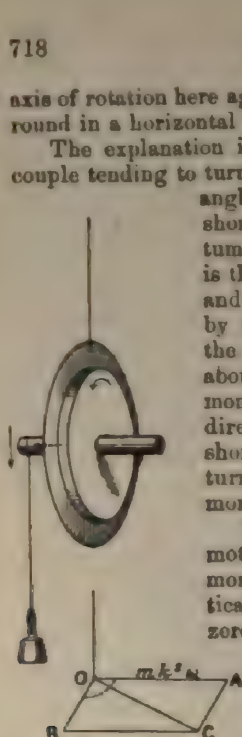

FIG. 14.

The explanation in general terms is this. The weight gives a couple tending to turn the gyrostat about a horizontal axis at right angles to that of rotation. This couple in any short interval of time produces moment of momentum about the axis specified, the amount of which is the moment of the couple multiplied by the time, and may be represented in direction and magnitude by the line O B. This must be compounded with the moment of momentum O A already existing about the axis of rotation, and gives for the resultant moment of momentum the line O C, which is the direction of the axis of rotation after the lapse of the short interval of time. The axis of rotation thus turns slowly round in the horizontal plane, and the more slowly the more rapidly the fly-wheel rotates.

The gyrostat in fact must have this precessional motion, as it is sometimes called, in order that the moment of momentum of the gyrostat about a vertical axis may remain zero. That it must remain zero follows from the fact that there is no couple in a horizontal plane acting on the gyrostat.

Thus any couple tending to change the direction of the axis in any plane produces a turning in a perpendicular plane. For example, if a horizontal couple, that is about a vertical axis, were applied to the axis of the gyrostat in the last figure it would turn about a horizontal axis, that is, would tilt over.

Again, consider a massive fly-wheel mounted on board ship on a horizontal axis in the direction across the ship. The rolling of the ship changes the direction of the axis, and produces a couple applied by the fly-wheel to the bearings, and an equal and opposite couple applied by the bearings to the fly-wheel. This couple is in the plane of the deck, and is reversed with the direction of rolling, and has its greatest value when the rate of turning of the ship is greatest. Thus the force on one bearing is towards the bow of the ship, the force on the other towards the stern, during a roll from one side to the other; and these forces are reversed during the roll back again. This is the gyrostatic couple exerted on its bearings by the armature of a dynamo on shipboard.

In the same way when a gyrostat is embedded in a medium and the medium is moving so as to change the direction of the axis of rotation, a couple acting on the medium in a plane at right angles to the plane of the direction of motion is brought into play. To fix the ideas, think of a row of small embedded gyrostats along this table, with their axes in the direction of the row, and their fly-wheels all rotating

in the same direction. Now let a wave of transverse displacement of the medium in the vertical direction pass along the medium in the direction of the chain. The vibratory motion of each part of the medium will turn the gyrostatic axis from the horizontal, and thereby introduce horizontal reactions on the medium. Again, a wave of horizontal vibratory motion will introduce vertical reactions in the medium from the gyrostats.

Now a wave of circular vibrations, like those we have already considered, passing through the medium in the direction of the chain, could be resolved into two waves of rectilinear vibration, one in which the vibration is horizontal, and another in which the vibration is vertical, giving respectively vertical and horizontal reactions in the medium. The magnetisation of the medium is regarded as due to the distribution throughout it of a multitude of rotating molecules, so small that the medium, notwithstanding their presence, seems of uniform quality. The molecules have, on the whole, an alignment of their axes in the direction of magnetisation. These reactions on the medium when worked out give terms in the equations of wave propagation of the proper kind to represent magneto-optic rotation.

It is worthy of mention that the addition of such terms to the equation was made by McCullagh, the well-known Irish mathematician, who, however, was unable to account for them by any physical theory. The necessary physical theory may be regarded as afforded by the mechanism which thus forms an essential part of Lord Kelvin's mode of accounting for magneto-optic effects.

Lord Kelvin, in his Baltimore Lectures, has suggested for magneto-optic rotation a form of gyrostatic molecule consisting, as shown in the figure, of a spherical sheath enclosing two equal gyrostats. These are connected with each other and with the case by ball-and-socket joints at the extremities of their axes, as shown in Fig. 15. If the spherical case were turned round any axis through the centre no disalignment of the gyrostats contained in it would take place, and it would act just like a simple gyrostat. If, however, the case were to undergo translation in any direction except along the axis, the gyrostats would lag behind, and the two-link chain which they form would bend at the centre. This bending would be resisted by the quasi-rigidity of the chain produced by the rotation, and the gyrostats would react on the sheath at the joints with forces as before at right angles to the plane in which the change of direction of the axis takes place.

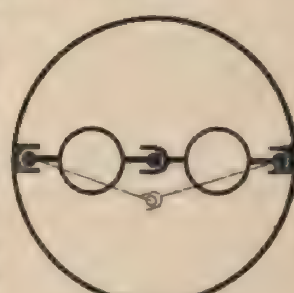

Fig. 15.

The general result is, that if the centre of this molecule be carried with uniform velocity in a circle in a plane at right angles to the line

of axes, the force required for the acceleration towards the centre, and which is applied to it by the medium, is greater or less according as the direction in which the molecule is carried round is with or against the direction of rotation of the gyrostats. That is, the effect of the rotation is to virtually increase the inertia of the molecule in the one case and diminish it in the other.

These molecules embedded in the medium are supposed to be exceedingly small, and to be so distributed that the medium may, in the consideration of light propagation, be regarded as of uniform quality. Lord Kelvin's last form of molecule, it may be pointed out, if the surface of its sheath adheres to the medium, will have efficiency as an ordinary single gyrostat as regards rotations of the molecule,

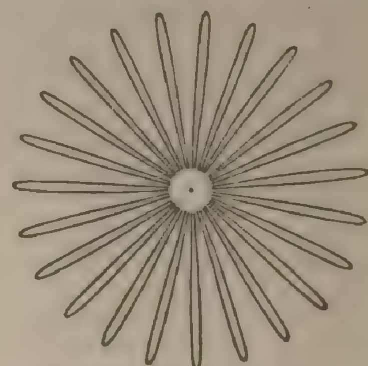

Fig. 16.—Path of the Bob of a Gyrostatic Pendulum.

As the pendulum moves, it passes from one ray to another on the opposite side, and the direction of motion at each swing alters through the angle between two rays. The central parts of the rays are left out. The marking point does not pass exactly through the centre.

and efficiency likewise as regards translational motion of the centre of the molecule. The former efficiency can be made as small as may be desired by making the molecule sufficiently small: the latter may be maintained at the same value under certain conditions, however small the molecule be made.

The lately discovered effect of a magnetic field in giving one period of circular oscillation of a particle or another according as the particle is revolving in one direction or the other about the direction of the magnetic force, is connected with magneto-optic rotation. There is a connection between velocity of propagation and frequency of vibration, which is exemplified by the phenomena of dispersion. In the Faraday effect, the two modes of vibration, if of the same period, have different velocities of vibration, consequently these two modes

of vibration must have different frequencies for the same velocity of propagation.

The vibrations of the molecules of a gas in which the Zeeman effect is produced by a magnetic field may be represented by the motion of a pendulum the bob of which contains a rapidly rotating gyrostat with its axis in the direction of the supporting wire of the pendulum. The period of revolution of the bob when moving as a conical pendulum is greater or less than the period when the gyrostat is not spinning according as the direction of revolution is against or with the direction of rotation.

The bob when deflected and let go moves in a path which constantly changes its direction, so that if a point attached to the bob writes the path on a piece of paper, a star-shaped figure is obtained. I cause the gyrostatic pendulum here suspended to draw its path by a stream of white sand on the blackboard placed below it, and you see the result.

I must here leave the subject, and may venture to express the hope that on some other occasion some one more specially acquainted with the electromagnetic aspects of the phenomenon may be induced to place the latest results of that theory before you.

[A. G.]

ANNUAL MEETING,

Monday, May 2, 1898.

Sir James Crichton-Browne, M.D. LL.D. F.R.S. Treasurer and Vice-President, in the Chair.

The Annual Report of the Committee of Visitors for the year 1897, testifying to the continued prosperity and efficient management of the Institution, was read and adopted, and the Report on the Davy Faraday Research Laboratory of the Royal Institution, which accompanied it, was also read.

Sixty-six new Members were elected in 1897.

Sixty-three Lectures and Nineteen Evening Discourses were delivered in 1897.

The Books and Pamphlets presented in 1897 amounted to about 260 volumes, making, with 632 volumes (including Periodicals bound) purchased by the Managers, a total of 892 volumes added to the Library in the year.

Thanks were voted to the President, Treasurer, and the Honorary Secretary, to the Committees of Managers and Visitors, and to the Professors, for their valuable services to the Institution during the past year.

The following Gentlemen were unanimously elected as Officers for the ensuing year:

President—The Duke of Northumberland, K.G. D.C.L. LL.D.
Treasurer—Sir James Crichton-Browne, M.D. LL.D. F.R.S.
Secretary—Sir Frederick Bramwell, Bart. D.C.L. LL.D. F.R.S. M. Inst. C.E.

Managers.

Sir William Crookes, F.R.S.
Sir Edward Frankland, K.C.B. D.C.L. LL.D. F.R.S.
The Right Hon. George Joachim Goschen, M.P. D.C.L. LL.D. F.R.S.
Donald William Charles Hood, M.D. F.R.C.P.
Sir William Huggins, K.C.B. D.C.L. LL.D. F.R.S.
David Edward Hughes, Esq. F.R.S.
Alfred B. Kempe, Esq. M.A. F.R.S.
Hugh Leonard, Esq. M. Inst. C.E.
Thomas John Maclagan, M.D.
Ludwig Mond, Esq. Ph.D. F.R.S.
Alexander Siemens, Esq. M. Inst. C.E.
The Hon. Sir James Stirling, M.A. LL.D.
Sir Henry Thompson, F.R.C.S. F.R.A.S.
Sir Richard Everard Webster, G.C.M.G. M.P. Q.C. LL.D.
Sir William Henry White, K.C.B. LL.D. D.Sc. F.R.S.

Visitors.

Sir Alexander Richardson Binnie, M. Inst. C.E. F.G.S.
Sir James Blyth, Bart. J.P.
Charles Vernon Boys, Esq. F.R.S.
Edward Dent, Esq.
James Edmunds, M.D. M.R.C.P.
Maures Horner, Esq. F.R.A.S.
Edward Kraftmeier, Esq.
Sir Francis Laking, M.D.
T. Lambert Mears, Esq. M.A. LL.D.
Lachlan Mackintosh Rate, Esq. M.A.
John Callander Ross, Esq.
William James Russell, Esq. Ph.D. F.R.S.
Sir James Vaughan, B.A. J.P.
James Wimshurst, Esq.
Alfred Fernandez Yarrow, Esq. M. Inst. C.E.

WEEKLY EVENING MEETING,

Friday, May 6, 1898.

Sir William Crookes, F.R.S. Vice-President, in the Chair.

Edward A. Minchin, Esq. M.A. Fellow of Merton College, Oxford.

Living Crystals.

Crystals are a class of bodies distinguished by many remarkable properties. Their definite symmetrical forms, limited by plane surfaces meeting at sharp angles, in conformity with some easily recognisable type of geometrical figure; their peculiarities of cleavage and etching; their growth and individuality, most strikingly manifested in their power of regeneration; and finally, their optical properties; each and all of these characteristics sufficiently mark out the crystal from the non-crystalline body. None of these qualities, however, are in any way due to the action of life. An ordinary crystal owes its peculiar characteristics entirely to the action of the laws of inorganic matter, laws which admit of being clearly formulated and accurately calculated.

Crystalline bodies are known, however, to occur which have been deposited within living bodies, and which owe their origin to vital activities. In such cases the crystal, while identical in its chemical composition and molecular structure with crystals of inorganic origin, may exhibit at the same time certain peculiarities which are due entirely to the circumstances of its origin. In this way an opportunity is afforded of making an interesting and important comparison. On the one hand we have the inorganic crystal, owing its striking properties to the action of physical laws which can be defined, calculated and artificially reproduced. On the other hand we have the living crystal, as it may be termed ("biocrystal" Haeckel), which exhibits certain additional features, the result of its origin amidst conditions which no one has succeeded as yet in imitating or explaining. The resemblances between the two kinds of crystal are such as are due to the intrinsic properties of the material composing them; the differences must therefore be the effect of differences in the surroundings in which the crystals arise. In other words, those points in which a living crystal differs from a crystal of the same kind, but of inorganic origin, must depend on the different activities of living and lifeless matter. Hence a careful examination of the peculiarities of the living crystal might be expected to throw considerable light upon the nature of life and the properties of living matter.

As an instance of a crystalline body which occurs both as an inorganic substance and as a living crystal, we may take calcite,

sufficiently well known as a mineral, and forming also the skelet many forms of animal life. In the latter condition it can be studied in the very simple group of organisms known as Ascon most primitive order of calcareous sponges.

In Ascons, as in other calcareous sponges, the skeleton is ma of minute splinters or spicules of calcite, which always conform t of three types of form; (1) rod-like or needle-shaped spicules, u more or less curved, and always with unlike ends; (2) three-ray triradiate spicules, having each three rays meeting at a central p and (3) four-rayed or quadriradiate spicules, consisting each basal system of three rays, exactly similar to the triradiate spi and an additional or fourth ray tacked on to it. The three basal may therefore be termed the *triradiate system* in the three-rayed four-rayed spicules alike, irrespective of the presence or abse the fourth ray.

With regard to the triradiate systems, it may further be n that three classes can be distinguished amongst them. Sometim three rays are unequal in size, and irregular in arrangement, m a figure which is quite asymmetrical; such forms are, however, paratively rare. More usually the triradiate systems exhibit a de symmetry which follows one of two patterns. In the first plac rays may meet at equal angles, so that, irrespective of the un development of the rays themselves, the spicule is symmetrical three planes. In the second place, the angles may be such tha spicule shows a marked bilateral symmetry, having an unp and two paired angles, with corresponding unpaired and paired Thus irregular, regular and sagittal forms of the triradiate sy can be distinguished, each of which may have an extra ray t on, and so become quadriradiate. The fourth ray may be str or curved, long or short, smooth or spined, but all its variat are quite independent of the variations of the rays of the l system.

Although the spicules of Ascons often exhibit very definite symmetrical patterns, it is obvious that their forms do not in the resemble those of the inorganic calcite crystal, and from their out appearance it would be impossible even to suspect them to have thing in common with the calcite crystal. In fact, several fea seen in the spicules in question are the exact opposite of those ch teristic of crystals. Few things are so remarkable in crystals a fact that their parts are so connected together that one part c vary independently of other parts, a property well seen in the regulating the addition of new faces during growth. But in spicule any part can vary independently of the rest. The rod forms always have the two ends unlike; the triradiate may hav the rays unlike, and of different sizes; and it is the rarest thi find a quadriradiate with the apical ray similar to the basal rays.

In spite of their remarkable divergence from the usual crysta form, however, it is easy to prove not only that the spicules

crystals, but also that each one is a single crystal, a fact discovered independently by Sollas and Ebner. Their crystalline nature is shown both by their behaviour to polarised light and by etching experiments. They do not answer to the cleavage test so satisfactorily, probably on account of the organic matter with which their substance is interpenetrated. But other tests show them to be true calcite crystals, distinguished, however, by a peculiar form, which can best be illustrated by imagining each spicule to have been, as it were, cut by a lapidary out of a single block of crystal, just as a diamond is cut into a faceted form which is not that of the natural diamond crystal. This comparison must only be taken as an illustration, however, and not as a description of how the spicule is formed, for it is not carved out of a block, but is built up to its shape, just as a stone house is not hewn out of solid stone, but built up of separate stones.

It is seen that the great difference between the living and the lifeless crystal is one of external form. In view of the regularity and symmetry of the calcite crystal, and the very precise geometrical laws that govern its form, the differences in this respect exhibited by the living crystal become very striking. It is evident that some disturbing influence must be at work which interferes with the natural development of the crystal. We know that if a calcite crystal develops of itself, it assumes a certain form. In order to discover what has caused the living crystal to take on its curious and unusual growth, we must examine the conditions under which it has arisen. Hence it is now necessary to leave for a moment the crystalline aspect of these spicules and look at them from another point of view, as portions of a living body. To do this we must understand something of the animal which has produced them and the part which they play in its internal economy.

The simplest calcareous sponge or Olynthus is an organism very easy to understand. It can be compared to a thin-walled vase, with a wide opening at the top, and a great many minute openings or pores on the sides. During life an internal mechanism produces a current of water which flows in through the pores into the cavity and passes out by the opening or osculum at the summit. All calcareous sponges start life in this condition, and the form and structure, whatever it may be, which they have when full grown depends simply on the manner in which the Olynthus grows. Hence this organism may be considered as representing probably the primitive type of sponge which was the ancestor of the whole group, and which is not found anywhere at the present day as an adult form, but occurs always in the life-history as a transitory stage, in which the structure of the sponge is found reduced to its simplest terms.

Now the wall of the young sponge is very thin and delicate, and could not support itself were it not for the spicules which stiffen it. When the body wall is examined more closely it is seen that the

form and arrangement of the spicules have a definite relation to its structure. In the simplest cases only triradiates are present, and then they are arranged in a single layer, all placed with one ray pointing downwards, away from the opening at the top. The rays of different spicules overlap and cross one another, and so produce a sort of lattice-work, with meshes rather like a honeycomb. In the meshes are placed the pores, and at first the arrangement is such that there are the same number of pores and spicules, the result being that each spicule has a pore in each of the interspaces between the arms. As the sponge grows, however, new pores and new spicules are constantly being formed, so that the simple arrangement is upset to some extent, though the same general pattern can be made out. When an extra fourth ray is added on to the triradiate system, it is always placed so as to project into the cavity, and if the extra ray is curved, it always points up towards the large opening at the top. If simple needle-shaped spicules are present they are always placed on the outside, with the straight portion of the shaft embedded in the wall, and the curved portion sticking out into the water.

The relation of the spicules to the structure of the sponge shows that they have a definite function to perform and an important part to play in the economy of the organism that has produced them. Their function is partly one of support, partly one of protection. Given a vase-like organism, with a thin porous wall, what are the architectural requirements of a supporting and protecting framework for it, supposing that for the material of the framework rods of calcite are to be employed? The simplest solution of the problem would be to place the rods in the body wall, so that one or more come to lie between each of the pores. Such an arrangement would, however, be far from perfect, since on the one hand a skeleton of loose unconnected rods is not very strong, and on the other hand it does not afford any protection. Hence the next step in the evolution of the framework is, on the one hand, to bend some of the rods so that they point outwards, and so cover the outside with a forest of sharp spikes; and, on the other hand, to join up some of the loose rods in the wall and unite them into composite systems. Now of all the systems that could be devised by joining rods together, none could be more suited to the type required than the triradiate figure produced by joining three rods only. In the first place each triradiate corresponds perfectly to the natural interspaces between the pores, which if disposed so as to best economise space, take on an arrangement in alternating rows, so that each pore is surrounded by six others at equal distances, forming a hexagon. In short, the arrangement of the pores repeats the familiar problem of the angles of the cells of the honeycomb, and the triradiate spicules correspond exactly to the interspaces. Secondly, it must be remembered that the sponge has to live in waves and currents, and its framework requires a certain amount of flexibility as well as strength. This condition also is best

fulfilled by the triradiate systems, which, while supporting the wall, allow it a great deal of freedom to bend and yield under the action of powerful currents. Were the rods united into more extensive systems, however, so as to form lattice plates or a continuous trellis-work, we should get a framework of greater strength but of dangerous brittleness, unable to withstand any violent shock. It is easy to understand, therefore, the evolution of the curved, rod-like spicules on the one hand, and the triradiate systems on the other. The next problem is to plan out a scheme of defence for the inner surface like the palisade with which the exterior is defended. This, of course, is easily done by making some of the rods project into the interior. But for reasons of internal economy it would be inconvenient for the spikes on the inner surface to slant out from it like those outside. Considerations of interior comfort require here that the spikes should start straight out from the wall, even though they curve at their tips. Now the spikes require support, and this cannot be obtained in the soft wall of the sponge, too thin to hold firmly a spicule stuck at right angles to its surface. These difficulties are overcome, however, by the upright spike being stuck on to the triradiate system, and this done, the result is at once a quadriradiate spicule, a great addition to the strength and stability of the sponge structure. For, in the first place, the quadriradiates constitute a formidable armament to obstruct the entrance of intruders. In the second place they fit in, so to speak, with a method by which the sponge is accustomed to protect itself against hard times. When exposed to unfavourable conditions, Ascons contract themselves very greatly and so become much more rigid, since their wall becomes much thicker and their cavity much smaller, sometimes vanishing altogether. When a sponge with quadriradiate spicules contracts to a certain point, the projecting rays interlock in the interior of the cavity, and, in this way the fragile organism attains a much greater rigidity and power of resistance to the action of external forces.

It is thus seen that the three classes of spicules are just those which are best fitted for supporting and protecting an organism having the structure of the simple sponge or Olynthus, which has been described. But this process of adaptation can be traced still further. It has already been pointed out that the symmetrical triradiate systems can be divided into two classes, sagittal and regular. To understand the significance of these two forms it is necessary to glance at the further growth of the Olynthus.

In Ascons, the primitive vase-like organism elongates, while at the same time its wall becomes folded and bulged out to form hollow outgrowths, each like the finger of a glove. The outgrowths continue to increase in length and become branched, and finally join together so that a network of hollow tubes is formed, clustered round the primitive osculum of the Olynthus, and also giving rise to new oscula of the same kind, which rise up from the network like

chimneys. In this peculiar growth two distinct types are found. In one type (*Clathrina*) the tubes form a close network opening by a few short oscula, usually very inconspicuous. In the other type (*Leucosolenia*) the sponge has a more erect form and consists chiefly of the conspicuous chimneys, united by an inconspicuous network of small tubes. Now in the former type of architecture the pressures and strains in the network of tubes are different at different spots, and cannot be said to predominate in one direction more than another. Hence, in Clathrina, we might expect to find a type of spicule adapted to these conditions, and as a matter of fact, the predominant spicule here is the triradiate with equal rays and equal angles: that is to say, an evenly balanced form fitted to resist tensions in any direction equally. But occasionally a Clathrina grows in a more erect and stalked form, and then strains in a vertical direction predominate; in such a case (e.g. *Cl. blanca*, *Cl. lacunosa*) the arm of the spicule, which is placed vertically, becomes greatly strengthened, especially in certain regions, the other two arms remaining small, sometimes very much so. In all cases, however, the equal angles are still retained.

In Leucosolenia, on the other hand, the erect growth requires strengthening chiefly in a vertical direction, and the form of the triradiate spicule is at once seen to correspond with this, having paired angles and a form which at once suggests adaptation to pressure in one direction rather than another. The spicules are placed with great regularity, the unpaired ray directed vertically, and the paired rays horizontally, so that the whole forms a beautiful basket-work, stiffened by vertical ribs and held together by horizontal girders. It is thus seen that even subordinate peculiarities of form have their special uses, which are evident when studied in connection with the architectural requirements of the whole organism.

The result, therefore, of an inquiry into the relations between the living crystals and the organism by which they are formed, is as follows: that both in their form and arrangement the spicules represent a most exquisite piece of engineering, and are to be regarded as adapted to support and protect the fragile and delicate body wall. Moreover, the history which has been traced for the development of the spicules is shown to be not altogether imaginary by the facts of the development of the spicules, which may now be briefly considered.

The calcareous spicules are formed within cells, derived from the external layer of the body wall, but each ray or branch owes its origin to a distinct cell. In the simplest case one cell forms a single rod-like spicule, and when a very large rod is to be formed, the mother cell may multiply into two or more daughter cells. When a triradiate is to be formed, three mother cells come together, one for each ray, and after each has divided into two daughter cells, they secrete three separate rods, which sooner or later become joined together to form the spicule. When a quadriradiate is to be formed,

a remarkable series of events takes place. First, three cells come together and form a triradiate system in the usual way. Then a cell is given off by the division of the nearest pore cell, and this cell travels to the little triradiate spicule and takes up a position over it, on its inner side. Then the cell secretes a little rod of calcite, which is stuck on to the triradiate system, converting it into a four-rayed spicule, so that not only is the fourth ray a late addition to the basal system, but it is derived from quite a different source, the basal rays being formed by cells of one class, the fourth ray by a cell of a different class. The development of the triradiate and quadriradiate spicules shows them, in fact, to be composite bodies, built up of a number of skeletal elements, each a simple rod. This is remarkable, and even paradoxical, in view of the fact already mentioned, that each spicule, when full grown, is a single crystal. In their earliest stages, however, it is found that the minute triradiate systems are at first non-crystalline, and only become so after the rays have been joined together. Then, since all parts are in continuity, the crystallisation takes place in such a way that all parts of the spicule have a uniform molecular arrangement, producing not three or four separate crystals, as might at first sight have been expected, but a single one.

It is seen, therefore, that the primitive skeletal element in Ascons is a simple rod, and that the general course of evolution was such as has been traced out, some rods remaining single but growing out from the surface; others becoming arranged in trios and forming triradiate systems; and others, again, becoming tacked on to the triradiates to form the four-rayed spicules. But how did the primitive skeletal elements, the rods, themselves originate? Unless an intelligible origin can be suggested for them, there is a gap in the scheme of evolution. Now any living organism, however simple, is composed of matter which is in process of constant change and transmutation. As a result of metabolism, substances of all kinds are continually being formed, and amongst them many of crystalline nature, which may be deposited from a state of solution and crystallise out. Hence it is not uncommon to find ordinary crystals in living tissues, crystals which show no sign of having any origin at all out of the common, and which must be supposed either to be of no use to the organism that produced them, or at least to perform some function for which their external form is not of great importance. It is a rational supposition, therefore, that the spicules of Ascons also had at one time the form as well as the constitution of crystals, and originated simply as bye-products, so to speak, of the wear and tear of the living substance. When, however, it became of importance to the organism that they should have one form rather than another, then their natural form became modified and completely altered. Now this is the most obscure portion of all their history, how, namely, the living substance can so act upon the growing crystal as to cause it to assume a form

which is not that which it would naturally assume. We can observe that it does so, and that not only in this, but in many other cases, living bodies appear to have the power of modifying and transforming their component materials in a way which we are far from understanding. No sooner, however, is this mysterious change effected than the crystal has crossed, so to speak, the line which separates the living from the lifeless world, and must now be regarded from an entirely different standpoint, that is to say, as a part of a living body. As such it is subject to new influences and is governed by new laws which, as it were, override those by which the lifeless crystal is ruled. In the first place, it must be supposed that each spicule, had it been deposited in an inorganic matrix, would have had the characteristic contours of an ordinary crystal of calcite. This receives, in fact, further proof from the interesting observations of Sollas, who showed that upon sponge spicules placed in a solution of carbonate of lime new layers of calcite are deposited, which tend to restore the ordinary crystalline form. Instead of that, however, it has a form which cannot be brought into any relation with its intrinsic crystalline properties. It is true that the attempt has been made to explain the symmetry often exhibited by the spicules as due to their crystalline nature. Not only, however, can any such explanation be shown to be inadequate in itself, but it is also quite unnecessary, since in other sponges, spicules even more symmetrical may occur, which are manufactured, so to speak, out of a non-crystalline material, namely, colloid silica. The symmetry and regularity of form which sponge spicules often possess are clearly, therefore, not due to the inherent properties of the material of which they are composed, but to the action of the living matrix in which they are deposited. The symmetry of a crystal, on the other hand, is one which in its fundamental traits is entirely independent of the matrix in which it is deposited. We have seen further that in a natural crystal the parts cannot vary independently. But in the living crystals every part varies independently of all the others, according to the needs of the organism, and the spicules can be traced through a long series of evolutionary changes, resulting in the many different forms with which we are acquainted.

We may therefore sum up with regard to these living crystals as follows. Their constitution is that of the calcite crystal, but their external form is that which the sponge requires, and not that which they would naturally assume. They furnish us, in fact, with a beautiful instance of what is termed adaptation, that is to say, the fact that any living organism tends to have just that form, structure and organisation in all its parts which it requires in order to maintain its existence in its peculiar mode of life, whatever it may be.

The principle of adaptation raises many scientific and philosophical questions of great importance, but certain points may be emphasised which have been seen in the instances under discussion. In the first

place, it is very evident that these adaptations did not come into existence suddenly, like an instantaneous photograph as it were, but are the result of a long and gradual course of evolution from the simple crystal, formed, so to speak, almost by chance in the molecular ferment and turmoil that goes on in the living organism, up to the highly perfected and elaborated forms of spicules which compose the supporting framework in different species of sponges. In the second place, the persistence of different species of sponges in certain grades of evolution shows that the adaptation in any given case is not to be regarded as perfect, but only as slightly better or worse than that seen in other species. This points to the main factor in the evolution having been the natural selection consequent upon competition and the struggle for existence.

[E. A. M.]

GENERAL MONTHLY MEETING,

Monday, May 9, 1898.

SIR JAMES CRICHTON-BROWNE, M.D. LL.D. F.R.S. Treasurer and Vice-President, in the Chair.

The following Vice-Presidents for the ensuing year were announced:—

Sir William Crookes, F.R.S.
Sir Edward Frankland, K.C.B. D.C.L. LL.D. F.R.S.
Sir William Huggins, K.C.B. D.C.L. LL.D. F.R.S.
Ludwig Mond, Esq. Ph.D. F.R.S.
The Hon. Sir James Stirling, M.A. LL.D.
Sir Henry Thompson, F.R.C.S. F.R.A.S.
Sir James Crichton-Browne, M.D. LL.D. F.R.S. *Treasurer.*
Sir Frederick Bramwell, Bart. D.C.L. LL.D. F.R.S. *Honorary Secretary.*

Hugh Bell, Esq.
Henry Marc Brunel, Esq. M. Inst. C.E.
Bailey Knight, Esq.
Lionel Phillips, Esq.
Alfred Morton Smale, Esq. M.R.C.S.

were elected Members of the Royal Institution.

The Right Hon. Lord Rayleigh was re-elected Professor of Natural Philosophy in the Royal Institution.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Secretary of State for India—Archæological Survey of India. Revised List of Antiquarian Remains in the Bombay Presidency. By H. Cousens. 4to. 1897.
Monumental Remains of the Dutch East India Company in Madras. By A. Rea. 4to. 1897.
Accademia dei Lincei, Reale, Roma—Classe di Scienze Fisiche, Matematiche e Naturali. Atti, Serie Quinta: Rendiconti. 1º Semestre, Vol. VII. Fasc. [illegible]
Asiatic Society, Royal—Journal for April, 1898. 8vo.
Astronomical Society, Royal—Monthly Notices, Vol. LVIII. No. [illegible] 8vo. 1898.
Boston Public Library—Monthly Bulletin, Vol. III. No. 4. 8vo. 1898.
[illegible] Charles, Esq. F.A.S.E. (the Author)—Science and Engineering, [illegible] 8vo. 1898.
British Architects, Royal Institute of—Journal, 3rd Series, Vol. V. Nos. [illegible] 4to. 1898.
Cambridge Philosophical Society—Transactions, Vol. XVI. Part [illegible] 4to. 1898.
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Chicago, John Crerar Library—Third Annual Report. 8vo. 1898.
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Analyst for April, 1898. 8vo.
Anthony's Photographic Bulletin for April, 1898. 8vo.
Astro-physical Journal for April, 1898. 8vo.
Athenæum for April, 1898. 4to.
Author for April, 1898. 8vo.
Bimetallist for April, 1898. 8vo.
Brewers' Journal for April, 1898. 8vo.
Chemical News for April, 1898. 4to.
Chemist and Druggist for April, 1898. 8vo.
Education for April, 1898.
Electrical Engineer for April, 1898. fol.
Electrical Engineering for April 15, 1898. 8vo.
Electrical Review for April, 1898. 8vo.
Electricity for April, 1898. 8vo.
Engineer for April, 1898. fol.
Engineering for April, 1898. fol.
Homœopathic Review for April, 1898. 8vo.
Horological Journal for April and May, 1898. 8vo.
Industries and Iron for April, 1898. fol.
Invention for April, 1898.
Journal of Physical Chemistry for February, March and April, 1898. 8vo.
Journal of State Medicine for April, 1898. 8vo.
Law Journal for April, 1898. 8vo.
Lightning for April, 1898. 8vo.
Machinery Market for April, 1898. 8vo.
Nature for April, 1898. 4to.
New Church Magazine for April, 1898. 8vo.
Nuovo Cimento for Feb. 1898. 8vo.
Photographic News for April, 1898. 8vo.
Physical Review for March, 1898. 8vo.
Public Health Engineer for April, 1898. 8vo.
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Travel for April, 1898. 8vo.
Tropical Agriculturist for April, 1898.
Zoophilist for April, 1898. 4to.
East India Association—Journal, Vol. XXX. No. 13. 8vo. 1898.
Electrical Engineers, Institution of—Journal, Vol. XXVII. Nos. 133, 134. 8vo. 1898.
Ellis, G. B. Esq. (the Author)—The Merchandise Marks Act. 8vo. 1898.
Florence, Biblioteca Nazionale Centrale—Bolletino, Nos. 295, 296. 8vo. 1898.
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American Chemical Journal, Jan.–April, 1898. 8vo.
Jordan, Wm. L. Esq. (the Author)—The Standard of Value. 7th ed. 8vo. 1896.
Kerntler, Franz, Esq. (the Author)—Die Möglichkeit einer experimentellen Entscheidung zwischen den verschiedenen elektrodynamischen Grundgesetzen. 8vo. 1898.
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Festrede, 14 Nov. 1896. 4to. 1897.
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Obach, Dr. Eugene F. A. M.R.I. (the Author)—Gutta Percha (Cantor Lectures). 8vo. 1898.
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Sidgreaves, Rev. Father, F.R.A.S.—Results of Meteorological and Magnetical Observations at Stonyhurst College Observatory, 1898. 8vo. 1898.
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The Smithsonian Institution, 1846–96. The History of its first Half Century. Edited by G. B. Goode. 8vo. 1897.
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Statistical Society, Royal—Journal, Vol. LXI. Part 1. 8vo. 1898.
Stewart, Charles, Esq. (the Author)—Stewart's Telegraphic Code. 8vo. 1897.
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Official Gazette, Vol. LXXXII. No. 13; Vol. LXXXIII. No. 1. 8vo. 1898.
Upsal, L'Observatoire Météorologique—Bulletin Mensuel, 1897. 4to. 1897–98.
Victoria Institute—Journal, No. 119. 8vo. 1898.
Zoological Society of London—Report for 1897. 8vo. 1898.

WEEKLY EVENING MEETING,

Friday, May 13, 1898.

Ludwig Mond, Esq. Ph.D. F.R.S. Vice-President, in the Chair.

Professor W. A. Tilden, D.Sc. F.R.S.

Recent Experiments on Certain of the Chemical Elements in relation to Heat.

The discovery that different substances have different capacities for heat is usually attributed to Irvine, but there can be no doubt that Black, Crawford and others contributed to the establishment of the idea. The fact that equal weights of different substances, in cooling down through the same number of degrees, give out different amounts of heat, may be illustrated by the well-known experiment, in which a cake of wax is penetrated with different degrees of rapidity by balls of different metals heated to the same temperature. But, for the quantitative estimation of the amounts of heat thus taken up and given out again—that is, the *specific heats*—the physicist must resort to other forms of experiment, each of which presents difficulties of its own. Broadly speaking, three principal methods have been used in the past for this purpose. The first is based upon the observation of the exact change of temperature produced in a known mass of water, by mixing with it a known weight of the substance previously, at a definite temperature above or below that of the water. The second consists in determining the quantity of ice melted, when the heated body is brought into contact with it in such a way that no heat from any other source can reach the ice. And the third method consists in observing the rate at which the temperature of the heated body falls through a definite range of degrees, when suspended in a vacuous space, as compared with the rate of cooling of another body taken as the standard.

The process of intermixture with water was used by the earlier experimenters in the last century, and some of the best results extant have been obtained by this method, which, however, is not so easy as it appears when the highest degree of accuracy is desired.

Lavoisier and Laplace, in 1780, devised the ice calorimeter which bears their name; and in a most interesting memoir, which is reprinted among Lavoisier's works, they show that they were familiar with the idea which in modern times is expressed as the principle of the conservation of energy. In this memoir they give the results of experiments, in which the specific heats of iron, mercury and a

few other substances are estimated with a very tolerable approach to accuracy. Although many of the metals were known to them, and supposing they had persisted in this work, it would not have been possible for them to make the discovery which was reserved for Dulong and Petit thirty-five years later, for the atomic theory had not then been conceived, and no elemental combining proportions had been determined.

Dulong and Petit * seem to have used at first the method of mixtures, and to have found, by direct experiment, that the specific heat of solids (metals and glass) increases with the temperature. They also studied (after Leslie) the laws of cooling of bodies; and two years after the publication of their first paper on the subject, they (Petit and Dulong, *sic*) arrived at the remarkable general expression which is associated with their names.†

After pointing out that all the results of previous experiments except those of Lavoisier and Laplace are extremely incorrect, they describe their own conclusions obtained by the method of cooling, conducted with many precautions to avoid error. The numerical expression of their experimental results is given in the following table:—

COPY OF TABLE BY PETIT AND DULONG.

(Ann. Chim. Phys. 1819, x. 403.)

—	Specific Heats.	Atomic Weights (O = 1).	Atomic Weight × Specific Heat.
Bismuth	·0288	13·30	·3830
Lead	·0293	12·95	·3794
Gold	·0298	12·43	·3704
Platinum	·0314	11·16	·3740
Tin	·0514	7·35	·3779
Silver	·0557	6·75	·3759
Zinc	·0927	4·03	·3736
Tellurium	·0912	4·03	·3675
Copper	·0949	3·957	·3755
Nickel	·1035	3·69	·3819
Iron	·1100	3·392	·3731
Cobalt	·1498	2·46	·3685
Sulphur	·1880	2·011	·3780

The statement of the relation indicated in the last column of figures is expressed in the following words of the authors, p. 405: "Les atomes de tous les corps simples ont exactement la même capacité pour la chaleur."

Here the question rested, till resumed many years later (1840) by Regnault, who in his first memoir ‡ pointed out the difficulties which

* Ann. Chim. 1817, vii. 144. † Ibid. 1819, x. 395. ‡ Ibid. 73, 5.

attend the acceptance of the statement of Petit and Dulong in the form in which they gave it. He then discussed the three principal experimental methods: viz. (1) fusion of ice; (2) mixture with water or other liquid; and (3) cooling; and decided in favour of the second, which he used throughout his researches. The general form of the apparatus used by the great physicist has been a model for the guidance of successive experimentalists since his time.

Another quarter of a century elapsed before the question of the specific heats of the elements was resumed by Hermann Kopp. His results were communicated to the Royal Society, and are embodied in a paper printed in the 'Philosophical Transactions' for 1865. After reviewing the work of his predecessors, he described a process by which he had made a large number of estimations of specific heat, not only of elements, but of compounds of all kinds in the solid state. Concerning his own process, however, he remarks that "The method, as I have used it, has by no means the accuracy of that of Regnault" (p. 81).

In 1870 Bunsen introduced his well-known ice calorimeter. This is an instrument in which the amount of ice melted by the heated body is not measured by collecting and weighing the water formed, but by observing the contraction consequent upon the change of state. The results obtained by Bunsen himself are uniformly slightly lower than those of Regnault for the same elements.

Since that time, experiments have been made by Weber, Dewar, Humpidge and others, in connection especially with the influence of temperature in particular cases.

Setting aside the elements, carbon, boron, silicon and beryllium, as providing an entirely separate problem, the question is whether the law of Dulong and Petit is strictly valid when applied to the metals. Kopp, in the discussion of his subject, came to the conclusion that it is not; but the grounds for this conclusion are unsatisfactory, since neither the atomic weights nor the specific heats were at that time known with sufficient accuracy. It has been customary to assume that the divergences from the constant value of the product, At. Wt. × Sp. Ht., are due partly to the fact that at the temperature at which specific heats are usually determined, the different elements stand in very different relations to their point of fusion: thus, lead at the temperature of boiling water is much nearer to its melting point than iron under the same conditions. The divergences have also been attributed to temporary or allotropic conditions of the elements. As to the relation to melting point, the specific heats of atomic weights seem to be practically the same in separate metals and alloys of the same which melt at far lower temperatures. For example, the atomic heat of cadmium is 6·35; of bismuth 6·47; of tin, 6·63; and of lead, 6·50; while the mean atomic heat in alloys of bismuth with tin and lead with tin ranges from 6·40 to 6·66 (Regnault), which is practically the same. Again, while the melting point of platinum is at a white heat, the metal becomes plastic at a low red heat, and yet the

specific heat at this lower temperature is very little less than it is near the melting point. The properties of many other metals, notably zinc and copper, change considerably at temperatures far removed from their melting points without substantial change in their capacity for heat.

As to allotropy, it is a phenomenon which is comparatively rare among metals, and in the marked cases in which it occurs we have no information as to the value of the specific heats in the several varieties, such as the two forms of antimony and the silver-zinc alloy of Heycock and Neville, and they may be left out of account. Bunsen compared the so-called allotropic tin, obtained by exposing the metal to cold for a long time, and found it ·0545 against ·0559 for the ordinary kind.* In dimorphous substances there is often no difference. Regnault found for arragonite ·2086 and for calcite ·2085 respectively. The differences between metals hammered and annealed, hard and soft, were also found by Regnault to be very small. †

Hard steel	·1175.	Same, softened	·1165
Hard bronze	·0858.	Same, softened	·0862

Kopp came to the conclusion, *first*, that each element in the solid state, and at a sufficient distance from its melting point, has *one* specific or atomic heat, which varies only slightly with physical conditions; and *secondly*, that each element has essentially the same specific or atomic heat in compounds as it has in the free state. This last is practically identical with the statement which is known as Neumann's law. With Kopp's conclusion I agree, but, from some of Regnault's results coupled with my own, the effect of *small* quantities of carbon and perhaps of sulphur upon the specific heats of metals is greater than has been supposed. If we take the results of Regnault and of Kopp and combine them with the most accurately known atomic weights, the products are still not constant.

Atomic Weights most accurately known (1897) combined with Specific Heats.

	A.W. (H=1).	S. H. Regnault.	S. H. Kopp.	At. Ht. Regnault.	At. Ht. Kopp.
Copper	63·12	·09515	·0930	6·01	5·87
Gold	195·74	·03244	..	6·35	..
Iron	55·60	·11379	·1120	6·33	6·23
Lead	205·36	·03140	·0315	6·45	6·47
Mercury liq.	198·49	·03332	..	6·61	..
,, − 78° to + 10° sol.	198·49	·03192	..	6·34	..
Silver	107·11	·05701	·0560	6·11	6·00
Iodine	125·89	·05412	..	6·81	..

* Pogg. Ann. 141, 27. † Ann. Chim. [3], ix.

The "Law" of Dulong and Petit is therefore only an approximation; but this may perhaps be due to inaccuracy in the estimation of the specific heat, owing to impurity in the material used. That is the problem which I have endeavoured to solve.

The introduction by Professor J. Joly of a new method of calorimetry, which depends upon the condensation of steam upon the cold body, and the excellent results obtained by the Author in the use of the differential form of his instrument,* led me to think that with due attention to various precautions—such as exact observations of the temperatures, and practice in determining the moment at which the increase of weight due to condensation is completed—results of considerable accuracy might be obtained.

The problem is to find two elements, very closely similar in density and melting point, which can be obtained in a state of purity, and then to determine with the utmost possible accuracy the specific heat of each under the same conditions.

The two metals cobalt and nickel were selected for the purpose. They were examined by Regnault, but the metals he used were very impure.

The cobalt employed in my experiments was prepared by myself. For the nickel I am indebted to Dr. Ludwig Mond. Both were undoubtedly much more nearly pure than any metal available in Regnault's time. The results obtained are as follows:—

SPECIFIC HEATS OF COBALT AND NICKEL.

Pure fused.

Cobalt, S. G. $\frac{21°}{4°}$, 8·718.	Nickel, S. G. $\frac{21°}{4°}$ 8·790.
·10310	
·10378	
·10310	·10953 } too high?
·10355	·10910 } too high?
·10373	·10930 } too high?
·10362	
Arith. mean .. .. ·10348	·10931

The value arrived at for cobalt is much lower than that (·1067) derived from Regnault's experiments, while that for nickel is practically identical with Regnault's, which is ·1092. This is certainly too high.

Further experiments will be made. Already, however, I feel certain that Kopp's conclusion is right, and that the law of Dulong and Petit, even for the metals, is an approximation only, and

* Proc. R. S. 47, 241.

cannot be properly expressed in the words of the discoverers. For, although the exact values of the atomic weights of these two elements, cobalt and nickel, are not known, it is certain that they are not so far apart as would be implied by these values for the specific heats.

Two other examples of somewhat similar kind are shown by gold and platinum, copper and iron.

For the gold I naturally applied to my colleague, Professor Roberts-Austen. The platinum I prepared from ordinary foil, by re-solution and re-precipitation as ammonio-chloride, and subsequent heating. Both metals were fused into buttons before use. The atomic heats come closer together than those of Co and Ni.

Copper and iron differ considerably in melting point, but both at the temperature of 100° are far removed from even incipient fusion. The copper was prepared from pure sulphate by electrolysis, the iron by reduction of pure oxide in pure hydrogen. Notwithstanding all our care, it was disappointing to find it contained ·01 per cent. of carbon, the source of which I am at a loss to explain. This iron is purer than any examined by Regnault or Kopp.

SPECIFIC HEATS OF GOLD AND PLATINUM.

Pure fused.

Gold, S.G. $\frac{18^\circ}{18^\circ}$, 19·227.		Platinum, S.G. $\frac{18^\circ}{18^\circ}$, 21·323.	
	·03052		·03147
	·03017		·03150
	·03035		·03144
Arith. mean	·03035	Arith. mean	·03147
Atomic heat	5·94	Atomic heat	6·05

SPECIFIC HEATS OF COPPER AND IRON.

Fused.

Copper (pure) S.G. $\frac{20^\circ}{20^\circ}$ 8·522.		Iron, S.G. $\frac{15^\circ}{15^\circ}$, 7·745, contains 0·01 per cent. copper.	
	·09248		·11022
	·09241		·11037
	·09205		
	·09234		
Arith. mean	·09232	Arith. mean	·11030
Atomic heat	5·83	Atomic heat	6·13

The differences observed between cobalt and nickel, and between gold and platinum, are manifestly not due to allotropes or to differ-

ences of melting point, which in these cases can have no effect on the result. So large a difference must be due to peculiarities inherent in the atoms themselves; and differences of atomic heat are to a certain extent comparable with the differences observed in other physical properties, which, like specific volume, specific refraction, &c., are approximately additive.

If we try to think what is going on in the interior of a mass of solid when it is heated, the work done is expended not only in setting the atoms into that kind of vibration which corresponds to rise of temperature, that is, it makes them hotter, but partly in separating the molecules or physical units from one another (= expansion) and partly in doing *internal* work of some kind, the nature of which is not known. A difference between metals and non-metals has been brought out by the researches of Heycock and Neville, who find that metals dissolved in metals are generally monatomic; whereas it is generally admitted that iodine, sulphur and phosphorus in solution are polyatomic. It is moreover remarkable that, although in respect to specific heat each element in a solid seems to be independent of the other elements with which it is associated, when the elementary substances are vaporised some rise in separate atoms like mercury, some in groups of atoms like iodine, sulphur, arsenic and phosphorus, and as the temperature is raised these groups are simplified with very varying degrees of readiness.

The two metals, cobalt and nickel, with which I began my inquiry, have very nearly the same atomic weight, the value, 58·24 for nickel and that for cobalt 58·49, being calculated by F. W. Clarke from the results of a great many analyses by many different chemists. They are so close together that for a long time they were regarded as identical, and Mendeléef does not hesitate even to invert the order by making Co = 58·5 and Ni = 59. These metals, nevertheless, differ from each other in several very important chemical characters. Nickel, for example, forms the well known and highly remarkable compound with carbonic oxide discovered by Dr. Mond. Cobalt, on the other hand, produces many ammino-compounds to which there is nothing corresponding among the compounds of nickel.

Having put aside the common excuses for the observed divergences from the constant of Dulong and Petit, we are compelled to look round for some other hypothesis to explain them.

The constitution of carbon compounds is now accounted for by a hypothesis concerning the configuration of the carbon atom introduced by Van't Hoff and Le Bel twenty-five years ago, and which is now accepted by the whole chemical world. It seems not unreasonable to apply a similar idea to the explanation of those cases of isomerism which have been observed in certain compounds of the metals, notably chromium, cobalt and platinum. This has already been done by Professor Werner, of Zürich. If the constitution of compounds can be safely explained by such hypothesis, this implies the assumption of peculiarities in the configuration of the individual constituent metals

around which the various radicles are grouped in such compounds ; a
hence peculiarities in the behaviour of such metals in the elemen
form may possibly be accounted for. For the atom of cobalt Profes
Werner employs the figure of the regular octahedron. For nick
therefore, which differs from cobalt in many ways, a different figu
must be chosen. This, however, is for the present a matter of pu
speculation.

W. A. T.

WEEKLY EVENING MEETING,

Friday, May 20, 1898.

The Hon. Sir James Stirling, M.A. LL.D. Vice-President, in the Chair.

The Right Hon. D. H. Madden, M.A. LL.D.

The Early Life and Work of Shakespeare.

In the year 1592 there was in London a moderate actor and struggling dramatist named William Shakespeare. He had as yet published nothing, and he was known chiefly as an adapter of the work of popular authors to the uses of the company of players with whom he was associated. As a dramatist, few would have thought of comparing him with Marlowe, Greene, Peele, Lodge, or Nash; and as a poet he was known only to some private friends, to whom he had shown certain sonnets and, it may be, the first heir of his invention, a poem entitled 'Venus and Adonis.'

Had he then met the fate which shortly afterwards overtook his great master, Marlowe, a tavern brawl might have deprived the world not only of 'Hamlet,' 'Othello' and 'As you like it,' but of all knowledge of the man who was destined to be their author. It is true that his genius had attained to the production of 'A Midsummer Night's Dream' and 'Romeo and Juliet'; but neither of these plays was printed until some years after, when his later productions had added to the reputation of their author. Had his fellows adventured on the publication of a posthumous volume, containing, in addition to these plays, 'Titus Andronicus,' 'Henry VI,' 'Love's Labour's Lost' and 'The Comedy of Errors,' it is possible that the truer instincts of the nineteenth century might have rescued the collection from the indifference of the eighteenth century, and the contempt of the seventeenth, when Pepys was not deterred by the fame of their author from describing 'A Midsummer Night's Dream' as the most insipid, ridiculous play, and 'Romeo and Juliet' as the worst, he had ever seen. If Thomas Thorpe had thought it worth while to publish the Sonnets at the instance of Mr. W. H. (which I greatly doubt), it is possible that the discernment of an unheeded critic might discover some of the finest poetry in the English language in the forgotten volume—for forgotten it certainly would have been at a time when Steevens deemed the sonnets unworthy of publication, as productions which no one would read.

I have suggested these possibilities with no intention of engaging in the most fruitless of all inquiries—speculation as to what might have been—but for a practical purpose. If we would clearly discern the man Shakespeare in relation to the known facts of his life, it is needful to close our eyes to the dazzling splendour of his later works. I invite you to do this for a moment, and, forgetful of theories, fancies and transcendental criticism, to fix your attention upon a few simple facts, proved by clear evidence, in the hope that we may be thus aided in the realisation of a personality, at once the most attractive and the most elusive.

For a reason, which will appear presently, I take the close of the year 1592 as the termination of what I have called the early life and work of Shakespeare. Of the man as he then existed, of the life which for some twenty-eight years he had lived on this earth, of the knowledge which he had acquired, of the pursuits in which he had engaged, and of the literary work which he had accomplished, we have means of knowledge fuller and more certain than we possess with regard to many great men whose lives are separated from ours by a much shorter interval of time; and the man, as we know him, and his work as we possess it, are in complete accord.

And yet Hallam wrote, with absolute truth, that of William Shakespeare "it may be truly said that we scarcely know anything." For he thus explained his meaning: "If there was a Shakespeare of earth, as I suspect, there was also one of heaven; and it is of him that we desire to know something." Of the Shakespeare of heaven; of the creator of Hamlet, Othello and Lear, our knowledge has been fairly summed up in the words: "He lived, and he died; and he was a little lower than the angels." And yet one other fact is certain. The Shakespeare of whom we would know something was one and the same person with his earlier self, and any knowledge which we may gain of the one adds to our understanding and appreciation of the other.

I have chosen the end of the year 1592 as a point in Shakespeare's life, because it is then that we obtain our earliest view of the man, in the light of a contemporary notice. Every student of the life of Shakespeare is familiar with the words in which he was denounced by Greene, who, when repenting on his deathbed of many grievous sins, somehow forgot to include "envy, hatred, malice and all uncharitableness." The authenticity of this passage, and its application to Shakespeare have not been questioned, but its full significance has I think, been overlooked.

In his 'Groatsworth of Wit' Greene conveyed a solemn warning to certain persons, three in number, whom he addressed as "Gentlemen, his quondam acquaintances, that spend their wit in making Plaies." Of these the first and third have been identified with reasonable certainty as Marlowe and Nash. The second is probably either Lodge or Peele. They are entreated to employ their rare wits in more profitable courses than writing plays for play-actors. They are

warned that they were in like case with Greene, they also would be forsaken by these "Puppets that speak from our mouths, those Antics garnished in our colours." "Yes, trust them not," he adds, "for there is an upstart Crow beautified with our feathers, that with his Tyger's heart wrapped in a Player's hide supposes he is as well able to bombast out a blank verse as the best of you, and being an absolute Johannes factotum, is in his owne conceit the only Shake-scene in a countrie." The line thus parodied, "O Tiger's heart wrapt in a woman's hide," occurs in the Third Part of Henry VI., and this circumstance, taken with the obvious play on his name, identifies Shakespeare as the object of Greene's invective.

Had this curious pamphlet been given to the world on the authority of Greene, it might be disregarded as the raving of a disordered brain. But it was revised and published in December 1592, about two months after Greene's death, by Henry Chettle, himself a dramatist of note, to whose pen it appears to have been attributed. For in the preface to his 'Kind Hart's Dream,' Chettle is at pains to disown the authorship and to make such amends as he could to two of the playwrights addressed by Greene. "A letter," he says, "written to divers play-makers is offensively by one or two of them taken." There was one of those, he tells us, "whose learning I reverence, and at the perusing of Greene's book stroke out what there in conscience I thought he in some displeasure writ." No such reverence for either the learning or the art of Shakespeare led Chettle to tone down the only really offensive part of the whole passage.

Of another of those who took offence he writes, that he did not so much spare him as since he wished, for which he is as sorry as if Greene's fault had been his own, "because myselfe have seene his demeanour no less civill than he excellent in the qualities he possesses. Besides, divers of worship have reported his uprightness of dealing, which argues his honesty, and his facetious grace in writing, that approves his Art."

There is no reason for applying to Shakespeare these words of Chettle, save only a sense of their appropriateness. For it was by one or two of the play-makers addressed by Greene that offence was taken, and Shakespeare was not of the number. I am not, however, careful to discuss the sufficiency of this reason, for the real significance of Chettle's preface consists in the evidence which it affords of the state of his mind when he edited and revised Greene's pamphlet. When he saw no reason to tone down the only really scurrilous passage in the 'Groatsworth of Wit"—the denunciation of Shakespeare as an impudent plagiarist—it is impossible to avoid the conclusion that either Shakespeare was unknown to him, or that he saw no reason to quarrel with Greene's estimate of character and literary ability.

Strange as Greene's words now sound in our ears, there is no reason why they should have startled Chettle. Without accepting the literal truth of any of the traditions, we cannot doubt that Rowe,

Shakespeare's earliest biographer, states with substantial truth that he was "received into the company then in being at first in a very mean rank." The playwrights of established position—Greene, Lodge, Peele, Nash, Marlowe—had all received a University education. They would, not unnaturally, look down on one who was not of their order, and whose earliest dramatic work took the form, not of original composition, but of adaptation. The popularity with playgoers of Shakespeare's adaptations was not likely to win the favour of the dramatists whose works were laid under contribution. We know, on the authority of Nash, that the Talbot scenes in 'Henry VI.' were applauded by thousands of spectators, and we learn from Ben Jonson that even twenty-five years later there were old fashioned playgoers who would swear that 'Titus Andronicus' and 'Jeronimo' were the best plays.

Thus we can easily understand, from a knowledge of Shakespeare's early life, how it was that his first work as a dramatist—great as we now recognise it to be in part—did not meet with immediate or cordial reception on the part of the literary world. In the end he overcame all opposition and asserted his supremacy, but when the volume of his early work was completed, the time had not yet come.

It was well said by Coleridge, in one of his lectures on Shakespeare, that a young man's first work almost always bespeaks his recent pursuits. Not so much, I would venture to add, in the selection of a subject, as in incidental passages and casual allusions, from which we may discern most certainly the class of images with which his mind is stored and which present themselves unbidden to his imagination.

If the authorship of Shakespeare's earliest play, 'Love's Labour's Lost,' were a matter of speculation, we should conclude with absolute certainty that it was the work of one who was thoroughly acquainted with the studies and pursuits of school.

I am not about to discuss the vexed question of Shakespeare's classical learning. Had I time to do so, I could not hope to add anything to Professor Bayne's essay entitled "What Shakespeare Learned at School," published in his 'Shakespeare Studies.' He there details, from authentic sources, the general course of grammar-school instruction in Shakespeare's time, and examines the evidence supplied by his writings of his having passed through such a course of study. Ovid and Mantuanus were favourite text books. So popular was Mantuanus in the sixteenth century that pedants like to him to whom we are introduced in 'Love's Labour's Lost,' under the name of Holophernes, preferred his 'Fauste, precor, gelida,' to 'Arma virumque'; in other words, the 'Eclogues' of Mantuanus to the 'Æneid' of Virgil. Shakespeare's love of Ovid appears most clearly in his early writings. The story of 'Venus and Adonis' is borrowed from the 'Metamorphoses,' and 'Lucrece' from the 'Fasti.' On the title-page of the former are two lines from Ovid's 'Elegies,' taken from a poem of which no English version had then been published. 'Titus Andronicus' is full of allusions to Ovid. In 'Love's Labour's Lost,' Holophernes puns on his name—Ovidius

Naso —surest token with Shakespeare of affectionate familiarity; "Why indeed 'Naso' but for smelling out the odoriferous flowers of fancy, the jerks of invention?" The extent to which Shakespeare had steeped himself in Ovid was noticed by his contemporaries. Meres wrote in 1598: "As the soule of Euphorbus was thought to live in Pythagoras so the witty soule of Ovid lives in mellifluous and honey-tongued Shakespeare."

The classical learning displayed by Shakespeare was precisely what a clever boy might be expected to carry away from the free grammar-school at Stratford. Thus Coleridge's conclusion appears to be a just one; "Though Shakespeare's acquirements in the dead languages might not be such as we suppose in a learned education, his habits had nevertheless been scholastic, and those of a student."

This conclusion agrees exactly with the testimony of a competent and trustworthy witness, so precisely in point that one is disposed to ask, why it was ever thought needful to resort to speculation and to expert evidence. If, indeed, the question of Shakespeare's classical learning had to be decided in accordance with the opinions of learned experts, we might well despair of arriving at a conclusion. According to critics like Whalley and Upton, he was a kind of poetic Porson, with head so crammed with Greek that he cannot say of valour that it "most dignifies the haver," without the Greek word ἔχειν being present to his mind. Between this extreme, and Farmer's conclusion that "his studies were most demonstratingly confined to nature and his own language," you may find every possible form of intermediate belief. I do not know a better illumination of the value of mere opinion and expert evidence, in matters of criticism.

There is no such ambiguity about the testimony of Ben Jonson. When he wrote of Shakespeare that he had "small Latin and less Greek," we feel sure that Shakespeare was criticised as a classical scholar by one who regarded himself as being, in this particular, his superior. If I were to hear it said of one unknown to me that he knew little law and less equity, I should conclude that the subject of the conversation was certainly not a layman, but probably a judge, or at all events some one who had made a special study of law. And if I knew the speaker to be a censorious man, with a good opinion of his own attainments, I should consider it likely that the man of whom he spoke was a fair lawyer, though probably more eminent in other respects.

Now the great, and, on the whole, generous, nature of Jonson, was infected with a double dose of "the scholar's melancholy, which is emulation." His love for Shakespeare, he tells us, and I have no doubt truly, approached to idolatry. And yet in the very passage in which he records his affectionate admiration, he does not hesitate to note what he regarded as defects, and he sums up, in words which sound strangely in our ears: "He redeemed his vices with his virtues. There was ever more in him to be praised than to be pardoned."

Jonson is not likely to have exaggerated Shakespeare's proficiency in the classical studies upon which he justly prided himself. "The rudiments of Greek," Mr. Sidney Lee tells us, "were occasionally taught in Elizabethan grammar-schools to very promising pupils." If Shakespeare had some Greek, we may fairly conclude that he was a promising pupil, and credit him with the full amount of learning which a clever boy would carry away from the grammar-school at Stratford—scholarship perhaps neither critical nor profound, and not disdaining the aid of translations when procurable, but for literary purposes a sufficient introduction to the masterpieces of the older civilisations.

If the early works of Shakespeare had been published anonymously, and we had to seek for some clue as to their probable authorship, a careful inquirer could not fail to note the frequent use of legal phraseology, especially in the Poems and earlier plays. I have recently seen it stated that there are no fewer than fifty-one legal terms and allusions in the Poems, of which twenty-nine occur in the Sonnets. I have not verified this statement, but I see no reason to doubt its accuracy. Remarkable as is the frequency of these allusions, the manner of their introduction is still more noteworthy. They are for the most part of a casual character, introduced without special reference to the matter in hand, or to the context, with which they are often out of harmony. A poet or a dramatist may employ a term of art with strict accuracy, without leading to the conclusion that he was himself possessed of technical knowledge. He may have consulted a book, or (better still) a friend skilled in the art, whenever it became needful to make use of technical language. But when terms of art are used, not of set purpose, but because they present themselves unbidden to the writer's mind, it is impossible to avoid the conclusion that they have become, somehow or other, part of his mental equipment. No one but a lawyer would go to a law book in search of a simile or a pun.

It is, I think, impossible for a layman to realise the extent to which legal terms and allusions are embedded in the ordinary language of Shakespeare. It would be easy to accumulate instances. Some are obvious enough, such as Rosaline's pun on the announcement of three proper young men of excellent growth and presence: "Be it known unto all men by these presents;" and the suggestion of Antipholus of Syracuse that a man may recover his hair by fine and recovery, capped by Dromio's "Yes, to pay a fine for a periwig and recover the lost hair of another man." Others are more recondite, as when Lepidus, with a lawyer's appreciation of the difference between taking by descent and by purchase, says of Mark Antony that his faults are "hereditary rather than purchased; what he cannot change, than what he chooses."

There is no known fact in Shakespeare's life associating him with the practice of the law. It is, however, reasonably certain that he found some employment for his time and his brains between his leaving school and his coming to London. "I would there were no

age between sixteen and three-and-twenty," says the Shepherd in 'The Winter's Tale,' "or that youth would sleep out the rest." Shakespeare may have relieved the tedium of those years by some of the exploits suggested by the Shepherd, but of his serious occupations we know nothing. There is therefore nothing to exclude any conclusion which may fairly be suggested by his writings. The clever and needy boy of sixteen may have found employment for a time in the office of one of the six attorneys practising in the Court of Record which we know to have then existed at Stratford. He may also have earned his bread for a time, as tradition asserts, by teaching in the school of Holophernes. Finally, tiring alike of school and law, he drifted into play-acting and play-writing. Certainly the age between sixteen and three-and-twenty does not seem to have suggested to his mind in after life the idea of sustained effort or fixed purpose, but only a certainty that the "boiled brains of nineteen and two-and-twenty" would hunt in any weather.

Such familiarity with legal phraseology as we find in Shakespeare's works bespeaks some acquaintance with law, but not more than could be readily acquired by a clever youth (and I suppose that Lord Frederick Verisopht's estimate of Shakespeare still holds good) who had served some sort of apprenticeship to the law, and had gained access to a few law books. A man may talk of warrants, charters, leets and law days, and not be a Lord Chancellor. He may play on the words "recovery," and "assurance," and yet not be a learned conveyancer. Jarndyce v. Jarndyce need not have been attributed to a Lord Chancellor, nor Bardell v. Pickwick to a Chief Justice, even if we did not know that the writer had picked up his legal knowledge in a proctor's office. Where a writer has a little law and sound brains he may be fairly expected to use his legal terms aright. This is what Shakespeare for the most part does. Mr. Castle indeed adduces several instances of the use of technical terms, otherwise than they would be used by a lawyer, from which he concludes that the plays were written by a layman, who sometimes relied on his own resources, and at other times had recourse to the aid of a trained lawyer. But why should this layman for ever hanker after legal phrases and allusions, in season and out of season? And why, if he realised the need of advice, did he adventure on their use in the absence of his adviser? It is surely more reasonable to have regard to Shakespeare's legal phraseology as a whole, and to draw our conclusion accordingly. There is a curious passage in Nash's 'Epistle to the Gentlemen Students of two Universities,' in which he writes of some that leave "the trade of noverint" and busy themselves with the endeavours of art, "affording whole Hamlets, I should say handfuls of tragical speeches." This passage, which was printed in 1589, may not refer to Shakespeare, but that it proves that a limb of the law turned playwright—for this is the significance of Nash's reference to the trade of noverint—is not an improbable supposition.

There is yet another characteristic of the early plays and poems, which would be of still greater value if we were driven to discover their authorship from internal evidence; for it would exclude many competitors and considerably narrow the area of search. I have elsewhere collected the allusions to field sports and to horsemanship which are scattered throughout the works of Shakespeare. They are to be found in his later, as well as in his early works, but nowhere in such freshness and abundance as in the first heir of his invention —'Venus and Adonis.' Of the description of the hare-hunt in this poem Mr. Bagehot remarks, that it is idle to say that we know nothing of its author, for we know that he has been after a hare. This is a concise statement of the inference to be drawn from the Shakespearian allusions to sport and to horses. In mere point of number they are without parallel in literature. There are to be found in Shakespeare about four hundred words and phrases distinctly relating to field sports, horses and horsemanship. Many of these terms of art can only be detected by those who have made a special study of the sporting literature of the age. For example, although the words "career" and "race" are still in use, they have long since lost the technical meaning which they once possessed in the language of the *manège*. Reading the passages in which these words occur, in the light of the technical knowledge which Shakespeare possessed, they acquire a fresh significance and convey a fuller meaning. Time will not permit me to enter into this subject at any length, but I may mention some of the characteristics of the Shakespearian allusions to sport or horsemanship. Sometimes they convey a secret of woodcraft or horse knowledge, as when we are warned against a horse with a cloud in his face, or taught how to avoid scaring a herd of deer by the noise of a cross-bow. Often they are used in illustration of human nature and character, as when we are told that "hollow men, like horses hot at hand, make gallant show and promise of their mettle," but when the time of trial comes on and they should "endure the bloody spur," they, "like deceitful jades, sink in the trial." Sometimes they convey a lively image, often an irrelevance, by which I mean an idea somewhat out of place with its surroundings; and puns on words connected with the chase, especially on the words "hart" and "deer," are almost beyond counting.

There is a distinctive note about Shakespeare's allusions to sport, which I have failed to find in either the detailed descriptions or casual allusions of any other writer. Applying Mr. Bagehot's canon, we surely know something of the man whose thoughts for ever run on horse, hound, hawk and deer. We know that many years of his early life must have been spent in the pursuit of sport, and if we were to draw any conclusion from local allusions, we should infer that those years had been spent not far from Gloucestershire or from Cotswold. And here we find the Shakespeare of fact and of tradition in perfect accord with the testimony of his early works.

I have directed your attention to some aspects of the Shakespeare

of 1592, in regard to which he appears to be intelligible and devoid of all mystery, save only as to the immensity of his genius. They appertain to the Shakespeare of this earth—schoolboy; possible attorney's clerk; certain huntsman, courser, falconer and horseman; needy adventurer; and theatrical factotum. But what of the Shakespeare of heaven?

The unity of Shakespeare has not yet been questioned. No one has doubted the personal identity of Greene's Johannes factotum with the supreme artist, many years afterwards addressed by one of the greatest of his contemporaries as "the wonder of the stage." This, wrote Hallam, is "an improvement in critical acuteness doubtless reserved for a distant posterity."

Had Hallam written some twenty years later, his forecast might have been different. A generation in which the existence of Shakespeare has been denied, might fairly be expected to question his unity. By "Shakespeare," I mean the author of the plays and poems; and his existence as a separate entity is surely denied by those who regard him as merely a phase or casual development of another man, and the authorship of the greatest of all literary productions as an unconsidered incident in a life-work of an entirely different kind.

When an irrational idea is entertained by men who are in other respects rational, we can generally find, if we search carefully, some reason for its existence; not, perhaps, an exquisite reason, but a reason good enough, in the absence of a better. Rational men who believed in the Tichborne claimant would tell you that the mother of Tichborne believed in him, and that she ought to know her own son: a reason good in itself, but overborne by the weight of adverse testimony. When Mr. John Bright said that "any man who believes that William Shakespeare of Stratford wrote 'Hamlet' or 'Lear' is a fool," he gave a reason for the faith, or want of faith which was in him, and voluminous writers have done little more than expand and illustrate this concise statement. But he overlooked the fact that 'Hamlet' and 'Lear' were not written by William Shakespeare of Stratford. They were the work of one who was linked to the man of Stratford no doubt by the tie of personal identity, but separated from him in a much more real sense by some twenty years of thought, work, study, observation of men and manners, and (for aught we know) of sin, suffering and remorse, in this city. Why, between the man of Stratford and the Shakespeare of 1592 there lay six years of work in London: a time more than sufficient to convert an unfledged schoolboy into a learned professor.

What are the characteristics of the author of 'Hamlet' and 'Lear' which have been noted as irreconcilable with what we know of the man of Stratford? They are these: the encyclopædic range of his knowledge, so vast that specialists in several branches of learning have claimed him as their own; his intimate acquaintance with human nature, as it manifests itself in all times and under all circumstances, at home and abroad, in courts and palaces, as well as

in humbler abodes; his familiarity with ancient literature; his knowledge of foreign languages, shown by his use of French and Italian books of which no translations are known to have existed; and the fact that, in Coleridge's words, "he was not only a great poet, but a great philosopher."

The Shakespeare of 1592, as we discern him, was on his way to the attainment of these great qualities, but he had not as yet attained. He had lived for six years in London under the intellectual influence of Marlowe, and probably on terms of intimacy with him. Marlowe was killed in 1593, and a few years afterwards Shakespeare, quoting a line from 'Hero and Leander,' addressed the author as "Dead Shepherd," in terms suggestive of personal attachment. In 1593 he published 'Venus and Adonis,' dedicating this "first heir of his invention" to the Earl of Southampton. That this dedication was as prudential and successful as his other speculations we may infer from the very different language which he used a few years later in his dedication of 'Lucrece.' He had then become on terms of intimacy with Southampton, which he described as 'love,' a word at that time descriptive of warm friendship. If tradition speaks truly this sentiment was returned in the substantial form of a gift of one thousand pounds. The Earl of Pembroke and the Earl of Montgomery are stated by the editors of the folio of 1623 to have prosecuted the plays and "their author living" with much favour, a statement of which an interesting illustration may be found in a note to Mr. Wyndham's recent edition of the Poems. The flights of the Swan of Avon, according to Ben Jonson, "did so take Eliza and our James," that we may fairly conclude that he was not neglected by their courtiers. Fuller, who was born in 1608, probably derived his knowledge of the wit combats at the Mermaid Tavern at first hand, from those who had witnessed or taken part in them. It was by the publication of the Poems that Shakespeare was first introduced to the polite society of the capital. Meanwhile his fame as a dramatist grew apace, for in 1598 Meres ranked him first in both tragedy and comedy.

Of his life in London, of the men and women with whom he conversed, of the books which he studied, of the scenes which he witnessed, we may conjecture much, but we know little or nothing. If there was something (as many have conjectured with Hallam) which changed the sweet and sunny nature of Shakespeare to gloom, that something must always remain buried in mystery. It can derive no clear or certain illustration from sonnets written (so far as can be learned from external evidence) before the advent of this gloom became traceable in his other writings. His love of rural sports, and a desire, like that of Scott, to attain a position of consequence in the country, may explain his abandonment of London life; but it can never solve the riddle of his total neglect of the greatest of all literary productions. One fact, however, is certain. The Shakespeare of 1592 was, in the course of a quarter of a century of London life,

subjected to precisely the kind of influences by which one endowed with illimitable genius and boundless powers of acquiring knowledge (and these must be assumed on any hypothesis) might in time be wrought into the author of 'Hamlet' and of 'Lear.' Reading his plays in chronological order, we can trace the development of his mighty intellect, until at last we are brought face to face with "a thing most strange and certain": the personal identity of the final outcome of all those years with the man whom we have been considering, and whom we can easily recognise as William Shakespeare, late of Stratford. I live in daily expectation of this identity being questioned. It is satisfactory to feel that when Hallam's anticipation is fulfilled, the interest of the subject which we have been considering will not be lessened. But you may then have to listen to many lectures, each dealing with the life and work of one only of the several individuals into whom criticism shall have resolved the components parts of that mighty whole, which, in the meantime, and provisionally, we still call WILLIAM SHAKESPEARE.

[D. H. M.]

WEEKLY EVENING MEETING,

Friday, May 27, 1898.

Sir William Crookes, F.R.S. Vice-President, in the Chair.

Lieut.-General The Hon. Sir Andrew Clarke, R.E. G.C.M.G.

Sir Stamford Raffles and the Malay States.

The subject which I wish to bring before the Members of the Royal Institution to-night is one that passing events now invest with a special and direct interest. Sir Stamford Raffles and his work at Singapore and in the Straits Settlements must always claim the attention of those who have dwelt in that region, and have had transactions connected with it; but it has been invested with general national importance and a peculiarly direct significance by its relationship to the progress of events in the Far East. At the present moment we are able and willing to appreciate the good work Raffles did for his country in founding Singapore. We can now all see how fortunate it was for England that, in 1819, he realised the importance of making secure the road to the Far East, and that his measures with that object in view were, after many difficulties, eventually crowned with success. If he had been beaten in his single-handed campaign against the authorities at Penang and in India, against also the Secret Committee, and even the Cabinet at home, our expansion eastwards would have been fettered, our trade would have been deprived of fresh avenues, and nothing short of a costly and hazardous war would have placed us in that position of vantage at the southern promontory of Asia, on the open highway to the marts of Siam, China and Japan, which he secured for us without a blow, and by his own unaided but indomitable energy. We can all of us see these results to-day; but, in paying our tribute to this remarkable man, we should recollect that he achieved these successes under great difficulties, that he was the object of slanderous misrepresentations, that he was opposed with a bitterness unknown in the present phase of society, and that charges of grave and ineffaceable purport were brought against him by his unscrupulous and deadly adversaries. It is only within the last few months that all the clouds obscuring the fame of Sir Stamford Raffles have been dispelled by the unanswerable official contemporary evidence which Mr. Demetrius Boulger has brought to light in his recent biography of the Founder of Singapore.

If the subject of this picturesque and varied career, this spectacle

of a strong man, struggling, under a weight of difficulties not of his own making, and of wrongs that he had never merited, to the goal of triumphant achievement, appeals to you who may have never seen the roadstead of Singapore, with the great ocean steamers passing eastwards and westwards at pistol-shot from our batteries, you will understand how much greater is the hold this theme has established on the mind of one who had the honour to hold practically the same post as that which Raffles filled, and who was privileged to carry out in the Malay States the wise and permanent principles of his liberal and large-minded policy. It is that association of place, principle and policy that has induced me to accede to the request to address you on the subject of Stamford Raffles and his work.

Before I draw your attention to the public side of Sir Stamford Raffles' career I will sketch for you, as briefly as may be, that part of his private life which preceded his attainment of official prominence in the capacity of Lieutenant-Governor of the temporarily subjected island of Java. Born in the year 1781, with every reason to believe that his family was of gentle origin although its fortunes had for some generations been obscure, young Stamford Raffles was compelled by the necessities of his parents to accept temporary employment in the Secretary's office of the India House. Here he did so well that he gained the approbation of his chief, Mr. William Ramsay, long Secretary to the East India Company, who at the earliest opportunity brought him on to the establishment. During these years young Raffles, after the long hours of his office, did everything in his power to supply the defects of an imperfect education, burning the midnight oil, or to be more exact the midnight candle, in pursuit of knowledge, despite his mother's protest against his extravagance. He had his reward, for early in the year 1805, before he reached his twenty-fourth birthday, he was appointed Assistant-Secretary at Penang with a large salary. He owed this sudden rise to the good opinion Mr. Ramsay had formed of him, and to the general belief in the office as to his exceptional ability, of which opinion the Chairman, Sir Hugh Inglis, made himself the spokesman. It has now been clearly shown that Mr. Ramsay had no other motive in securing this appointment for his young friend than the desire to advance a deserving man, and that when he said the departure of his assistant was "like losing a limb" he intended no exaggeration and spoke from his heart. When Raffles got this appointment he naturally bethought himself of getting married and of securing a partner during his exile. Many years must elapse before he could again set foot in England, and it was only natural, as he said, to secure one "bosom friend, one companion to soothe the adverse blasts of misfortune and gladden the sunshine of prosperity." He found this lady in Olivia Fancourt, the widow of an Assistant-Surgeon in the Madras Establishment. Her maiden name was Devenish; she had resided in India when her first husband had died, and during the nine years of her second married life in the East

she was the paragon of all a wife should be. She gained the estee and respect of the Earl of Minto, who wrote of her as "the gre lady," and of Dr. Leyden, who addressed one of the happiest effor of his muse to Olivia. She fascinated her husband's staff, and ev the Malay clerk Abdulla probably revealed the truth when he sai "it was she that taught him."

On arrival at Penang, or really before arrival, while at se Raffles showed that his pertinacity and assiduity were not abated b the rise in his fortunes, by turning his attention to the study of th Malay language. He worked hard at it, employed on his own accou a staff of native teachers and translators, and was soon a qualifie interpreter. But he became much more than a mere interprete He mastered Malay history, laws, and the great principles of navigation by which the commerce of the Archipelago had been controlle He grasped the importance of Malacca, and by a timely remonstran he saved it from the fate which the Government had decreed. H read much of Singapura, "the lion city" and metropolis of the ol Malay empire, and he probably thought of reviving its departed glo before he knew that it would make an unrivalled maritime statio Malay studies strengthened by a common pursuit his friendship wi Leyden, and the importance of this fact was that Leyden was the resident at Calcutta, and that he had gained the confidence of th Governor-General, Lord Minto. If Raffles had been an ordinary ma the appointment to Penang would never have possessed any great significance than a good, well-paid post, and his name would nev have been handed down to posterity as one of our greatest Pro-Consul At Penang there never was the least chance of any special distinctio There is no need to disguise the fact that Raffles was ambitious. H broke through the barriers of local insignificance that would have hel him confined in a vegetating existence until he added his own to th numerous graves of his colleagues on the island, or returned to pa his closing years in England with an impaired constitution and no one of all his dreams achieved. He looked beyond Penang, he sa the opportunity of freeing the Straits and the Spice Islands from th jealous control of the Dutch which arose out of the temporary asser tion of French authority through Napoleon's incorporation of Holland. These events were of public notoriety; they formed the top of conversation both at home and abroad, but Raffles alone, with singular prescience and forethought, at once saw how they could b turned to Imperial advantage.

He left Penang on leave, and he went to Calcutta. He wa received by Lord Minto, on whom he made a most favourable impression, and in a few weeks he won the Governor-General round to hi policy of conquering Java as the sure way to secure for ever th predominance of British commerce in the waters of the Far East. A that moment Raffles was exactly twenty-nine years of age. Yet fo some inscrutable reason this half-forgotten and unappreciated publi servant is even to-day slighted, judging by the inadequate receptio

his biography has met with, and by the reluctance the critics have shown to accept his claims to greatness at the just rate his services to the country and the Empire both demand and justify.

Well, he was only twenty-nine when, coming as a stranger, he won the responsible ruler of India round to his views on a question of external policy which entailed the despatch of the largest expedition up to then sent from the shores of India. He not only framed the policy, but he was entrusted with the task of carrying it out. I will not detain you with the details, but he discharged his task with unerring wisdom and unsurpassed energy. He soon discovered the best route for the expedition to Batavia, one that had never previously been used by Europeans. The officers of the Royal Navy laughed at him, or rather, thought slightingly of his professed knowledge of this sea route, and predicted nothing but misfortune, but he had the laugh of them, for the route followed proved perfectly safe, and the expedition reached the roadstead of Batavia without losing a ship or even a spar. The ultimate success of the undertaking was, to a great extent, dependent on the early arrival of the ships on the coast of Java, and this was due mainly to the courage and confidence shown by Raffles.

If the service Raffles rendered throughout the preparations of the Java expedition was great, so was his reward. He deserved it, no doubt, but public servants do not always get what they deserve. To do that, every one of the higher powers would have to be a Lord Minto—just, generous, with the will to bestow the merited reward and the courage to stand or fall by those they nominate. Well, Raffles was made Lieutenant-Governor, with the fullest powers, of the island of Java immediately after its conquest. I do not intend to dwell on his remarkable administration of that beautiful and still but partially-developed island. It will suffice to say that in five years he pacified the portion left under the native sultans in a manner that no former Government had ever attempted, he raised the industrial and agricultural prosperity of the island to the highest point, and he increased the revenue sevenfold. His government of Java forms one of the brightest pages in the history of Anglo-Indian administration, but the Fates, or to be more precise, the Congress of Vienna, decreed that the island should be restored to the Dutch, and thus, except as a model, the work of Raffles, in probably the richest and most beautiful island of the world, came to an end.

This meant much more for Raffles than the loss of a Lieutenant-Governorship. It signified the destruction of his hopes, of his ambition, not for himself but for the country. Java was, in his hands, to be the stepping-stone, the half-way house to China and to Japan. It was to secure for England the position in those seas of an undisputed supremacy. She was to be the beneficent mistress of the countless islands of the Archipelago, and the security of her position was to be based on the generosity of her commercial policy towards the rest of the world. Raffles was a Free Trader before the phrase

was known in party politics at home. While the East India Company clung to its monopolies, Raffles, its servant, made every port within its jurisdiction a Free port, and, with the one exception of opium, allowed every article to be exported or imported under all friendly flags at a customs rate of five per cent. Lastly, within the shortest radius, he sought for the finest naval station that Nature had provided in those seas. He thought he had found it in Banca, or in Billiton. All these hopes, these ambitions if you will, were dashed to the ground by the Congress of Vienna. Of the great fabric of beneficent rule and Imperial power created in the mind of Raffles, and of which his energy and address had laid the corner stone, nothing remained.

Under the shadow of this great disappointment Raffles came to England in 1816. He returned two years later to the East as Lieutenant-Governor of Fort Marlborough, or Bencoolen, in Sumatra. He was charged with no special mission, nor was he entrusted with the execution of any external policy. He was to confine his attention to the local matters of what was called in those days the West Coast and he was, if possible, to reduce the heavy expenditure of the establishment. At Bencoolen those who dreaded the active imagination and untiring energy of Stamford Raffles felt sure that he would have no opportunity of disturbing their tranquility by raising burning questions, by contending for rights that they were well content to see lost or left in abeyance. All they hoped from him was that he would increase the cultivation of pepper, improve the book-keeping of the offices, and perhaps indulge in the harmless direction of natural history, that activity of mind which they knew him to possess. Such were the motives of those in power when they sent back to the East the man who had inspired the Governor-General's policy in a great issue, and administered the affairs of a thickly populated island with a skill not inferior to that of Warren Hastings.

I have now brought you to the turning point in this great man's career. His banishment to Sumatra, for that is what the appointment would have signified to an ordinary Governor, was intended to put an end to his opportunities of agitating the minds of his superiors in India and London. They did not want to be troubled any more about the questions of the Archipelago or the Dutch proceedings therein, and they believed that the deplorable condition of the moribund settlements on the West Coast would effectually prevent his meddling with anything outside them.

We know how baseless was this expectation. Local affairs, the limited horizon of a Sumatran station, were incapable of chaining the imagination of a man who had known how to emancipate himself from Penang and to become one of the leading personages in the Anglo-Indian world. He had much to do at Bencoolen. He did it. He restored the prosperity of that station, he established an equilibrium in the finances, and he arrested the decline in the

fortunes of the West Coast. But while he did this his energy, his vigilance and his audacity remained undiminished for his great and final struggle with England's great rival in the East. He saw that there was no one else who would essay the task, and, with his buoyant spirit, he assumed the direction of the necessary national policy in this quarter of the Far East. Well for England was it that he did so, as the opportunity he saw, if it had been then lost, might never have recurred.

The restoration of Java to the Dutch was inevitable; great as was the loss and the pity, we could not retain it except by setting a bad example to the other European Powers who wished to benefit by the prostration of France after Waterloo. But with the restoration of Java we had done all that the most exacting sense of justice could require of us. There was no reason for us then to sit down supinely while the Dutch extended the area of their authority and made their position the base of aggressive operations at our expense. They recovered, by the Castlereagh Convention, Malacca and Java. They found the island of Java in a flourishing condition. On the records of the Government stood the facts as to the schemes and views of Raffles. They took over his surplus, and, to the best of their capacity, they also took over his projects. They seized Billiton, they laid hands on Banca, they asserted their jurisdiction at Palimbany, and they planted their flag at Rhio. In this manner they secured much more than they ever possessed before. Their hold on the Straits of Malacca was tightening, and if the British authorities had remained inactive for but a few months longer, there seems no reason to doubt that they would have brought under their flag the whole of the territories of Johore, within which stood the peerless harbour and roadstead of Singapore.

At that supremely critical moment Raffles reached Bencoolen. He took in the whole situation at a glance. The Dutch, he said, had scarcely left us a foot to stand on, but there was still time to secure that foot. He reached Bencoolen in March 1818; he at once addressed the Governor-General, the Marquis of Hastings, who had, in the matter of the Gillespie charges, shown himself none too well disposed towards Raffles, and in July he was invited to come to Calcutta to discuss the situation. Raffles did not waste a day. Immediately on receipt of this invitation he hastened to Calcutta in a miserable country boat, and laid his plans and proposals before Lord Hastings. He succeeded first of all in making his peace, as he termed it, with the Governor-General, who went so far as to say, "Sir Stamford, you can depend on me." But his second success was the greater, for he obtained the Governor-General's authority to counteract Dutch encroachments by establishing British influence and authority in Acheen and at Rhio. In notifying this news to a friend he added, "At Rhio, I fear, we may be too late." Within little more than six months of his return to the East, Raffles had thus obtained permission to do what no one else would do, viz. to keep the Straits open for

British trade and to place a check on the excluding policy of the Dutch. He thus resumed, in a different form, the task he had crowned with success in Java, of obtaining on the road to the Far East a free port and a naval station adequate for the expansion and security of British trade. In the first act he had been beaten by the force of circumstances, and by the fact that the political requirements of Europe never allowed the local arguments in favour of retention to be impartially considered; but now, in the second act of his duel with the Dutch, there was a reasonable chance of success, because the Governor-General, at least, had become alive to the necessity of doing something. Thus, for the second time in his career, Raffles brought a Governor-General of India round to his views, and made the policy of the country conform to his views of the situation.

On 28th November and 5th December, 1818, Raffles received his instructions to proceed to the Straits of Malacca. In the former it was laid down that "the proceedings of the Dutch authorities in the Eastern Seas leave no room to doubt that it is their policy to extend their supremacy over the whole Archipelago." To counteract the injury to British trade from this policy it was proposed to arrange "the establishment of a station beyond Malacca such as might command both the Straits of Malacca and of Singapore."

The port of Rhio was suggested as the most likely place, and as one where the Dutch had no rights. In the second despatch, provision was made for the Dutch having forestalled the British in the occupation of Rhio. In that event an arrangement was sanctioned with the Sultan of Johore. The significance of this reference lay in the fact that the port of Johore was the old Zion City of the Malays, Singapura or Singapore, and how thoroughly Raffles's mind was fixed on this point may be inferred from his saying in a letter written, a few days after he received his instruction, on board ship at the mouth of the Hooghley, "do not be surprised if my next letter to you is dated from the site of the ancient city of Singapura."

We have now reached the point at which Raffles has not only obtained the highest sanction for his measures to counteract the spread of Dutch influence to the exclusion of British, and the very moment when he had practically fixed in his mind the place, Singapore, by the acquisition of which he intended to defeat their policy. I do not intend to enter into the question of the rival pretensions of Colonel Farquhar. Mr. Boulger's researches and the official documents have settled that dispute. But having just quoted Raffles' letter from the Sandheads, let me follow it up by saying that at once on his arrival at Penang, on 1st January, 1819, Raffles wrote to the Governor-General, "the island of Singapore appears to me to possess peculiar and great advantages" for the desired station. In his own mind, as recorded on the official records, Raffles had fixed on the position of Singapore long before he saw it. His Malay studies had made him acquainted with its past history, and he entertained a reasonable hope that it would be possible to revive its ancient

importance under the British flag. On 29th February,* 1819, he hoisted the Union Jack at Singapore, and in the nearly eighty years that have since elapsed, the evidence as to the value and importance of what Sir Stamford Raffles acquired for us has been steadily increasing, and with every prospect of further development. We can see with our own eyes by its geographical position the magnitude of its trade, the prosperity of its settlers, of what momentous importance Singapore is to the British Empire. Survey the ring of British stations that girdle the globe, and I doubt if there is one more indispensable for our security. But Raffles saw these things in anticipation. Singapore was a barren spot with few inhabitants and one small block-house erected in haste, when he wrote, "it has been my good fortune to establish this station in a position combining every possible advantage, geographical and local," and again, "you will be happy to hear that the station of Singapore contains every advantage—geographical and local—that we can desire, an excellent harbour which I was the first to discover, capital facilities for defence to shipping if necessary, and the port in the direct track of the China trade; we have a flag at St. John's, and every ship passing through the Straits must go within half-a-mile of it." These expressions of opinion, written within a few days of the hoisting of the British flag at Singapore, will show what its founder thought of its future. It will suffice for me to say that all, and more than all, he foretold has been fully realised.

That is how we obtained Singapore. Let me tell you in a few words how nearly we lost it. You have seen how quickly Raffles acted. Within seven weeks of his sailing from the Ganges he had planted the Union Jack at Singapore. Those were the days of slow sailing ships. Three weeks were taken in the voyage to Penang, another three weeks were passed at Penang, and less than a week sufficed for this energetic man to visit and reject the Carimonos and to occupy Singapore by treaty with the Sultan of Johore. It was well that Raffles acted with this promptitude, for on the receipt of a despatch from Lord Hastings to the effect that he intended to employ Raffles on a special mission to the Straits, the Secret Committee sent out a furious despatch forbidding his employment, and declaring that "any difference with the Dutch will be created by Sir Stamford Raffles' intemperance of conduct and language." These official attacks so far influenced Lord Hastings that on 20th February, 1819, he sent orders to Raffles to give up the plan of founding a port and to return to Bencoolen. Fortunately before that despatch was even penned the matter had been settled, and Lord Hastings supported the *fait accompli.* The Dutch protested and indulged in a paper war, which, as Raffles throughout predicted, was all they could do. The arguments and facts were against them; but, if there had been telegraphs or even steamers in those days, Raffles would never

* The anniversary is now kept on the 6th February.

have succeeded in securing Singapore in the teeth of his official superiors.

As stated above, on the 29th February, 1819, Raffles formally occupied on his own responsibility the island of Singapore, and continued to watch over its progress till he finally left it on the 9th June, 1823,* having on his departure received, amongst other tributes of respect and esteem accorded to him, including one from the Supreme Council of India, an address from the people of Singapore, in which it states, "at such a moment we cannot be suspected of panegyric when we advert to the distinguished advantages which the commercial interests of our nation at large have derived from your personal exertions. To your unwearied zeal, your vigilance, and your comprehensive views, we owe at once the foundation and maintenance of a settlement unparalled for the liberality of the principles on which it has been established—principles the operation of which has converted, in a period short beyond all example, a haunt of pirates into the abode of enterprise, security and opulence."

After Raffles' departure, Singapore and the settlements on the Straits were, under successive Governments, limited to the ordinary administration of an Indian out-station. The failure of a military expedition in 1831, and the partial success of one sent in 1832 to retrieve that failure, on the Malacca frontier, induced the Indian Government to withhold, more or less, all intervention in the native states amongst which its settlements were situated. On the transfer of these settlements to the direct authority of the Crown the same policy was continued, and thus remained till 1874.

In order to form a just estimate of the value of what has been done in the Malay Peninsula it would be necessary to describe its condition in January 1874, when it was determined that the internal struggles which were then paralysing trade in all the western states and decimating the population, had become a serious danger to the neighbouring British settlements. Years of guerilla warfare between rival Malay chiefs and their adherents on the one hand, and between various Chinese secret societies and factions on the other, had put a stop to all legitimate work. Towns and villages had been destroyed, mines closed, orchards wasted, and fields left uncultivated for years. There was no safety for life and property, no money, no trade, and little food in the country. Lawlessness and oppression prevailed everywhere, and those who found it hard to live on shore took to the water and made the Straits of Malacca the scene of their operations, so that hardly a day passed but some small trading vessel would be attacked and burnt after the entire crew had been murdered. Probably at no time had the ill fame of the Malacca Straits so truly

* Sir S. Raffles died on the 4th July, 1826, after having been elected the first President of the Zoological Society in the previous April. This Society, which has given pleasure to millions of young and old, was founded mainly by his exertions.

justified its reputation for acts of piracy as in the closing months of the year 1873.

For particulars of the terrible sufferings and terrible oppression of the Malay working classes, men and women, it would be well to consult the reports written by the Residents and forwarded to the Colonial Office. Briefly, it may be said that, while the facts were more than enough to justify the interference of Great Britain, far too long delayed, it happened that at this very time influential Malay chiefs in Perak, Selangor and Sungei Ujong sought the assistance of the Governor of the Straits Settlements to put an end to a state of affairs which had got beyond their control, and in Perak the claimant to the supreme power asked that a British officer might be sent to aid him in the administration of the government of the country.

This was the moment at which it was decided to interfere for this purpose, and what is known by the Treaty of Pemkore was the result. The Governor of the Straits Settlements went to Perak, taking with him the officers considered best qualified to assist in the difficult task of pacifying Malays and Chinese, putting down all violence with a firm hand, healing old sores, making, or attempting to make, reconciliation of quarrels, restoring to their homes women who had been captured and carried into slavery, and dividing the mining lands between opposing factions of Chinese. All this was done, but not all at once—this and a great deal more—and while it is interesting to tell in a few words the result to-day of the experiment made twenty-four years ago, it is still more interesting to note the means by which that result has been brought about.

A few figures and one or two facts will best illustrate this result.

In 1874 a rough approximation of the then population was assumed at 180,000. In 1891, when a fairly reliable census had been taken, the population of the four protected states was 424,218; whilst the last census raises the population to 610,093.

The total land revenue in 1875 was 866 dollars; in 1895 it had reached 511,237.

The total revenue of 1875, the first year in which it was at all regularly collected, was 409,394 dollars; in 1896 it amounted to 8,434,083.

The value of the total imports and exports were in 1876, as far as then could be ascertained, a million and a half dollars; in 1896 it just touched fifty millions.

In 1874, beyond an occasional native path or elephant track through the jungle, no road existed; now a network of well graded and macadamised roads traverses these States. In addition, railway works have been carried on, and are being rapidly extended, and last year's revenue from these was a little over 300,000 dollars.

Irrigation works have made good progress.

In civil administration the establishment of judicial and police tribunals, schools, hospitals, as well as police stations and gaols, all

the needs of civilisation, have been provided ; nor has culture, in th formation of museums and libraries, been wholly neglected.

The sanitary boards have done good work.

The cardinal feature of interest in the story is the means b which all piracy and land fighting, whether by Chinese or Malay was absolutely stamped out ; by which taxation was almost abolish slavery suppressed, justice done, roads and railways construct prisons and hospitals built and maintained, and above all, the chie reconciled to the new life, and the recognition of equality of all rac and classes before the law. It has been done by the residents layi down and insisting on the constant recognition of the principle th the interests of the people they were set to govern should be the fir consideration of Government officers. By learning their languag their prejudices, their character, and by showing them that co sideration which alone can secure sympathy and a good understan ing between Government and people, their respect, and, to so extent, their affection has been won. The natural tendencies of o race are not exactly inclined to these lines, and what has been do and the present feeling as to how the natives should be treated, due to the personal influence of a succession of Residents who gain their knowledge by their own intelligence and experience ; for the were no authorities to consult the administrative experiment in t Malay peninsula standing alone, and having no parallel in Briti administration of alien races.

The Residents were told they were to collect and administer t revenues of the State to which they were accredited. They w also told their advice was to be asked *and acted upon* in all questio except those of Mahomedan law and Malay custom. At the sa time they were warned that they were only "advisers," and that they went beyond that they would be held responsible for any troub which should arise from their action, in what must have been cyn ally described as "a delicate and difficult position ;" but the ve elasticity and wide discretion of this policy was the foundation of i marvellous success. It would certainly not be easy to conceive more impossible position. Entire control of all revenue ; to be co sulted about everything, and the advice tendered *must* be followe That clearly implies the responsibility for the whole Governme of the country. But then the individual who held this positi was to remember that he was only an adviser, not a ruler ; he had means to enforce his directions, and he was warned that he would held personally responsible for any trouble that might arise from th impossible position. The men to whom the work was entrusted once took the entire control and the responsibility with it, and trust to their own determination and tact to keep the peace, lead the chie without driving them, but drive where necessary, and secure the sy pathy and goodwill of the people.

Now that the position of control is recognised, there is force back it, and the anomaly is at an end, but out of the difficulties

that ambiguous instruction has perhaps grown the administration of sympathy, consideration and mutual respect which obtains between the Malay people and the British officers in the services of the native State Governments. I do not for a moment desire to minimise the great work accomplished in Egypt; but I claim for the achievements in the Malay Peninsula the praise which is due to greater success under more difficult circumstances.

Not by wars involving the slaughter of native races, not by drafts upon the imperial exchequer, not by the agency of chartered companies, which necessarily seek first their own interests, has the development of the Malay States been attained. Their present peace and marvellous advance in prosperity have been due to a sympathetic administration, which has dealt tenderly with native prejudices, and sought to lead upwards a free people instead of forcibly driving a subject race.

The example and success of Stamford Raffles should encourage us at the present juncture. He showed us what could be done by courage, confidence and a clear mind. The progress of our commercial and political power in the East brought us into collision with two formidable European rivals, the French and the Dutch. The former were vanquished by Clive on the mainland of India, the latter were finally crushed after an incessant struggle of two centuries by the founding of Singapore. The credit for the latter achievement is as clearly due to Raffles alone as the victory of Plassey was to Clive, and I myself hold the opinion, to which I may add I gave expression before the publication of Mr. Boulger's biography, that of these two great Englishmen Stamford Raffles was the greater.

Raffles died a poor man. No thought of accumulating a vast fortune, or of seeking money as a means to power and patronage, appealed to his mind. His ambitions were satisfied with work done for the future of the empire. This was the true imperialist.

I have said enough to draw your attention to the varied, arduous and ill-appreciated career of Stamford Raffles. I have touched on the magnitude of his work and the difficulties under which it was accomplished. Injured and traduced during his life, he has been neglected by later generations. But his work will endure as long as the British Empire. It was achieved at a moment of depression such as the present. The game seemed lost, the Government was indifferent and short-sighted, the enemy was up and doing, the margin of opportunity was narrowed to the smallest compass, cowardice or hesitation controlled our action, yet one man was able to turn the bitter draught of defeat into the ambrosia of victory. So will it be again if our public servants keep before them the inspiring example of Stamford Raffles.

The life of Stamford Raffles is full of great lessons of vital import to all those to whom the British Empire is alike an object of national pride and of grave responsibility. That Empire was not built up by the genius of statesmen, but by the patient labours, the fore-

sight, and the vigorous initiative of men like Raffles. The directors of the Honourable East India Company in London, anxious only for immediate pecuniary returns, and Lord Hastings, absorbed in the local affairs of India, failed absolutely to perceive the eventual necessity for a British high road to the Far East. That Malacca was occupied and tenaciously held, and that Singapore became a British possession, was mainly, if not wholly, due to Stamford Raffles.*

The enormous importance of the Straits Settlements to-day, as the key to the great ocean highway which stretches up to the Gulf of Pe-chi-li, is abundantly recognised. But for the possession of this key, what would now be our position in the China seas? Yet the man who saw into the dim future and who strove, as some strive for personal distinction or for wealth, to gain and to keep this priceless possession, received scant recognition and few honours from the nation to whose interests he gave his life. Almost may he be said to have died of a broken heart. It is only now, when the splendid fabric of the Empire is beginning for the first time to be understood, that tardy reparation is accorded to the memory of one of its great founders.

All important as was the work which Raffles accomplished, his aspirations were realised only in part. The surrender of the Dutch islands was an act which no other nation in the world would have countenanced. Those foreign critics who affect to regard the growth of the Empire as the result of a policy of unexampled rapacity, have not taken the trouble to read history. The total extent of territory which we have abandoned is enormous, and the Dutch colonies have been twice handed back to Holland. The action was magnanimous, but the progress of the world has certainly not benefited. The restoration of Java, against which Raffles strove in vain, gave back the natives to a rule in which little consideration of their interests or their rights found place. Sumatra, which, in British hands, would long ago have been a thriving colony populated by a contented race, has been the scene of continuous warfare. Raffles suggested an alliance with Siam, which, if then carried out, would have saved this interesting country from partial dismemberment, and from the menace which still hangs darkly over it. His idea of a confederacy of Malay States has been partially, at least, realised in the Malay Peninsula, where it is my greatest pride to have inaugurated the system which has led to prosperity and unexampled development of commerce.

The great guiding principle of Raffles' policy was to understand the native character, and to govern as far as possible by the agency of native institutions. This is a golden rule, occasionally forgotten, but essential to dealing with Eastern races.

The period covered by the official life of Sir Stamford Raffles was

* In this sketch I have purposely omitted to mention other names than that of Raffles, in order to avoid undue lengthening of the narrative.

a turning-point in our relations with the Far East. A new chapter in the history of those relations has now opened. The beginning of the century saw the establishment of that great trade route which has since conferred upon us four-fifths of the commerce of China. With the exception of the acquisition of Hong Kong with Kowlon in 1842, and of the rising colony of North Borneo, Great Britain has not added to her possessions in the China Seas. Port Hamilton, lying a short distance south of Korea, was occupied only to be abandoned. Throughout these years our policy has been to leave China territorially intact, and to open up her resources by the agency of Treaty Ports. That policy is now practically at an end. Since Raffles founded Singapore, Russia has become firmly established in the Far East, and her policy, long evident, of occupying Manchuria, and such ports in the Gulf of Pe-chi-li has now been realised. Germany is established on the China sea-board, with claims and concessions which extend into the Hinterland. Meanwhile France has moved up from the South, and is about, it is said, to occupy a port opposite to Hainan. The partition of China may be said to have commenced. While we might have preferred that the opening out of this vast country should have been gradually carried out through its own ports, other powers, more ambitious, perhaps, and less patient, had other views, and have decided to attempt by a direct process what we were content to leave to indirect methods. Sooner or later this was absolutely inevitable, unless China showed promise of an internal awakening of which there was no real hope. I do not see in the recent proceedings of Russia, Germany and France any cause for alarm or any ground for recrimination. We are not and we never were prepared to occupy Manchuria ourselves. We have no right to complain if Russia here and Germany in Shantung undertake to develop the resources of these territories. To Russia a warm water port in the East is a real need. Geographical conditions all pointed to the Liao-Tung peninsula as furnishing such a port. In occupying Port Arthur and Talienwan, Russia is simply fulfilling her evident destiny and acting in obedience to natural forces. Her action creates no legitimate grievance. We have no right to claim to exclude another power from territory which we do not intend to occupy. I believe that in spite of restrictions the opening up of Manchuria will benefit British trade just as the development of European Russia has added to our commerce. Our only wise course is to recognise facts long foreseen, and since the partition of China has commenced to make certain of our share. I do not gather that any step in this direction has been taken. We are apparently to occupy Wei-hai-wei, which lies 600 miles beyond our sphere, and we have done nothing to secure our position at the mouth of the Yangtse. The ancient fable of the dog and the bone stands true now as always. By reaching after the image of a power which is not to be ours, we risk losing the real substance. I consider, therefore, that we should welcome a Russian occupation

of Manchuria and a German occupation of Shantung; but that w ought at once to clearly define our sphere of future direct influenc in central China, and take immediate steps to make that influence reality when the time comes. We deferred providing India with frontier line until the Russians had advanced across the plains o Central Asia, and difficulties were the natural result. If we defe defining our share of China greater difficulties will assuredly arise.

No one power can monopolise the trade of an opened-out China There is room for all, and we can, if we choose, secure our jus share. If we do not maintain our present proportion of the whol trade of China, it does not thereby follow that we shall not gai enormously, for that whole trade at the present time is but a fractio of what the future will bring. If, as I believe fully, we shall kee our full share of future commercial advantages, it will be due i great measure to the wisdom and the foresight of Stamford Raffles who, in Singapore, secured for us the great gate of one of the mos important trade routes of the world.

[A. C.]

WEEKLY EVENING MEETING,

Friday, June 3, 1898.

Sir Henry Thompson, F.R.C.S. F.R.A.S. Vice-President, in the Chair.

Professor W. M. Flinders Petrie, D.C.L.
Professor of Egyptology in University College, London.

The Development of the Tomb in Egypt.

The general ideas about the Egyptians are so bound up with their preservation of the dead, that some connected account of the development of the tomb may be of interest to others beyond the group of specialists; the more so as my aim is to illustrate the sequence of ideas and of gradual changes in series, rather than to deal with solely archæological matters.

The reasons that the tomb has become so much associated in our minds with the Egyptians are partly real, partly accidental. No doubt the Egyptian thought much of a future state, attached great importance to it, and provided for it in every way that he could devise. Yet we should be taking a very one-sided view if we supposed that the dead were more thought of than the living. It is owing to the accidental conditions that the tombs are so far more noticeable than the houses of ancient Egypt. The tomb was always placed on the desert high above the inundation, and often imperishably cut in the solid rock. The house was usually in the fertile plain of the Nile, and is therefore now buried ten, twenty, or thirty feet in the alluvial deposits left each year by the inundation.

Ancient Egypt has all been covered up far out of sight, except such works as stood on the raised desert edge of the valley; and naturally enough the greater part of these remains are for the dead rather than for the living. Hence our ideas are liable to be very one-sided as to the relative importance of the house and the tomb in the real life of the Egyptians, and we judge of them almost as imperfectly as English life might be judged if the will office in Somerset House were its only evidence.

It is as impossible to understand the arrangement of a tomb without knowing the theory of the soul, on which it was constructed, as it is to understand a temple without knowing the religion, or a house without the social life. The Egyptian had four theories about the soul, probably belonging to successive waves of population that had overflowed the country from different sources. There was the bird

theory, according to which the soul or *ba* fluttered about in and out the tomb as a human-headed bird; the spiritual body or *ka* s coming out of the tomb and wandering about. This soul and gh needed sustenance, and were fed by the tree goddess, who dwelt the thick sycamores which overshadowed the cemeteries. T theory more probably belonged to the earliest negroid inhabitants Egypt.

Secondly, there was the Osiris theory, according to which t deceased went to the elysian kingdom of Osiris, and there ploug and sowed and reaped and threshed the heavenly corn. This m probably belong to the Libyan stratum. Thirdly, there was the S theory, according to which the soul went to join the company the gods in the boat of the sun-god Ra, which sailed daily acr the waters above the firmament, heavenly ocean. This seems due Mesopotamian influence, to which t beginnings of hieroglyphs are also be attributed. Fourthly, there is t mummy theory, according to which t body must be imperishably preserv for ages until reunited to the so This was perhaps due to the Red S invaders of Phœnician kinship.

Fig. 1.—Section of tomb, from a papyrus, showing door above, well-shaft with *ba* flying down, and mummy in chamber with offerings below.

Now all these theories were mix together throughout historical tim and combined as best they might though each is mutually destructive all the others if logically carried o The most usual theories with which have to deal in considering the tom are the first and last combined,—t *ba*-bird of the soul, supposed to fly and out of the grave, the *ka* or spirit body to come out in search of fo and the mummy all the time lying in in the sepulchre. Thus we see it a papyrus, where the *ba*-bird is fly down the pit from the door of t tomb, bearing food and drink to t mummy lying below. In one of t rock-cut tombs of Deshasheh there is a beautiful provision for s visits. The well-shaft was flagged over with slabs in the cham of offerings, but a little channel in the rock gave place for the ba pass from the well into the upper chamber where the statues w placed, which it desired to visit and inhabit. And another li channel opened from the statue-chamber out to the open air on t hill top, so that the *ba* and *ka* could thus go in and out to v both the tomb and the outer world. Any one who has seen the ba

serious owls, with half-human expressions, which flit noiselessly up and down the open tomb shafts, can readily understand what the Egyptian thought when he credited the fleeting soul with like action.

Having thus before us the theory of the soul and of burial, we can now turn to consider the actual tombs.

The oldest burials that we know in Egypt are those belonging to the prehistoric population, which differed greatly from the historical Egyptians. They belong to the age when only the bird theory and Osiris theory were in force, and perhaps the sun-god theory; but certainly when the mummy theory was quite unknown. Instead of preserving the body by mummifying, they often cut it up and buried only the bones, or only a part of the bones. The bodies, moreover, are always

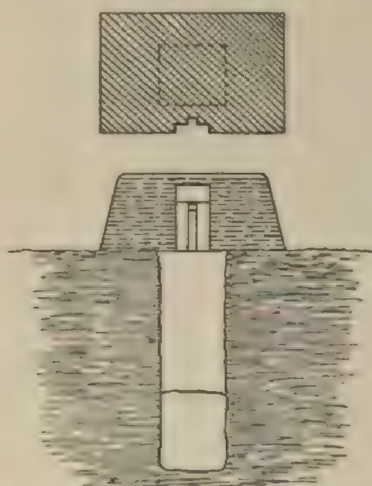

FIG. 2.—Typical early tomb, plan and section.

buried in a contracted position, and not laid out like the mummy. The graves are open square pits, lined with mats, and roofed over with beams and brushwood. Thus they were quite different from the later type of Egyptian tombs. It is well to see thus that the actual remains that we find reach back to a time before the general soul-theory of later ages had yet been brought in.

But it is the later time of the historical development of the tomb that we have mainly to consider at present. The tombs that we actually have for study range continuously from about 4000 B.C. down to Roman times; but the principal age of consecutive development is from about 4000 to 2500 B.C. or the IVth to the XIIth dynasty. After that time no new ideas were introduced in the ordinary tombs, and only gradual decay and simplification is to be seen.

The earliest tombs of the simplest type, such as I have found in the cemetery of Dendereh, show only the essential parts. There is a sepulchral chamber under the ground (see Fig. 2); a square pit to reach that; a mound heaped over the pit, either of mere earth held together by a brick wall, or else of mud-brick throughout; and lastly a doorway figured always on the east face of the mound, at which the *ba*-bird was supposed to fly out, and the *ka*-ghost to walk out to receive the food which was offered to it. The essential parts of this door are (1) the lintel or *panel*, with a figure of the dead and his name and titles, over the door; (2) the *jambs* which

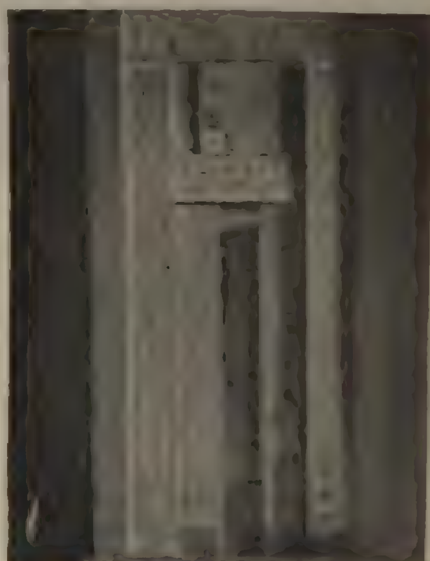

Fig. 3.—False door. Tomb of Ahat.

support this; (3) the *niche* or entrance between the jambs with a figure of the dead coming forth; with (4) a round roll or *drum*, imitated from a log lintel to the door, which generally bore only the name, with perhaps a short title. This doorway for the soul, or "false door," as it is now commonly called, is a most necessary part of the tomb; it became developed into a great monument in itself, and finally changed and dwindled down into the mere funeral tablet on a small scale. This whole raised mound and false door is known by the modern name of a *mastaba*, or "platform" in Arabic.

But the survivors craved to have some immediate token of the dead, to which their offerings might be made. If the *ka*, or spiritual

body, passed out through this door, why not give it some abiding place in its own likeness? And, to do this, what more natural than to picture it in the doorway? Such an image would be obviously a suitable abiding place for the wandering immaterial *ka*, where it could rest and be refreshed by the provision which was brought by its pious descendants. Accordingly, a figure in relief was sculptured in the doorway niche; and in front of that the food was laid, and the drink poured out into a trough of stone, on an altar of offerings that was placed before it.

The next step was to have a statue of the dead, so as to simulate the living person most completely. The more indistinguishable it

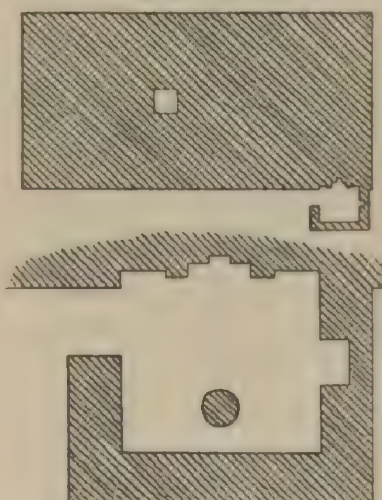

FIG. 4.—Tomb of Ka-aper, plan. Lower part is detail of upper plan five times larger. So also in the following plans.

was from life, the more happy the *ka* would be when inhabiting it. Thus a grand impulse was given to the most realistic art and the most expressive portraiture; and it is to this requirement that we owe the brilliant examples of Egyptian art that have come down to us. This statue, however, could not be left in the open air before a tomb, even in the Egyptian climate; it was too much exposed to injury, which would grieve and hurt the *ka*. So a little room was added in front of the false door, with a niche in which the sentient statue was preserved, as in the tomb of Ka-aper at Saqqara (Fig. 4). Here also the statue of his wife was found, which is one of the most life-like of these wooden figures that has been preserved to us. Here the statue was safe, and the family could visit it, and lay their

offerings before it. Yet the statue was exposed to possible injury. So the desire of the family to see it was subordinated to their wish to save it from harm, and it was walled in by screening off the end of a corridor before the tomb; the corridor itself being an enlargement of the statue-chamber, where the offerings were made. Such is seen in the tomb of Ka-mena, at El Kab.

The next step for the preservation of the statue was to deepen the recess of the false door so as to hold the statue within it. This was done in the tomb of Nefermaat at Medum. There a very deep niche contained the statue, safely walled in with solid masonry across

FIG. 5.—Wooden statue of wife of Ka-aper.

the entrance. Then the jambs of the doorway were expanded laterally to form a façade, but yet each made of one single stone. To protect and enlarge the mastaba, two successive coats of brickwork were added all round it. In placing the first it was not desired to hide the façade, so a cross passage was left in order that the sculptured stone façade should remain visible, and a direct passage was left through the brickwork. The outer coat of brick covered the entrance finally, and a court was added in front for the offerings. This is a particularly important link in the series, as we see how the wish to leave exposed the sculptured façade of the niche led to a cross passage being left inside the brick coating (Fig. 6).

Observe how in the next tomb, that of Rahotep at Medum, this cross passage has become incorporated in the primary construction, and a cruciform chamber of stone is the result. The statues of Rahotep and Nefert were placed in the two recesses thus formed, one on either side. Two coats of brickwork were superadded, so as to entirely close the chamber; a false door was made in the outer coat, and a court for offerings built before it, in which lay a large quantity of little cups and dishes of pottery. Meanwhile, a second false door in the same mastaba—that for the wife Nefert—remained in the undeveloped form of a simple niche, because there was no need for it to hold her statue, which was in her husband's chamber. So far,

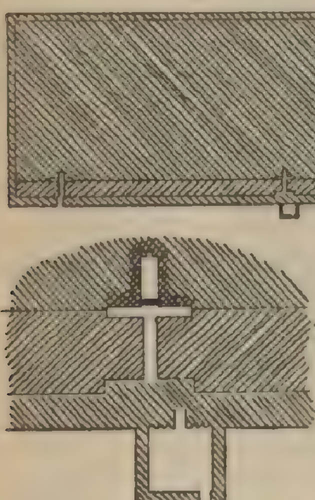

FIG. 6.—Plan of tomb of Nefer-maat.

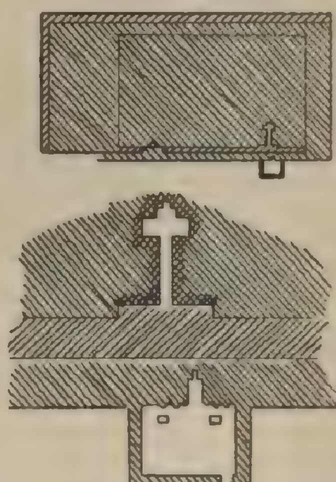

FIG. 7.—Plan of tomb of Rahotep.

the statues were safeguarded, but the family could see no more than a stranger could.

The next point of change was in the wish for the family to see the sculptures, and enter the chamber when they came with offerings; while yet the statue was to be better secured. This is seen done in the tomb of Seker-kha-bau (see Fig. 9). Here the end of the cross chamber is walled off to hold the statues, thus forming a separate closed cell for them; and this cell is commonly known to the modern natives as a *serd-ab*. The chamber itself retains the panelled construction typical of the mastaba face, showing that its true nature as a part of the primary mastaba was not forgotten, although it was

now enclosed in front to form a chamber within the mass. So fa
I have only dealt with tombs belonging to the first fifty years or
of which such remains are known coming a little later. The nex
step was to make a façade front to the chamber, and to bring out th
panelled pattern, or repetition of false doors, on to the outer fac
This is shown in the tomb of Ptahshepses at Saqqara.

Next a regular enclosure wall was put on before the tomb front
as we see in a tomb at Medum (No. 22). There the chamber is com
plete; but an outer passage has been added, and the serdab is walle
off at the end of it, just as it had before been walled off at the en

FIG. 8.—Head of Nefert, in limestone.

of the primitive passage which developed into the chamber. Anoth
pit or chamber appears in the mass, probably for casting th
funeral offerings in; as it was a custom to ascend the mound of
mastaba, and leave dishes and jars of offering on the top near th
mouth of the pit. The pit or well is to the right hand. When—
as here—the well has been moved away to the right, and the chamb
or false door to the left, it was because a passage had been develop
between the well and the funeral chamber; and thus the false do
was kept always close before the actual place of the body below.

We reach the full completion of this type, rather later on, in th
VIth dynasty tomb of Senna at Dendereh. There the passage

front is regularly formed with an entrance door, and it covers sixteen false doors along the front of the mastaba. The chamber has been lengthened out greatly. No serdab is to be seen, as that apparently was a Memphite feature unknown in the upper country. And the pit is long in order to allow of a coffin being lowered at full length with the body inside it.

Much the same construction appears in the large mastaba of Prince Mena of the VIth dynasty at Dendereh. Two pits appear there; that nearest the front leads to the funeral chamber lying behind the offering chamber. The further pit led to another chamber containing pottery, and was doubtless for the offerings. How this was reached is seen at the right hand, where a door from outside leads into a courtyard with a bench along two sides of it. From this court a flight of steps led on to the top of the mastaba; the blank part beyond the steps having been covered with their continuation upward, now denuded away. The squares across which the shading is carried are

FIG. 9.—Plan of tomb of Seker-kha-bau at Saqqara.

merely construction cells left hollow in the brickwork, and filled up with gravel.

The tomb was further elaborated by the addition of courts and chambers in front of the true mastaba. In the tomb of Nenkheftka at Saqqara, the chamber, its false door, and its serdab, with a slit through which the statue might receive its incense, are all within the mastaba. Subsequently three chambers were added on the front of the mastaba, to serve as an introduction to the rest.

This is seen further developed in the tomb of Ty at Saqqara, where the chamber has two false doors (for Ty and his wife), a serdab on the left of it, with three slits for censing the statues. A new supplementary chamber appears to the right of it. The front is enclosed so as to form a passage, in which is a false door as in other examples noticed. The new feature is a large court prefixed to this passage, containing twelve pillars, and approached by a porch with two pillars (see Fig. 10).

This type was carried further by prefixing the pillared cou[rt] directly in front of the chamber, as in a tomb at Dendereh. And th[e] same is carried out more fully in the tomb of Ateta at Saqqara.

Lastly, the court was incorporated entire in a single constructi[on] of the mastaba as a square block of building in the tomb of Ptahhot[ep] at Saqqara, in which the primary mastaba is lost sight of in the in[-] creasing complication of chambers.

Such complication was, however, only exceptional. On comi[ng] down about a thousand years later, we still find the old type [of] mastaba existing, as in that of Mentuhotep at Dendereh. There th[e] façade has thirteen false doors along it. The chamber has beco[me] lengthened out with a continuation to the whole length of the mastab[a] and an entrance appears in the north end of the mastaba, the purpo[se] of which we cannot now be certain about.

Fig. 10.—Plan of tomb of Ty.

The most distinct change in the later time, that is to say, abo[ut] the XIth dynasty, or 2800 B.C., was in the funeral pits. In all th[e] earliest tombs they are square: and soon after they were lengthene[d] out from north to south, and ran southward into the funeral chambe[r] which lay behind the false door. In the later time, however, the[y] were placed just behind the false door, with the chamber west of the[m] below. And they were therefore lengthened from east to west, i[n] order to pass the coffin more conveniently into the chamber. Thi[s] distinction in the direction of the pit, at first north to south, and lat[er] on east to west, is one of the first tests of the age of a mastaba. Oft[en] two pits were made side by side, as here, leading each to a chambe[r] apparently for the husband and wife separately. One false doo[r] served for both of them, and this would not be unlikely, as the wif[e]

is often placed together with her husband on his stele in the false door. One tomb is peculiar for having an annex on the south, with a long chamber but no false doors. The doorway left in the wall between the two is probably merely structural, as both mastabas were filled up solid with gravel. Such annexes occur in other cases, and are as yet unexplained.

A usual feature of the XIth and XIIth dynasty mastabas—at least at Dendereh—is to revert to the early type where the passage-chamber opened from the end. In one case there is a mixed form with the front entrance still made, and yet the end open.

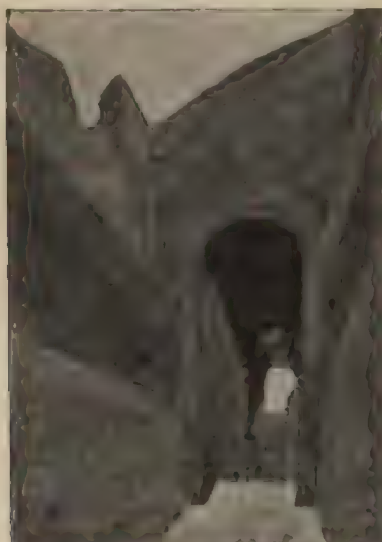

Fig. 11.—Brickwork tunnel in tomb of Adu I.

We now pass from the consideration of the plans of these tombs, in which we have seen every stage of development, from the primitive mound with a niche in the side of it, to the elaborate mass of chambers for various funeral purposes, and we turn back to note the development in the sections of the great tombs of the feudal princes.

The earliest example is one at Medum, where we see the central pit not opening directly into a chamber but into a sloping passage which leads to the chamber. So far we have not found any early tombs (except pyramids) which have a sloping entrance passage, and that type does not seem to have ever been adopted for small tombs, but only for those belonging to rulers.

In the later part of the old kingdom, about 3400 B.C., we have a splendid series of tombs of the Princes of Dendoreh, built upon the type of the sloping passage. Adu I. built a grand vaulted tunnel of brickwork, which led down to the funeral pit. This tunnel has four rings of brickwork in the vault arch, and is finely built. It would be set down as Roman by most persons, but in the last few years we have pushed back the history of the Egyptian arch of brick to the XIXth dynasty, then to the XIIth, and now to the VIth dynasty. Probably it began even earlier, but it is here in full use at 3500 B.C.

In the section the entrance is through an arched doorway in the outer wall. That opened on a very narrow court or passage, in which a stairway led to the top of the mastaba, as in Mena's. This court was filled up with brickwork to cover the entrance to the tunnel.

FIG. 12.—Sections of tombs of Adu I. and Adu II.

The tunnel ran down at a steep slope, the roof of it afterwards turning horizontal to meet the wall at the tower, and it was walled up. The well did not cause any break in the floor, and scarcely any on the side of the passage, which runs on downward in the rock to the funeral chamber. Two small chambers at the sides of the passage contained funeral offerings of pottery, &c. Entering the chamber, it is of a T form, wide on either hand, and then narrowing to a long recess of just the width of the sarcophagus lid. The sarcophagus itself is sunk in the rock floor, and the lid lay on the floor, or possibly with a pavement flush with the top. The whole chamber and coffin recess was lined with sculptures of offerings; this provision for the support of the *ka* having been at this age transferred down from the place of offerings above to the actual place of the body below the ground. This tomb is the most complete of this type, and enables us to understand the others which follow it.

The next tomb, that of Prince Adu II., has the same arched doorway. The passage is much steeper, as they wished to reach the same depth more quickly. The well is at the end of the passage, and not intersecting it midway. The chamber is T-shaped, as before; but it is lined with bricks, and had brick vaults for roofing each part; all of these have now fallen in, together with much of the gravel rock above.

The plan of Adu II. has the offering chamber and well in the usual positions. But, in addition, there is a second well in the N.W. corner, which was, doubtless, for his wife Ana, who appears on a tablet with Adu; in the chamber at the bottom was a female skull. The chamber of the second well was to the south, so that it came nearly behind the second false door in the upper chamber of offering. The large false door is exactly in front of the place of the sarcophagus in the main funereal chamber. The front of this mastaba has a full development of the false-door decoration: twelve doors on one hand, and eighteen on the other, thirty in all. A feature of these large mastabas of the nobles is the provision of tombs for their families near them, much as several of the kings had the small pyramids of their family adjoining their own pyramid. This plan is most distinct in this mastaba, where a court is added on at the south end, containing nine pit tombs for the family of Adu, beside a tenth in front of the false doors.

The next tomb shows a new departure in construction. The very steeply sloping passage of Adu II. had probably caused trouble in making the barrel roof of it—an early settlement of the lower part is to be seen. So a new idea appears in the providing a horizontal barrel roof to a sloping passage, thus keeping all the brickwork level, while the floor rapidly descends. The result is a passage which is about fifteen feet high at the end. The well is put nearer to the end of the passage, and the sloping floor continues down past it into the chamber. This lower, or funereal chamber, has so much caved in that the details are lost.

Having thus succeeded in economising material by the construction of lofty hollows vaulted over, the same principle was carried further in the mastaba of Prince Merra. Here an entrance passage opens into a court, from which a flight of steps led to the top. But there is no doorway from this court into the passages. The only entrance was by a well behind the court, which led to a high vaulted passage with sloping floor. This passage was lighted by a high-up archway, at the deep end of it opening on to a well shaft. Beyond the wall was another lofty passage chamber with a domed roof, and through this the funereal chamber was reached. This was much simpler and poorer than before, not having any lateral branches, but being merely a place large enough to get in the sarcophagus and place it to one side. Nor was there any sculpturing of the sides, or indeed any lining.

The last stage that we have found in this series is that of Prince Beb, where the well of entrance and the second well are placed near together, and nothing comes between the high-vaulted sloping passage and the funereal chamber. In this last there is no inscription on the outside of the mastaba nor on the chamber; but the whole care was given to crowding the inside of the coffin with very lengthy magic texts. This seems to mark a change of belief, from the earlier idea of the *ka* wandering about from the tomb, inhabiting its statue, and accepting its offerings, to the different idea of the importance of the mummy and the need of its having the preservative charms as close to it as possible. Thus in this series of tombs we have seen the earliest at Medum, with a central well and sloping entrance to the chamber; the long sloping passage of Adu I. prefixed to the well entrance; the well pushed on to near the chamber in Adu II.; the start of high-vaulted spaces in the next tomb; the extension of these large spaces in order to economise material, with barrel and domed roofs; and, lastly, the rearrangement of the parts. If we could extend this chain onward beyond the century or two which it covers, we should doubtless be able to trace many more changes into diverse forms; but the lack of material is our difficulty, and it is only this spring in my work at Dendereh that the present series has come to light.

I do not propose here to deal with the series of changes to be seen in the construction of pyramids, as that alone would be a large subject. But we may notice how the earliest type of pyramid starts from the mastaba with a long sloping passage. The royal mastaba tomb of Seneferu had such a passage, starting—as do these passages of the princes' tombs—from the ground level. The next stage was to add a coat of masonry around the pyramid like the successive coats around Rahotep's mastaba, and to continue the original mass upward. This was done seven successive times, each time supposed to be the last, as the masonry was finely finished off with polished surfaces. Finally came the idea of putting one continuous coat from top to base, and so the first pyramid came into existence. When once this form was started, the later kings designed their pyramids at one stroke and had no such intermediate steps of construction; this is obvious when we look at the arrangement of the internal passages. So we must by no means suppose that because the first pyramid was thus developed, that therefore every pyramid went through the same stages.

Of the later times of the Egyptian kingdom very little architectural material has been examined from the cemeteries. In the XXVIth Dynasty, about 600 B.C., tombs were made with a well shaft, and one chamber or several at the bottom of it under the ground, but we know nothing of the surface buildings. Too often any rich tomb was provided by ejecting the former occupier of some noble structure. The stages of the latest degradation can be traced. The deep well and chamber became shortened and simplified in the Ptolemaic times

At the end of that period the chamber was made still smaller, and the coffin was left projecting into the well. Then it was simply placed in the well, which became thus a deep grave and nothing more. In Roman times the well was made shallower stage by stage, until at last it became a mere shallow grave, only two or three feet deep. Finally the whole system of preserving the body and burying a special class of funeral objects came to an end with Christianity in Egypt, when the body was buried in the clothes worn during life, and any objects buried with it were those which had been actually used by the person.

[NOTE.—Although the lower edge of the plans is east and the top west, yet the reader's right hand is south and left hand north, owing to the plans having been reversed in making the blocks.]

[W. M. F. P.]

GENERAL MONTHLY MEETING,

Monday, June 6, 1898.

SIR JAMES CRICHTON-BROWNE, M.D. LL.D. F.R.S. Treasurer and Vice-President, in the Chair.

Arthur Wemyss Horsbrugh, Esq.

was elected a Member of the Royal Institution.

The Special Thanks of the Members were returned for the following Donations to the Fund for the Promotion of Experimental Research at Low Temperatures:—

	£	s.
Mrs. G. J. Romanes	5	1
Sir Frederick Bramwell, Bart.	100	0
Professor Dewar	100	0
Dr. Ludwig Mond	200	0
Charles Hawksley, Esq.	100	0
Sir David Salomons, Bart.	21	0
Dr. Rudolph Messel	100	0

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The French Government—Documents Inédits sur l'Histoire de France: Lettres de Catherine de Médicis, Tome VI. 1578–79. 4to. 1897.

Topographie Historique du Vieux Paris. Région Centrale de l'Université. 4to. 1897.

The Lords of the Admiralty—Nautical Almanac Circular, No. 17. 8vo. 1898.

British Astronomical Association—Journal, Vol. VIII. No. [illegible]
Camera Club—Journal for May, 1898. 8vo.
Chemical Industry, Society of—Journal, Vol. XVII. No. 4.
Chemical Society—Journal for May, 1898. 8vo.
Proceedings, No. 195. 8vo. 1898.
Cornwall, Royal Institution of—Journal, Vol. XIII. Part 2.
Cracovie, l'Académie des Sciences—Bulletin International, 1
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Dax, Société de Borda—Bulletin, 1897, No. 4. 8vo.
Editors—American Journal of Science for May, 1898. 8vo.
Analyst for May, 1898. 8vo.
Anthony's Photographic Bulletin for May, 1898. 8vo.
Astrophysical Journal for May, 1898. 8vo.
Athenæum for May, 1898. 4to.
Author for May, 1898.
Bimetallist for May, 1898.
Brewers' Journal for May, 1898. 8vo.
Chemical News for May, 1898. 4to.
Chemist and Druggist for May, 1898. 8vo.
Education for May, 1898. 8vo.
Electrical Engineer for May, 1898. fol.
Electrical Engineering for May, 1898.
Electrical Review for May, 1898. 8vo.
Engineer for May, 1898. fol.
Engineering for May, 1898. fol.
Homœopathic Review for May, 1898.
Horological Journal for May, 1898. 8vo.
Industries and Iron for May, 1898. fol.
Invention for May, 1898. 8vo.
Journal of Physical Chemistry for May, 1898. 8vo.
Journal of State Medicine for May, 1898. 8vo.
Law Journal for May, 1898. 8vo.
Machinery Market for May, 1898. 8vo.
Nature for May, 1898. 4to.
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Physical Review for April, 1898. 8vo.
Public Health Engineer for May, 1898. 8vo.

Iron and Steel Institute—Journal, Name Index, Vols. I.–L. 8vo. 1898.
Ives, F. E. Esq. (the Author)—Krömsköp Colour Photography. 8vo. 1898.
Janet, Charles, Esq. (the Author)—Natural History Papers. 1897. 8vo and fol.
Jervis, Chevalier G. (the Author)—Guida alle Acque Minerali d'Italia. Provincie Meridionali. By G. Jervis. 8vo. 1896.
Johns Hopkins University—American Chemical Journal for May, 1898. 8vo.
Life-Boat Institution, Royal National—Annual Report for 1898. 8vo.
London County Council Technical Education Board—London Technical Education Gazette for April–May, 1898. 8vo.
Manchester Geological Society—Transactions, Vol. XXV. Part 15. 8vo. 1898.
Manchester Literary and Philosophical Society—Memoirs and Proceedings, Vol. XLII. Part 2. 8vo. 1897–98.
Manchester Steam Users' Association—Boiler Explosions Acts. Reports, Nos. 957–1036. fol. 1897.
Meteorological Society, Royal—Meteorological Record, No. 67. 8vo. 1898.
Quarterly Journal, No. 106. 8vo. 1898.
Navy League—Navy League Journal for May, 1898. 4to.
Numismatic Society—Chronicle and Journal, 1898, Part I. 8vo.
Odontological Society of Great Britain—Transactions, Vol. XXX. Nos. 6, 7. 8vo. 1898.
Paris, Société Française de Physique—Séances, 1897, Fasc. 3. 8vo.
Bulletin, Nos. 114–116. 8vo. 1898.
Pharmaceutical Society of Great Britain—Journal for May, 1898. 8vo.
Phillips, Charles E. S. Esq. M.R.I.—Submarine Telegraphs: Their History, Construction and Working. By C. Bright. 8vo. 1898.
Photographic Society of Great Britain, Royal—The Photographic Journal for April, 1898. 8vo.
Rochechouart, La Société les Amis des Sciences et Arts—Bulletin, Tome VII. Nos. 4–6. 8vo. 1897–98.
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Royal Society of London—Philosophical Transactions, Vol. CXCI. A, Nos. 216–218. 4to. 1898.
Proceedings, Nos. 393–395. 8vo. 1898.
Saxon Society of Sciences, Royal—
Philologisch-Historische Classe—
Berichte, 1898, No. 1. 8vo.
Selborne Society—Nature Notes for May, 1898. 8vo.
Society of Arts—Journal for May, 1898. 8vo.
Tacchini, Prof. P. Hon. Mem. R.I. (the Author)—Memorie della Società degli Spettroscopisti Italiani, Vol. XXVII. Disp. 3. 4to. 1898.
Tasmania, Royal Society of—Papers and Proceedings for 1897. 8vo. 1898
Teyler Museum, Haarlem—Archives, Ser. II. Vol. V. Part 4; Vol. VI. Part 1. 8vo. 1898.
Thorpe, W. G. Esq. (the Author)—The Hidden Lives of Shakespeare and Bacon. 8vo. 1897.
Toulouse, Société Archéologique du Midi de la France—Bulletin, Series in 8vo, No. 20. 8vo. 1897.
United Service Institution, Royal—Journal for May, 1898. 8vo.
Verein zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, 1898, Heft 4, 5. 4to.
Vienna, Geological Institute, Imperial—Verhandlungen, 1898, Nos. 3–8. 8vo.
Whitworth, The Rev. William A. M.A. M.R.I. (the Author)—The Expectation of Parts, into which a magnitude is divided at random. 8vo. 1898.
Zoological Society of London—Proceedings, 1897, Part 4. 8vo. 1898.

WEEKLY EVENING MEETING,

Friday, June 10, 1898.

Sir William Huggins, K.C.B. D.C.L. LL.D. F.R.S. Vice-President, in the Chair.

The Right Hon. Lord Rayleigh, M.A. D.C.L. LL.D. F.R.S. *M.R.I.* Professor of Natural Philosophy, *R.I.*

Some Experiments with the Telephone.

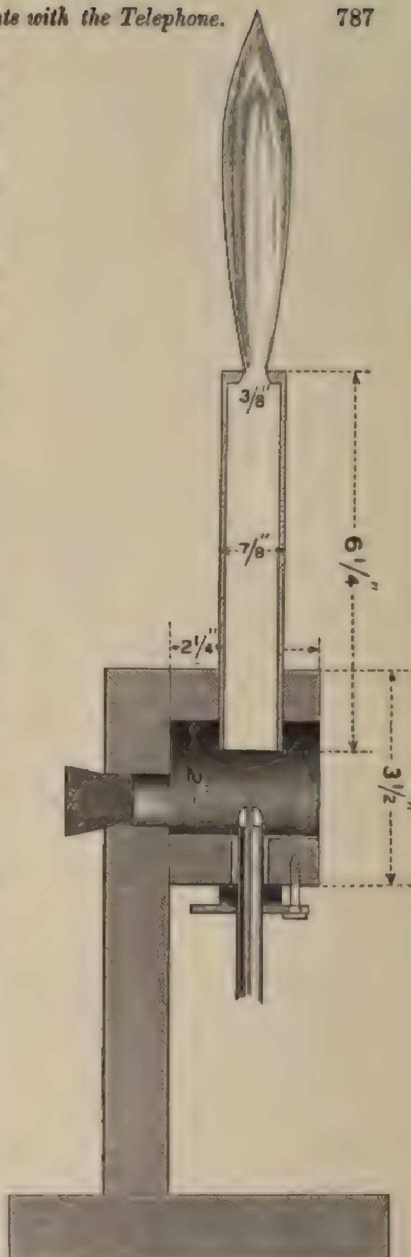
Early estimates of the minimum current of suitable frequency audible in the telephone having led to results difficult of reconciliation with the theory of the instrument, experiments were undertaken to clear up the question. The currents were induced in a coil of known construction, either by a revolving magnet of known magnetic moment, or by a magnetised tuning-fork vibrating through a measured arc. The connection with the telephone was completed through a resistance which was gradually increased until the residual current was but just easily audible. For a frequency of 512 the current was found to be 7×10^{-8} ampères.* This is a much less degree of sensitiveness than was claimed by the earlier observers, but it is more in harmony with what might be expected upon theoretical grounds.

In order to illustrate before an audience these and other experiments requiring the use of a telephone, a combination of that instrument with a sensitive flame was introduced. The gas, at a pressure less than that of the ordinary supply, issues from a pin-hole burner † into a cavity from which air is excluded (see figure). Above the cavity, and immediately over the burner, is mounted a brass tube, somewhat contracted at the top where ignition first occurs.‡ In this arrangement the flame is in strictness only an indicator, the really sensitive organ being the jet of gas moving within the cavity and surrounded by a similar atmosphere. When the pressure is not too high, and the jet is protected from sound, the flame is rather tall and burns bluish. Under the influence of sound of suitable pitch the jet is dispersed. At first the flame falls,

* The details are given in 'Phil. Mag.' vol. xxxviii. p. 285 (1894).

† The diameter of the pin-hole may be 0·03".

‡ 'Camb. Proc.' vol. iv. p. 17, 1880.



becoming for a moment almost invisible; afterwards it assumes a more smoky and luminous appearance, easily distinguishable from the unexcited flame.

When the sounds to be observed come through the air, they find access by a diaphragm of tissue paper with which the cavity is faced. This serves to admit vibration while sufficiently excluding air. To get the best results the gas pressure must be steady, and be carefully adjusted to the maximum (about 1 inch) at which the flame remains undisturbed. A hiss from the mouth then brings about the transformation, while a clap of the hands or the sudden crackling of a piece of paper often causes extinction, especially soon after the flame has been lighted.

When the vibrations to be indicated are electrical, the telephone takes the place of the disc of tissue paper, and it is advantageous to lead a short tube from the aperture of the telephone into closer proximity with the burner. The earlier trials of the combination were comparative failures, from a cause that could not at first be traced. As applied, for instance, to a Hughes' induction balance, the apparatus failed to indicate with certainty the introduction of a *shilling* into one of the cups, and the performance, such as it was, seemed to deteriorate after a few minutes' experimenting. At this stage an observation was made which ultimately afforded a clue to the anomalous behaviour. It was found that the telephone became dewed. At first it seemed incredible that this could come from

the water of combustion, seeing that the lowest part of the flame was many inches higher. But desiccation of the gas on its way to the nozzle was no remedy, and it was soon afterwards observed that no dewing ensued if the flame were all the while under excitation, either from excess of pressure or from the action of sound. The dewing was thus connected with the *unexcited* condition. Eventually it appeared that the flame in this condition, though apparently filling up the aperture from which it issues, was nevertheless surrounded by a descending current of air carrying with it part of the moisture of combustion. The deposition of dew upon the nozzle was thus presumably the source of the trouble, and a remedy was found in keeping the nozzle warm by means of a stout copper wire (not shown) conducting heat downwards from the hot tube above.

The existence of the downward current could be made evident by private observation in various ways, perhaps most easily by projecting little scraps of tinder into the flame, whereupon bright sparks were seen to pass rapidly downwards. In this form the experiment could not be shown to an audience, but the matter was illustrated with the aid of a very delicate ether manometer devised by Professor Dewar. This was connected with the upper part of the brass tube by means of a small lateral perforation just below the root of the flame. The influence of sound and consequent passage of the flame from the unexcited to the excited condition was readily shown by the manometer, the pressure indicated being less in the former state of things.

The downward current is evidently closely associated with the change of appearance presented by the flame. In the excited state the gas issues at the large aperture above as from a reservoir at very low pressure. The unexcited flame rises higher, and must issue at a greater speed, carrying with it not only the material supplied from the nozzle, and constituting the original jet, but also some of the gaseous atmosphere in the cavity surrounding it. The downward draught thus appears necessary in order to equalise the total issue from the upper aperture in the two cases.

Although the flame falls behind the ear in delicacy, the combination is sufficiently sensitive to allow of the exhibition of a great variety of interesting experiments. In the lecture the introduction of a threepenny piece into one of the cups of a Hughes' induction balance was made evident, the source of current being three Leclanché cells, and the interrupter being of the scraping contact type actuated by clockwork.

Among other experiments was shown one to prove that in certain cases the parts into which a rapidly alternating electric current is divided may be greater than the whole.* The divided circuit was formed from the three wires with which, side by side, a large flat

* See 'Phil. Mag' vol. xxii. p. 496 (1886).

coil is wound. One branch is formed by two of these wires connected in series, the other (in parallel with the first), by the third wire. Steady currents would traverse all three wires in the same direction. But the rapidly periodic currents from the interrupter distribute themselves so as to make the self-induction, and consequently the magnetic field, a minimum; and this is effected by the assumption of opposite values in the two branches, the ratio of currents being as $2 : -1$. On the same scale the total or main current is $+1$. It was shown by means of the telephone and flame that the current in one branch was about the same (arithmetically) as in the main, and that the current in the other branch was much greater. [R.]

GENERAL MONTHLY MEETING,

Monday, July 4, 1898.

Sir James Crichton-Browne, M.D. LL.D. F.R.S. Treasurer and Vice-President, in the Chair.

The Special Thanks of the Members were returned for the following Donations to the Fund for the Promotion of Experimental Research at Low Temperatures:—

	£
Sir Frederick Abel, Bart. K.C.B.	100
Sir Andrew Noble, K.C.B.	100
Sir John Brunner, Bart. M.P.	50

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Lords of the Admiralty—Greenwich Observations, 1895. 4to. 1897.
Greenwich Spectroscopic and Photographic Results, 1895. 4to. 1897.
Cape Meridian Observations, 1892 to 1895. 4to.
The Cape Photographic Durchmusterung for the Equinox, 1875, Vol. II. 4to.
Annals of the Cape Observatory, Vol. IV. 4to.
Report of the Astronomer-Royal to the Board of Visitors, 1898. fol.

The British Museum Trustees—Facsimiles from Early Printed Books in the British Museum. fol. 1897.
Catalogue of the Stowe MSS. Vol. I. Text; Vol. II. Index. 8vo. 1895–96.
Catalogue of Greek Coins: Ionia (1892); Mysia (1892); Alexandria and the Nomes (1892); Troas, Æolis and Lesbos (1894); Caira, Cos, Rhodes, &c. (1897); Lycia, Pamphylia and Pisidia (1897). 8vo.
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Aburrow, Charles, Esq. M. Inst. C.E. (the Compiler)—Annual Report relating to the Public Works Department of the Stadsraad, Johannesburg, S.A.R. fol. 1897.

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Memoirs, Vol. V. No. 3. 4to. 1898.
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British Astronomical Association—Journal, Vol. VIII. No. 7. 8vo. 1898.
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Cambridge Observatory Syndicate—Astronomical Observations made at the Observatory, Vol. XXIII. 1872-75. 4to. 1898.
Cambridge Philosophical Society—Proceedings, Vol. IX. Part 8. 8vo. 1898.
Cambridge University Library Syndicate—Annual Report, 1897. fol.
Camera Club—Journal for June, 1898. 8vo.
Chemical Industry, Society of—Journal, Vol. XVII. No. 5. 8vo. 1898.
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Proceedings, Nos. 196, 197. 8vo. 1897.
Chicago, Field Columbian Museum—Bulletins: Botanical Series, Vol. I. No. [illegible] Anthropological Series, Vol. II. No. 2; Zoological Series, Vol. I. Nos. 9, 1[illegible] 8vo. 1898.
Clowes, Frank, Esq. F.C.S. M.R.I. (the Author)—The Detection and Measurement of Inflammable Gas and Vapour in the Air. By F. Clowes and B. Redwood. 8vo. 1896.
A Treatise on Practical Chemistry and Qualitative Analysis. 6th ed. 8vo. 1895.
Quantitative Chemical Analysis. By F. Clowes and J. B. Coleman. 8vo. 1897.
Constable, Messrs. T. and A. (the Publishers)—The twenty-sixth volume of the Publications of the Scottish History Society, containing "Diary of Lord Wariston," "Preservation of the Honours of Scotland," "Lord Ma[illegible] Legacies," "Highland Affairs in the 18th Century." 8vo. 1896.
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Brewers' Journal for June, 1898. 8vo.
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Chemist and Druggist for June, 1898. 8vo.
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Electricity for June, 1898. 8vo.
Engineer for June, 1898. fol.

Editors—continued.
Engineering for June, 1898. fol.
Homœopathic Review for June, 1898. 8vo.
Horological Journal for Dec. 1895, March and Nov. 1897, and June, 1898. 8vo.
Industries and Iron for June, 1898. fol.
Invention for June, 1898.
Journal of Physical Chemistry for June, 1898. 8vo.
Journal of State Medicine for June, 1898. 8vo.
Law Journal for June, 1898. 8vo.
Lightning for June, 1898. 8vo.
Machinery Market for June, 1898. 8vo.
Nature for June, 1898. 4to.
New Church Magazine for June, 1898. 8vo.
Nuovo Cimento for May, 1898. 8vo.
Photographic News for June, 1898. 8vo.
Physical Review for May-June, 1898. 8vo.
Public Health Engineer for June, 1898. 8vo.
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Jordan, Wm. L. Esq. M.R.I. (the Author)—The Admiralty Falsification of the "Challenger" Record. 8vo. 1890.

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Meriden Scientific Association—Transactions, Vol VIII. 8vo. 1897–98.

Mersey Commissioners—Report on the present state of the Navigation of the River Mersey, 1897. By Sir G. S. Nares. 8vo. 1898.

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Royal Society of Edinburgh—Proceedings, Vol. XXII. No. 1. 8vo. 1897-98.
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Proceedings, Nos. 396-398. 8vo. 1898.
St. Petersburg, Académie Impériale des Sciences—Mémoires, Tome V. Nos. 6-13; Tome VI. Nos. 1-3, 5. 8vo. 1897-98.
Sanitary Institute—Journal, Vol. XVII. Part 3. 8vo. 1896.
Scottish Microscopical Society—Proceedings, Vol. II. No. 2. 8vo. 1896-97.
Selborne Society—Nature Notes for June, 1898. 8vo.
Smithsonian Institution—A Catalogue of Earthquakes on the Pacific Coast, 1769-1897. By E. S. Holden. (Smith. Misc. Coll.) 8vo. 1898.
Society of Arts—Journal for June, 1898. 8vo.
Tacchini, Prof. P.—Memorie della Società degli Spettroscopisti Italiani, Vol. XXVII. Disp. 4. 4to. 1898.
Tarleton, Alfred H. Esq. M.R.I. (the Author)—Nicholas Breakspear (Adrian IV.), Englishman and Pope. 4to. 1896.
United Service Institution, Royal—Journal for June, 1898. 8vo.
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United States Department of the Interior—Report of the Secretary of the Interior, 1895-96, Vol. IV. Parts 1-4. 4to. 1896.
Report of the Secretary of the Interior, 1896. 5 vols. 8vo. 1896-97.
United States Patent Office—Official Gazette, Vol. LXXXIII. Nos. 2-5. 8vo. 1898.
University of London—Calendar, 1898-99. 8vo. 1898.
Victoria Institute—Journal, No. 118. 8vo. 1898.
Vienna, Imperial Geological Institute—Jahrbuch, Band XLVII. Heft 2. 8vo. 1897.
Abhandlungen, Band XVII. Heft 4. 4to. 1897.
Wagner Free Institute of Science, Philadelphia—Transactions, Vol. V. 8vo. 1898.
Yorkshire Archæological Society—Yorkshire Archæological Journal, Part 57. 8vo. 1898.
List of Members and Catalogue of Library. 8vo. 1898.
Zoological Society of London—Proceedings, 1898, Part 1. 8vo.
Transactions, Vol. XIV. Part 6. 4to. 1898.
Zürich, Naturforschende Gesellschaft—Vierteljahrsschrift der Naturforschenden Gesellschaft, 1898, Heft 1. 8vo. 1898.

GENERAL MONTHLY MEETING.

Monday, November 7, 1898.

SIR JAMES CRICHTON-BROWNE, M.D. LL.D. F.R.S. Treasurer and Vice-President, in the Chair.

The Special Thanks of the Members were returned for the following Donations to the Fund for the Promotion of Experimental Research at Low Temperatures:—

John B. Carrington, Esq.	£25
Charles Scott Dickson, Esq. Q.C.	£100

THE PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Lords of the Admiralty—Report of Her Majesty's Astronomer at the Cape of Good Hope for 1897. 4to. 1898.

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GENERAL MONTHLY MEETING,

Monday, December 5, 1898.

SIR JAMES CRICHTON-BROWNE, M.D. LL.D. F.R.S. Treasurer and Vice-President, in the Chair.

Herbert William Allingham, Esq., F.R.C.S.
T. Newbold Piddocke, Esq.
Edward Preedy, Esq.
William Munro Tapp, Esq. LL.D.
The Hon. William Frederick Cuthbert Vernon,
Mrs. Adela Wetzlar,
Charles Theodore Williams, M.A. M.D. F.R.C.P.

were elected Members of the Royal Institution.

The Special Thanks of the Members were returned to Dr. George Wyld for his present of a Portrait of Dr. Thomas Garnett, the first Professor in the Royal Institution.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

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WEEKLY EVENING MEETING,

Friday, February 25, 1898.

Sir Frederick Abel, Bart. K.C.B. D.C.L. LL.D. F.R.S. Vice-President, in the Chair.

Captain Abney, C.B. D.C.L. F.R.S. *M.R.I.*

The Theory of Colour Vision applied to Modern Colour Photography.

The subject of my address this evening is a very large one, and would occupy more time than the hour allotted to me, if I entered fully into every part of it. All I can hope to do is to put before you the main scientific reasoning which has led to the success at present attained in colour photography, by a combination of colours, and by the absorption of colouring matter.

On the screen we have the spectrum of the electric light, and a very beautiful object it is. But it is not to its beauty that I wish to call your attention, but to the varying brightness of its different parts, and further, to the fact that in it we have strictly pure colours, that is a series of simple colours, and not mixed colours such as we may find in nature. Now if we can reproduce fairly well by means of photography this grand multi-coloured band, both as regards colour and also brightness (that is luminosity), we may say that we have succeeded in doing what is required, and that all hues in nature, with their varying shades and brightness, can be equally well reproduced. The exponent of colour photography is bound to go to the spectrum for his information, and this I must do to-night. On the wall is a diagram of the spectrum in the shape of a curve, which shows the luminosity of every individual part. If we could abolish colour from our minds, and merely look upon the spectrum as a monochromatic band having waves of different oscillation frequency, we should have this same curve, and our eyes would be like a photographic plate, which knows no colour quâ colour. All that the plate knows is that a certain wave length, having a certain amplitude, will so affect its sensitive surface that a certain opacity of deposit will be attained on applying the developer to it. If two or more colours are mixed, each of the wave lengths will play its own part, and an opacity will be produced representing the sum of the separate effects. A little reflection will show that whatever photographs we may obtain we must use outside coloured light to illuminate them if a coloured object is to be reproduced. We have to consider what are the fewest

colours that we can use, for evidently simplicity is a great desideratum, and the number agreed upon must settle the number of separate photographs required.

This brings us to the question of how we see colour, and how many sensations of colour we have. I am not going into the debatable ground of rival colour-vision theories, but I am going to adopt for to-night that one which will answer every practical purpose, and that is the Young theory, in which it is held, and held correctly, that a red, a green, and a blue sensation are alone needful to produce the sensation of any other hue by admixture one with another. The fundamental colour sensations are not necessarily identical with any particular colour, but as a matter of fact, at all events one of these sensations is to be found excited singly in the spectrum, viz. that which is excited by the extreme red. The extreme violet seems to be a compound of two sensations, one a deep blue and the other red, so that the pure blue and also the green sensations can never be singly stimulated in the normal eye. The diagram shows these sensations as curves representing the stimulations by the spectrum colours of the seeing apparatus in the retina (Fig. 1). The scales on each curve are so adapted that when the ordinates of the sensation curves are equal we get white. To get a yellow, the red sensations and green sensations are equally stimulated, for there the curves cut. It will be seen that the purest green sensation is largely mixed with white, for at one point, where the red and blue curves cut, the green curve is above them. At that point, then, the red and

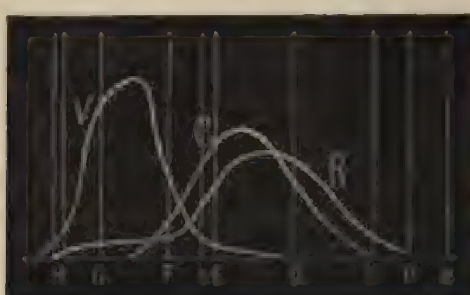


Fig. 1.

blue, and a certain portion of green, go to form white, and the balance is green, so here the pure green sensation is diluted with white. At any other point it is mixed with some other sensation, either red or blue, and also up to certain points with white. Of course, if we could get three colours which only stimulated respectively the three fundamental sensations, we should take three appropriate photographs of the spectrum and illuminate them with those three colours.

But the amount of white which is in the purest green sensation renders it desirable to choose a green which has less white inherent in it, to prevent the mixture being pale.

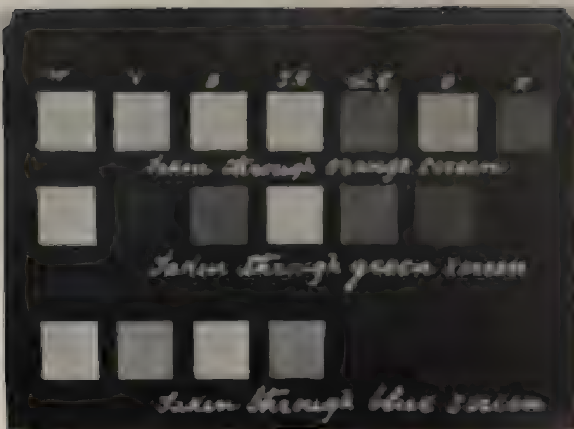
In 1861, Clerk Maxwell gave a lecture in this theatre, in which the method of producing photographs in the colours of nature by means of illuminating three photographic pictures, and combining the images together, was foreshadowed, and it is to this, and to his

FIG. 2.—Maxwell's Curves of Colour Sensations.

original work on the mixture of colours, that we must turn. By means of what he called his colour-box, he could mix any three colours of the spectrum together, and, for reasons which appeared adequate at the time, he took a bright red, a bright green, and a bright blue of the spectrum as best representing the sensations. He referred all other colours of the spectrum to these, and expressed them as mixtures of the three. The diagram that he made is given (Fig. 2). The heights of the different curves he obtained by measuring the width of the three slits through which any three chosen colours came, and making such widths the ordinates. The standard red he chose was a red containing a little green; the standard green near E is nearly free from white, but a glance at the diagram (Fig. 1) will show it is mixed with a certain amount of red; Maxwell's blue contained a certain quantity of green. This is merely history, but it may be remarked that where we are dealing with colours, and not sensations, the colours he chose are probably nearly the best for the purpose we have in view. I have reduced the Maxwell curves so as to represent luminosity as well as colour, and you will see that they all fit into the spectrum curve, and that the great mass of brightness is due to the green and red. Of blue there is but very little. These curves should be kept well in your mind.

We need not trouble you much about colour mixtures. I have an apparatus here which allows us to mix any colours together

Fig. 3.



White. Violet. Blue Peacock Green. Chromium Green. Orange. Red.

Fig. 4.

Red Image. Green Image. Blue Image.

Fig. 5.

1.
2.
3.
4.

Li D Li H

Fig. 6.

1.
2.
3.

Li D Li H

In the small spectrum we can place three slits and make a patch of white light. By altering the width of one or two of the slits we can form colours of any hue. [*White was here matched, and three other colours made, and again white.*] Instead of light being diminished by alteration of the width of the slit, we can cut off varying quantities from each ray and allow them to impress the retina for different times, the persistence of vision blending the impressions together. In fact, by an artifice of the kind I have here, which consists of a long band of paper punctured along the three lines of the slits with holes of different sizes, and passing the strip in front of the slits, we can play a regular tune in colour. [*Shown.*]

But we can get these same tunes of colour, though not quite so pure (i.e. unmixed with white) if we use considerable parts of the spectrum. The slits are withdrawn, and all those parts of the spectrum which can come through the holes are mixed together and the colours are reproduced, but not in quite such purity as before.

There is another method of altering the intensity of the rays, and that is by placing in front of the slits photographic deposits of different opacities, and you see that we have, as before, different colours produced. The diagram (Fig. 3) shows a print of the deposits employed. The three rows represent the transparencies of white, violet, blue, peacock blue, dark green, orange and red, taken through an orange, a green, and a blue screen respectively. The three left-hand squares in the transparency covered the three slits, and white was formed on the screen. The next three squares gave a violet, the next three a blue, and so on. This is the foundation of colour photography. Having learnt that the colours mixed together need not be single rays of the spectrum, but may occupy adjacent parts on each side of the single ray and still produce approximately the same results, we can go a step further, as it shows that we may use the light coming through media such as coloured glasses instead of pure spectrum colours.

An interesting experiment is to imitate the spectrum by means of a red, a blue and a green glass. A slit is placed in the lantern, and an image of it thrown on the screen. We have a disc formed of these three glasses, each being shaded by an appropriate mask, to imitate the extent of each sensation in the spectrum. This disc rotates in front of the slit. The varying combinations give a large range of colour, and we have a tolerable representation of the spectrum produced.

I think now we are in a position to realise what is required in order to reproduce by photography the spectrum with all its colours. We must get three photographic negatives, each one of which will take in only so much of the spectrum as is represented by the colour sensations as shown in the diagram, and secure that the brilliancy of the light coming through the transparencies or positives taken from the negatives at each part shall be represented by the heights of the curves, the maximum height in the positive being represented in each

case by bare glass. Behind the "red" photograph we place a red medium, such as red glass, which occupies but a small part of the spectrum and is equivalent to the red sensation, and behind the green photograph a green medium, also taking in but a small part of the spectrum, and behind the blue photograph a blue medium, and if the luminosity of *the mixed light coming through the red, the green, and the blue when unshaded by the positives forms white*, we shall have a representation of the spectrum.

Suppose that we were going to reproduce the image of the electric light carbons by combining three distinct photographs together backed by proper media, and that we wished to know what each transparency would look like when illuminated with its proper colour, we can show this in a fairly simple manner. Close in front of the slit of the spectroscope is a lens of such a focus that a sharp image of the carbon points is thrown on the surface of the prism. The prism analyses the colours, and a lens in front of the spectrum collects the coloured rays again and gives us an image on the screen of the carbon points. Placing three slits in the spectrum, we alter their width until the image again appears white at the brightest part. We may substitute three lenses of equal foci for the single lens, and we have three images side by side, which, as just seen when combined together, will give the white image of the crater and the redder image of those parts where the heat is less intense. We can vary this experiment. If we place against the prism a small square made up of circular glasses of different colours, we have the image of the glasses on the screen when the whole spectrum is used. With the slits inserted as before, we also get white light and the colours of the glasses (Fig. 4). The three lenses, also placed before the slits, give the separate images such as we wish to obtain by photographic means.

But how about securing these photographs? Can we find three different photographic plates which will be exactly sensitive to the required parts of the spectrum, excluding all other parts?

It will be seen that the parts overlap (see Fig. 1). Thus the green and red curves overlap, as do also the green and the blue. It may at once be stated that there are no such different kinds of plates to be found. But if we can find one plate which is sensitive to the whole spectrum, we can, by using absorbing media, cut off those portions which are required. Now the ordinary plate, with short exposure, is not sensitive much beyond the blue (see No. 4, Fig. 5), but if we give it a slightly longer exposure it will be found sensitive to the green and yellow as well, and, with a still further exposure, to the extreme red; so that we can use an ordinary plate for the purpose, as it is sensitive, but in vastly different degrees, to the whole spectrum, but we have to cut off all the parts we do not want.

In the spectrum of the light transmitted by an orange glass in the spectrum, we see that the red, yellow and green alone penetrate, and this is the region of the spectrum that the red sensation curve occupies. A blue-green glass cuts off most of the red and the violet, and

this gives the part occupied by the green sensation curve; and so with the blue. Evidently, then, by using the orange, blue-green and blue media for the three photographs of the spectrum, we shall secure three negatives representing, with some degree of exactness, the sensation curves, though the exposures given to each one will be very different. The red will require nearly one hundred times more exposure than the blue, and the green an intermediate exposure. On the screen we have the negatives obtained, and also the positives (Fig. 6). No. 1 was taken through the orange, No. 2 through the green, and No. 3 through the blue screen. The superposed images of these three positives, if backed by red, green and blue light, will give us the spectrum. [*Mr. Ives showed the projection on screen.*] The picture is fairly perfect, and exemplifies what can be done with an ordinary plate.

What I wish to impress upon you is that the screens used for the taking of the three different negatives must each allow a large part of the spectrum to pass, whereas the colour screens used to illuminate the three positives, where the images are superposed, will be more efficient the smaller the part of the spectrum that is used, for if large parts are used the colours will be tinged with white. This is a most important point in three-colour photography.

We have modifications of plates which allow shorter exposures to be given to the green and the red. Cadett's spectrum plate (see No. 1, Fig. 5), for instance, can be utilised for giving equal exposures through a blue, a green and a red medium, when the white light is first toned down to a pale yellow, which, however, still contains all the colours of the spectrum.

Then there are others, such as Lumiere's (see Nos. 2 and 3, Fig. 5), which are sensitive to the green and yellow and red, as well as to the blue, but which exhibit gaps in sensitiveness in the length of the spectrum. These plates can be utilised for photographing colours in nature, though they must fail for photographing the spectrum. But to atone for the gaps, the absorbing media used have to be modified to effect a compromise as it were. Mr. Ives, who is the inventor of the Photochromoscope, and who is present this evening to show some of his wonderful results, has kindly lent me a slide showing the screens with which to take the three negatives with Lumiere's pan-chromatic plates.

By modifying the screens, any plate which is sensitive to the yellow and orange may be utilised, even though it is not at all, or only very feebly, sensitive to the red. For be it remembered that the colours in nature are not pure spectrum colours. A red, for instance, such as this glass, contains an appreciable amount of yellow in it, and the yellow will impress the plate sufficiently to answer the purpose of obtaining the requisite density to represent the red. Of course, if there were a red of a spectrum simplicity, it would not impress the plate. Except with the ordinary plate such as I have used, there has to be a series of compromises. Again, it must be

remembered that the negatives obtained have to be converted into positives; and further, that for effective working all three negatives must, in ordinary circumstances, be obtained on one plate, and by one length of exposure. Mr. Ives has worked this out with a wonderful degree of exactitude, and his camera can be examined in the Library after the lecture to show the manner in which he has accomplished it. He has aimed at getting a perfectly graduated negative with each colour screen, and in the positives from them there are absolutely transparent parts, thus securing the maximum brilliancy.

These positives are backed by colour screens chosen to imitate the three colours used by Clerk Maxwell in his colour mixture equations.

Now, having explained the principles of the three-colour photography, I will get Mr. Ives to throw three or four of his pictures on the screen, and I have to thank him for his ready acquiescence in responding to my request for his help to-night. It is a pleasure to acknowledge that Mr. Ives has been the pioneer in this colour photography, working on exact principles, which he has applied to practical purposes.

In connection with the same subject we have the more recent process due to Professor Joly, of Dublin. Instead of taking three negatives and from them three transparencies, he combines the three in one. To take his negatives he observes the same general principle as that already enunciated, for he places in contact with his sensitive plate a screen consisting of a series of orange, green and blue lines ruled on white glass and touching one another; each line is $\frac{1}{300}$ of an inch in width. Every third line is a colour screen in orange, the next line and third from it a green, and the remaining ones blue. To tone down the excess of blue in daylight, the lens is covered with a pale yellow screen. The one negative is therefore a mixture of three colour negatives. A transparency is taken in the usual way, and by placing in contact with it a screen ruled in red, green and blue, the red lines occupying the position of the orange line in the taking screen, the green the green, and the blue the blue, we have a representation in colour of the original object. [The taking screen, the viewing screen, and a negative and a positive were shown, as also a selection of finished pictures taken by Professor Joly.]

Suppose we take one set of Ives' negatives and make duplicate prints from them in bichromated gelatine, we should get, on development, transparent gelatine of different thicknesses. Where the light had most acted the film would be thickest, and where no light had acted the gelatine would be practically absent, and the intermediate intensities of light acting would give intermediate thicknesses of gelatine. We may dye one set of gelatine prints with a transparent red, a transparent green and a transparent blue, to imitate the viewing screens, and if these were superposed we should find a very different result to that obtained by triple projection. What ought to be black

would be white, and what ought to be white would be black, and the colours shown would be complementary. A yellow *by projection* we know is caused by a full mixture of red and green light, but by superposition the red would cut off all the blue-green light, and the green all the purple light, and the image would be nondescript, and so with other colours. If we dyed the second set of gelatine negatives with the complementary colours a very different state of things would be found. Taking the yellow, for example, which in the "red" and "green" negatives would be shown by great opacity and in the blue by total transparency, the part of the print in the "red" negative would be represented by very feeble sea-green, and that in the green by very feeble purple, whilst in the blue negative it would be represented by full yellow. From the first two the only light penetrating would be the blue, and the only colour reaching the eye after passing through the third gelatine transparency would be the yellow, and so for other colours. Hence, for superposed pictures, either for the lantern or for prints, the complementary colours to those of the viewing screen should be used. This is the foundation of most of the three-colour printing processes extant.

We have three such prints in the three colours, lent me by Messrs. Waterlow & Sons, and here they are superposed to make the final coloured print. This triple printing can be done either by lithography or by printing in colour from gelatine films.

I have endeavoured, by a brief sketch, to show you the principles on which photography in colour has been based—principles which are truly scientific—and which my friend, Mr. Ives, has adopted in all his work. The rule-of-thumb man, who works according to his own sweet will, is a man to whom a certain amount of success will be given, but it is to him who works on the true principles of science that the highest success must accrue. I have endeavoured to show you that Young's theory of Colour Vision, though a theory, is yet of supreme use in this particular branch of industry. I have purposely omitted to mention many of the glaring mistakes which have been made by the rule-of-thumb man, both at home and abroad, in regard to it.

[W. de W. A.]

WEEKLY EVENING MEETING,

Friday, April 22, 1898.

Sir James Crichton-Browne, M.D. LL.D. F.R.S. Treasurer and Vice-President, in the Chair.

W. H. M. Christie, Esq. C.B. M.A. F.R.S. Astronomer Royal.

The Recent Eclipse.

After the failure through bad weather, which was the fate of nearly all the expeditions in the eclipse of 1896, widely spread though they were from Norway through Siberia to Japan, it was felt that every effort should be made to occupy as many stations as practicable along the track of the recent eclipse of 1898, January 22, which, starting from Equatorial Africa, crossed India and ended in the Chinese Empire. It was at first hoped that it would have been possible to send one of the observing parties to Africa, but it was not found practicable to establish stations in Somali Land, and thus the field was narrowed to the shadow track through Central India. Practically the choice of stations was confined to the neighbourhood of the places where the various railway lines intersect the central line of the shadow track, and of these the more westerly had the advantage of giving slightly longer duration of totality. Fortunately the weather chances were unusually favourable in the recent eclipse, the prospect of clear sky at that time of year in Central India being so great that Mr. Eliot, the Meteorological Reporter for India—to whom we are so much indebted for his collection of the weather statistics—is said to have put the chances at 25 to 1 in favour of a fine day for the eclipse.

The Joint Eclipse Committee of the Royal and Royal Astronomical Societies arranged for four parties of observers:—

1. Sir Norman Lockyer, whose main equipment was prismatic cameras, at Viziadrug.

2. Professor Turner and myself to take large and small scale photographs of the corona. Karad (south of Poona) was originally selected, but owing to the outbreak of plague there the Bombay Government advised its abandonment, and Sahdol (a station further east and with somewhat shorter duration of totality), on the railway connecting Katni and Bilaspur, was substituted.

3. Captain Hills and Mr. Newall. Slit spectroscopes and photographs of corona at Pulgaon.

4. Dr. Copeland to take large scale photographs of the corona with a lens of 40 feet focus.

Besides these there was a party under the auspices of the British Astronomical Association at Talni, consisting of Mr. and Mrs.

Maunder, Mr. Thwaites and Mr. Evershed; and the Viceroy of India occupied a station in the neighbourhood of Buxar, near Benares, with a large party, which included Mr. Pope, of the Indian Survey, who took photographs of the corona. Mr. Michie Smith, Government Astronomer at Madras, and a party of observers occupied a station at Sahdol. There were also three parties of observers at or near Jeur, to the S.E. of Poona, viz. the American astronomers, Professor Campbell and Mr. Burckhalter, taking large-scale photographs of the corona; the Japanese astronomers, also taking photographs of the corona; and Professor Naegamvala, of the Poona College of Science, with a large party of observers.

Admirable arrangements were made by the Government of India, special facilities were accorded by the Indian railway companies, and valuable assistance was rendered by the Admiralty to Sir Norman Lockyer, H.M.S. 'Melpomene' being detailed for his party.

I will now pass on to the consideration of the results obtained in this eclipse. These may be classified as

I. Photographs of the Corona.
II. Spectroscopic Observations.
III. Polariscopic Observations.
IV. Photographs of Partial Phase for position of the Moon.
V. Miscellaneous.

I. *Photographs of the Corona.*

A special feature of this eclipse was the number and the variety of instruments which were utilised to obtain large-scale photographs of the corona, on a scale of about 4 inches to the sun's diameter. Professor Campbell, Dr. Copeland and Mr. Michie Smith had each a telescope 40 feet in length, the form of mounting this long tube being different in each case. Mr. Michie Smith pointed his tube to the pole, and reflected the sun's rays into it by a plane mirror turning about a polar axis—what is known as a polar siderostat. In this form the image rotates slowly as the mirror turns with the diurnal movement, and the plate (15 inches square) should therefore be rotated slowly to get an absolutely fixed image. Mr. Michie Smith had arranged for this, but did not receive the apparatus in time. For short exposures of a few seconds, however, the rotation would hardly be appreciable.

Professor Campbell mounted his tube on a timber framework, so as to point to the position of the sun at mid-totality, and applied clockwork to move his plate, which was 17 inches by 14 inches. Dr. Copeland used a fixed mirror to reflect the rays into his telescope, which was mounted horizontally, and moved his plate (18 inches square) by clockwork.

The instrument I used was on a different principle, the large scale being obtained by applying a concave lens (placed at the proper point within the focus) to magnify the image formed by an object-

glass of comparatively short focal length, and thus the total length of the telescope is kept within manageable dimensions—11 feet in my case, instead of 40 feet as in the ordinary form. This combination is in fact an application of the well-known Barlow lens, and forms what has since become known to photographers as the tele-photo form. My instrument was the photographic telescope, of 9 inches aperture and 8 feet 6 inches focal length, presented some years ago to the Greenwich Observatory by Sir Henry Thompson, and to this was applied a tele-photo concave magnifier of 3 inches diameter giving a solar image 4 inches in diameter, with a field of view of 10 inches diameter ($2\frac{1}{2}$ diameters of the sun).

The same so-called tele-photo form was also used for two smaller telescopes of 4 inches aperture which gave a solar image $1\frac{1}{2}$ inches in diameter, each of these being combined with another photographic telescope of 4 inches aperture and 62 inches focus (known as the Abney lens) in a double tube. Thus each "double tube" gave two photographs of the corona, large and small scale, the former to show detail and the latter to give as great extension as possible. These "double tubes" were first used in the eclipse of 1893. In the recent eclipse they were effectively employed by Professor Turner at Sahdol, and by Captain Lenox Conyngham, R.E., under Captain Hills' direction, at Pulgaon.

Another important feature in the instrumental equipment was the cœlostat, a form of mounting a mirror devised by M. G. Lippmann in 1895, and successfully used in the recent eclipse at three stations (Sahdol, Pulgaon and Viziadrug); the observers being indebted to Dr. Common for designing the instruments, supervising their construction and, most important of all, supplying the large plane mirrors (16 inches in diameter).

Another new departure of much interest was Professor Burckhalter's device for giving to each part of the corona the exact exposure best suited to its brightness. The brightness of the inner parts near the sun's limb is so overpowering, as contrasted with the faintness of the outer streamers, that widely different exposures are required to bring out their respective details, and thus it is necessary to take a series of photographs, the combination of which should represent the whole phenomenon.

Professor Burckhalter arranges to get the whole on one plate by giving exposures rapidly increasing from the sun's limb to the edge of the field, this being effected by means of a slit of peculiar form in a metal screen which rotates rapidly in front of the photographic plate, and thus gives intermittent exposures of duration depending on the width of the slit, which increases rapidly from the sun's limb outwards.

Another interesting instrument was that used by Mr. Thwaites at Talni with a triple object-glass, $4\frac{1}{2}$ inches in diameter, of Cooke's new form adapted both for visual observation and for photography.

Valuable series of photographs of the corona were obtained with

all these instruments, the exposures being so arranged that each series of photographs would give a complete representation of the corona, showing the details in the different parts.

A small-scale photograph of the corona, taken by Mrs. Maunder with a lens of $1\frac{1}{2}$ inch aperture and 9 inches focus on a Sandell triple-coated plate, is remarkable for the great extension of the corona which it shows, one ray in particular being traceable to a distance of nearly 3° from the sun.

II. *Spectroscopic Observations.*

These were made with two classes of instruments:

a. Slit Spectroscopes.

b. Prismatic Cameras.

a. Slit Spectroscopes.—Captain Hills, R.E., using two spectroscopes with two flint prisms and four quartz prisms respectively, fed by a 12-inch heliostat, in combination with two telescopes of $4\frac{1}{2}$-inch and 5-inch aperture respectively, obtained fine photographs of the coronal spectrum and of the flash spectrum at the beginning and end of totality. These latter show clearly the progressive changes from the dark line spectrum of the sun to the bright line spectrum of the chromosphere as the moon covered the sun's disc.

Mr. Newall with a 4-prism spectroscope attempted to determine the relative motion of the corona on opposite sides of the sun in the line of sight, by the displacement of the coronal lines in the spectrum; but unfortunately his attempt to determine the rotation of the corona failed through the faintness of the spectrum at the region photographed, only 8′ from the sun's limb. He, however, succeeded in obtaining a fine photograph of the spectrum of the flash at the end of totality. He also observed the distribution of coronium round the sun's limb with a diffraction grating in front of an object-glass of $3\frac{1}{2}$ inches aperture and 29 inches focus. With this instrument he noted seven bright patches of coronium, three being traced to a distance of 12′ from the sun's limb. Two of these coincided roughly with coronal streamers in the north-east and south-west.

b. Prismatic Cameras.—Sir Norman Lockyer's party at Viziadrug made use of two prismatic cameras, i.e. photographic telescopes, with one or more large prisms placed in front of the object-glass. One of these had an object-glass of 6 inches aperture with two large prisms in front of it, the other was larger, having an object-glass of 9 inches aperture, but with only one prism, so that its dispersion was only about half of that given by the other.

With these instruments valuable series of photographs were obtained at the beginning and end of totality, showing the spectrum of the chromosphere, and during totality for the coronal spectrum. In each case rings represented the various lines of the spectrum, giving the images of the chromosphere or corona surrounding the eclipsed sun as formed by light of the various wave-lengths emitted by it.

Mr. Evershed at Talni also obtained fine photographs of the spectrum of the chromosphere and corona with a smaller prismatic camera.

III. *Polariscopic Observations.*

Professor Turner at Sahdol obtained photographs showing radial polarisation in the coronal streamers, his object being to determine how much of the light of the corona is polarised radially, and consequently due to reflected sunlight.

Mr. Newall made eye observations which indicated strong polarisation of the atmosphere at all points within 30° of the sun, the plane of polarisation being not vertical.

IV.

Photographs of the partial phase for determination of the position of the moon were taken by me at Sahdol, the longitude and local time being accurately determined by Major Burrard, R.E., and Lieut. Crosthwaite, R.E., of the Indian Survey Department.

V. *Miscellaneous.*

A number of drawings of the corona were made by observers at the various stations occupied, but their value would have been much greater if the observers had worked with a stump to represent the gradations of light in the corona, instead of attempting to draw with a pencil an outline of the corona, which has essentially no defined boundary.

[W. H. M. C.]

WEEKLY EVENING MEETING,

Friday, April 1, 1898.

Sir Edward Frankland, K.C.B. D.C.L. LL.D. F.R.S. Vice-President, in the Chair.

Professor Dewar, M.A. LL.D. F.R.S. *M.R.I.*

Liquid Air as an Analytic Agent.

The increasing importance of low-temperature research is shown by the gradual development of the applications of liquid air for scientific and other purposes. The much larger apparatus now used in the production of the liquid enables experiments to be made on a more imposing scale.

Liquid air poured from a tin can, filled by being dipped into a 5-gallon jar filled with the liquid, into a large silver basin heated to redness, remained apparently as quiescent at this high temperature as in cooler vessels, and maintained a spheroidal condition, just like other liquids. The temperature of the liquid air was about $-190°$ C., or 83° absolute, while the vessel in which it was placed had a temperature of 800° C., or 1073° Ab. In other words, between the wall of the silver vessel and the liquid air there was a difference of temperature of 1000° C., 12 times the absolute temperature of the liquid.

Liquid air can be of great service in the qualitative separation of mixtures of gases. With the object of ascertaining the proportion *of any gas in air that is not condensable* at about $-210°$ C. under atmospheric pressure, *or is not soluble in liquid air* under the same conditions, a series of experiments was made with the following apparatus.

A cylindrical bulb of a capacity of 101 c.c., marked B in figure, had a capillary tube sealed into it terminating in a three-way stopcock, as shown at E. The parts marked C and D consist of soda-lime and sulphuric acid tubes for removing carbonic acid and water. The stand marked G holds the large vacuum test-tube into which B is inserted, and which contains liquid air maintained under continuous exhaustion. As this low temperature had to be kept steady from one to two hours, while at the same time the bulb B had to be completely covered with liquid air, it was necessary to arrange some means of keeping up the liquid air supply without disturbing the apparatus. The plan adopted is shown at H, which is a valve arrangement which can be so regulated as to suck liquid air from the large vacuum vessel A, and discharge it continuously along a pipe into the vacuum test-tube G, the latter being kept under good exhaustion. In working the apparatus, the tube I is connected to a gasometer containing 10 cubic feet of air, so that the volume of air condensed in each

experiment may be observed. This was generally from $2\frac{1}{2}$ to 3 cubic feet. If there is a very small proportion of some substance not liquefiable or soluble in liquid air, then we should expect the vessel B would not fill up completely into the capillary tube. This is, however, exactly what does take place. After 40 minutes' cooling,

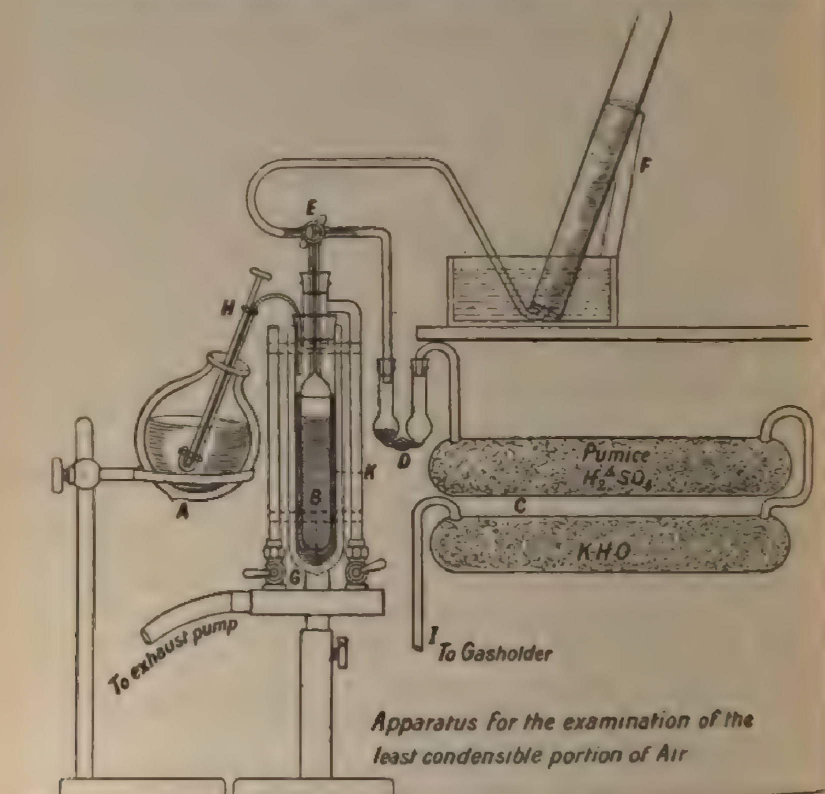


Fig. 1.

the vessel B and the cool part of the tube were filled with liquid. In this experiment some 80 litres of air were condensed, and any accumulated uncondensed matter must have been concentrated in the upper part of the capillary tube, which had a volume of 0·5 c.c. Under the conditions, therefore, the material looked for must be less than 1 part by volume in 180,000 of air.*

* These experiments, along with the succeeding ones on Bath Gas, were all described in a Paper entitled, 'Liquefaction of Air and the Detection of Impurities,' given at the Chemical Society on 4th November, 1897.

To test the working with an uncondensable gas added to air, a volume of 10 cubic feet was taken in the gasholder, and to that 500 c.c. of hydrogen were added. This is in the proportion of less than 1 in 500. Even after two hours' cooling, the tube B could only be filled four-fifths. In order to prove that the gas accumulated in the upper part of B was hydrogen, the three-way stopcock at E was turned, and the temperature allowed to rise, so that the gas was expelled from the evaporation of the liquid air and collected over mercury as shown at F. The gas thus collected was easily combustible and consisted chiefly of hydrogen. The amount of hydrogen was then reduced to 1 part in 1000 of air, and it was found that after one-and-a-quarter hours' cooling, the bulb B had filled to within a half c.c. of the capillary tube. A new sample of air containing 1 part of hydrogen in 10,000 of air, filled the bulb B completely as if it were ordinary air.

It appears from these experiments that 1 part of hydrogen in 1000 of air is just detectable in the form of an uncondensable residue. As the 80 litres of air condensed contained some 80 c.c. of hydrogen, it appears that 100 c.c. of liquid air at from $-200°$ to $-210°$ C. had dissolved nearly all this gas; in fact, that 20 c.c. of hydrogen at the low temperature is dissolved in 100 c.c. of liquid air, and can only be detected by examining the first sample of gas boiled off or extracted by lowering the pressure on the liquid. In the paper on 'The Liquefaction of Air and Research at Low Temperatures,'* it was shown that if hydrogen containing a small percentage of oxygen were employed for the purpose of getting a hydrogen jet, the liquid collected from it was oxygen, containing, however, so much hydrogen dissolved in it that the gas coming off for a time was explosive.

Coal gas, which is a mixture of hydrogen, marsh gas, carbonic oxide, and various illuminating gases and impurities, after passing through a coil of pipe surrounded with solid carbonic acid for the purpose of condensing the vapours of benzol, naphthalene, &c., when supplied to a tube similar to B, surrounded by boiling liquid air, gave a liquid and gaseous portion at the lowest temperature. It was possible to condense in this way all the constituents of coal gas, and to separate them after liquefaction by fractional distillation, except carbonic oxide and hydrogen.

Ultimately, however, the carbonic oxide would be condensed, and hydrogen be left alone in the gaseous state. Similarly, any gas less easily condensed than air could be separated from a mixture of the same with air. Hydrogen present in air to the extent of one in a thousand is just detectable, but smaller quantities escape direct observation owing to solution in the liquid. In order to press this inquiry a little further, some natural gas known to contain a different constituent, like helium, suggested itself as being worthy of trial. Lord Rayleigh's analysis of the gas from the King's Well, at Bath, gave

* Proc., 1895, vol. xi. p. 221.

1·2 part of helium per 1000 volumes, so that it seemed admirably adapted for such experiments. By the kind permission of the Corporation of Bath, an abundant supply of this Gas was obtained for experimental purposes.

In a paper read before the Royal Society on December 19, 1833,* by Dr. Daubeny, Professor of Chemistry at Oxford University, on the 'Quantity and Quality of the Thermal Springs of the King's Well in the City of Bath,' there are some interesting details. Dr. Daubeny's experiments extended over a month, and he estimated the volume of gas given off as from 80 to 530 cubic inches per minute (average 264). The temperature of the water of the King's Well was 115° Fahr., and the amount of water per minute was equal to 126 gallons. The average volume of gas was 240 cubic inches per minute. The gas was collected from an area of 20 feet in the centre of the bath; the maximum amount of gas obtained was 300 cubic inches, while the minimum quantity was 194 cubic inches per minute. Calculated at the rate of evolution of 250 cubic feet per day for 5000 years, then the whole gas given off amounts to 456 million cubic feet.

Thirty-two years after Daubeny's experiment Professor Williamson made a more elaborate examination of the Gases of the King's Well. In B.A. Reports, 1865, he gives the following as the volume composition of the gas:—

Carbonic Acid.	Oxygen.	Marsh Gas.	Nitrogen.
2·948	0·54	0·18	96·33
3·056	0·617	0·216	96·11

Williamson used a funnel 3 ft. 9 in. in diameter to collect the gas, and obtained a quantity equal to a rate of 112 cubic feet per day. This is only about half the amount Daubeny collected, and may be explained by the great alterations made in the bath itself between the dates of the observations.

In passing, it is interesting to note the general character of the saline constituents of the spring, as the most probable hypothesis is that the argon and helium come from the rocks traversed by the water. The following analysis was made by Dr. Attfield.

	Grs. per Gallon.
Carbonate of calcium	7·8402
Sulphate of calcium	94·1080
Nitrate of calcium	·5623
Carbonate of magnesium	·5611
Chloride of magnesium	15·2433
Chloride of sodium	15·1555
Sulphate of sodium	23·1400
Sulphate of potassium	6·7020
Nitrate of potassium	1·0540
Carbonate of iron	1·2173
Silica	2·7061
	168·2898

* Royal Soc. Proc., vol. iii. p. 254.

Ramsay, the geologist, estimated the mineral ingredients obtained from this source in one year would equal a square column 9 feet in diameter and 140 feet high. Roscoe detected by spectroscopic examination the presence of lithium, strontium and copper. The sample of Bath gas examined by Rayleigh contained scarcely any oxygen and but little carbonic acid. The weight in a given globe of the N from the Bath gas (2·30522) is about half-way between that of chemical nitrogen (2·299) and "atmospheric" nitrogen (2·3101), suggesting that the proportion of argon is less than in air, instead of greater, as had been expected. Later experiments by Rayleigh proved that this nitrogen contained helium as well as argon.

The sample of gas from the Bath Spring was treated exactly in the same way as the hydrogen mixtures before referred to. During liquefaction there was a marked difference in the appearance of the liquefied gases, for while the hydrogen and air mixtures on condensation gave clear transparent liquids, the product from the Bath gas was turbid, and a precipitate gradually formed which by transmitted light looked yellowish-brown. The yellowish-brown precipitate is a hydro-carbon, probably of the petroleum series, having a marked aromatic smell, and is liquid at the ordinary temperatures. It was probably this gas which Professor Williamson gave as marsh gas in his analysis. Further research will be made on this substance. Another peculiarity of the liquid nitrogen obtained from Bath gas is that, on examining it with a spectroscope, even through a thickness of two inches of liquid, no trace of the characteristic oxygen absorption spectrum could be obtained. In all other attempts to make nitrogen for liquefaction on the large scale, oxygen could always be detected in the liquid by means of its absorption spectrum. Another phenomenon was that the gas from the King's Well could not be entirely condensed by refrigeration with liquid air boiling *in vacuo*. After the cooling had continued for some time, the gas ceased to flow into the condensing vessel, and the upper part of the vessel was occupied by a gas that would not undergo liquefaction at the temperature together with substantially liquid nitrogen saturated with the said gas.

About 70 litres of the Bath gas were condensed, certainly the largest quantity of this gas ever subjected to chemical examination. This was boiled off, and as by accident too much nitrogen had volatilised along with the gas, oxygen was added, and the mixture sparked over alkali, to get rid of the excess of nitrogen. The sample of gas directly collected from the liquid nitrogen contained about 50 per cent. of helium. During the sparking the helium lines were well marked (along with others, the origin of which must be settled later), and a vacuum tube filled with the product of the sparking gave a splendid spectrum of the gas. The recorded unknown lines in the Bath helium were subsequently detected along with helium in the more volatile portion of liquid air.* Eight months after my paper to the Chemical Society, and some two months after this address was

* See 'Nature,' vol. lviii. p. 570, Letter of Sir William Crookes, Oct. 11, 1898.

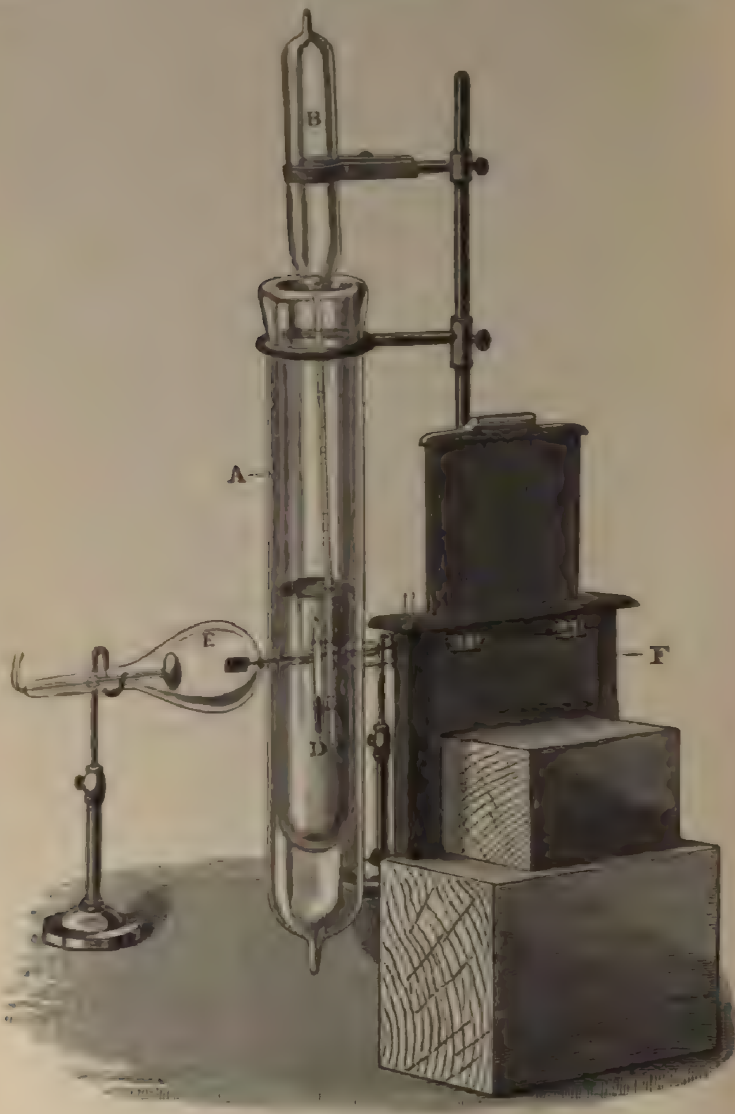


Fig. 2.

A, glass vacuum vessel, containing liquid air. B, tube of argon. C, tube of liquid chlorine. D, tube of metallic sodium. E, Röntgen X-ray bulb. F, photographic plate behind sheet aluminium.

delivered, the same material was found by Professors Ramsay and Travers to exist in argon, and has been recognised and named by them Neon, a new element.

It is, therefore, possible to separate helium from other gases by liquefaction when it is only present to the extent of one part in one thousand. From this it would appear that helium is less soluble in liquid nitrogen than hydrogen in liquid air, and is of greater volatility than the constituents of the other gases which were condensed. If the sample of the uncondensed gas from the first liquefaction of the Bath gas were again treated in the same way, a much more concentrated specimen of helium could be obtained. Provided helium were wanted on a large scale, then a liquid air apparatus, similar to that in use at the Royal Institution, transported to Bath, and worked with the gas from the King's Well, could be made to yield a good supply, as the gas contains 1·2 parts in 1000.

Argon, which is present in the proportion of 1·4 per cent., condenses with the nitrogen; but if the liquid be allowed to slowly boil away, a residuum may be obtained containing about 7 per cent. of argon. Argon, when frozen, solidifies to a perfectly clear glass.

Absorption of Röntgen Radiation at Low Temperature by Different Bodies.

The transparency of bodies to the Röntgen radiation is an interesting study, although we are not in a position to draw definite conclusions from the results. As a general fact we know the opacity of elements in the solid state increases with the atomic weight.

In the experiments small tubes of the same bore were filled respectively with liquid argon and chlorine, potassium, phosphorus, aluminium, silicon and sulphur, and exposed at the temperature of liquid air (in order to keep the argon and chlorine solid) in front of a photographic plate shielded with a sheet of aluminium to an X-ray bulb (see Fig. 2). The order of increasing opacity of the shadow of each substance was observed, and the sequence in the list given above represents the results. A tube containing silicon was a little more transparent than the potassium or chlorine. Sodium and liquid oxygen and air, nitrous and nitric oxides proved much more transparent than chlorine. Tubes of potassium, argon and liquid chlorine presented no very marked difference of density on the photographic plates.

From these experiments it would appear that argon is relatively more opaque to the X-rays than either oxygen, nitrogen, or sodium, and that it is on a level with potassium, chlorine, phosphorus, aluminium and sulphur. This may be regarded as supporting the view that the atomic weight of argon is twice its density relative to hydrogen.

Thermal Transparency at Low Temperatures.

Pictet, after an elaborate investigation, concluded that below a certain temperature all substances had practically the same thermal

transparency, and that a non-conducting body became ineffective at low temperatures in shielding a vessel from the influx of heat. Experiments, about to be detailed, however, prove that such is not the case, the transference of heat observed by Pictet appearing to be due not so much to the materials themselves as to the air contained in their interstices. Good exhaustion in the ordinary vacuum vessels used in low temperature work reduces the influx of heat to one-fifth of what is conveyed when the annular space of such double-walled vessels is filled with air. If the interior walls are silvered, or excess of mercury is allowed to remain, the influx of heat is diminished to one-sixth of the amount entering without the metallic coating. The total effect due to the high vacuum and silvering is to reduce the ingoing heat to one-thirtieth of the original amount, i.e. roughly, to $3\frac{1}{2}$ per cent.

By filling the annular space between the walls of several similar vacuum vessels with various substances, and exhausting them all to the same low pressure, large differences in the thermal isolation were observed. The rate of evaporation of equal volumes of liquid air contained in the respective vessels measures the rate of influx of heat. Moreover, it appears that what might be called under the circumstances the thermal transparency of some materials diminished at very low temperatures instead of increasing, as had been asserted. Thus, of two vacuum tubes (one simply exhausted, and the other having powdered carbon in the vacuum space), the latter, at low temperature, was the most efficient preserver of liquid air, showing that the carbon diminished the radiation and gas convection. But when the vacuum was destroyed and air admitted into the space, the liquid air in the carbon tube boiled off much more vigorously than that in the simple tube, indicating that at ordinary temperatures carbon allowed more heat to pass than did air.

In conducting these experiments, generally sets of three double-walled glass tubes, as nearly identical in size and shape as possible, were mounted on a common stem, and two out of the three filled with different kinds of powders, while the third is left empty as a standard for comparison (Fig. 3). In this way each set had the same vacuum, and as intercommunication between the tubes after sealing off from the pump was left free, any deterioration in the vacuum on keeping affected all three vacuum tubes to the same extent.

The preparation of such tubes entails enormous labour, because it takes days of exhaustion with a mercurial pump to extract the occluded gases, even at as high a temperature as the glass would stand. Before beginning the experiment, the vacuum tubes of each triple set were filled with liquid air, and allowed to stand half an hour in order to get the heat conduction in the porous mass into a steady state. The tubes after this treatment were filled to the same height, and the relative times required to distil off the same volume of liquid air from each observed—the outer surface of the vacuum tubes being maintained at a steady temperature by immersion in a large vessel of water. Neither the tubes nor the shape of the vacuum space

in each were absolutely identical, so that the results are simply comparative. The general ratio of heat propagation found for two substances when different sets of double-walled tubes of about the same form and proportion were compared, remained substantially

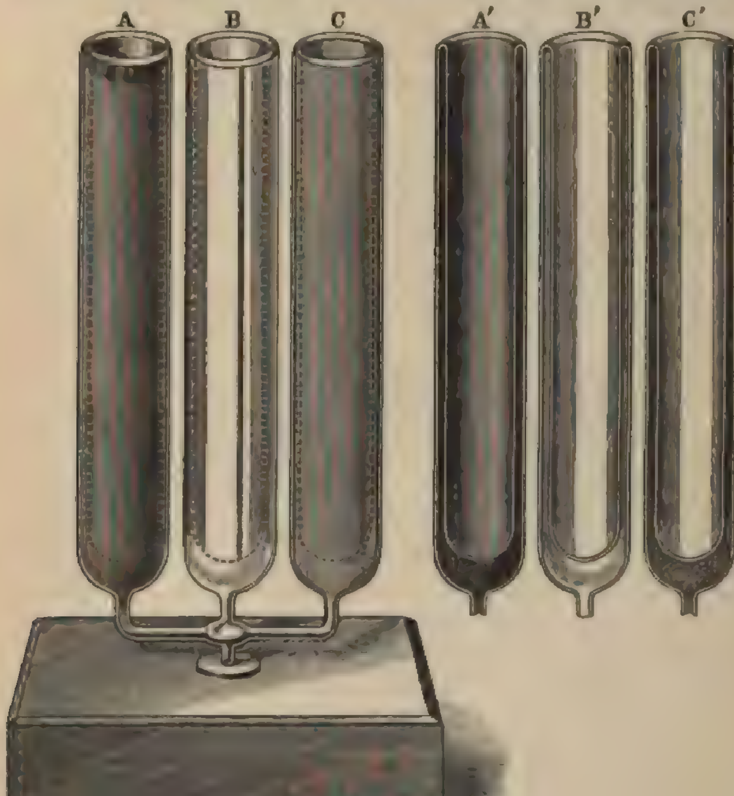


FIG. 3.—Three tubes blown on to one stem, so that the exhaustion in each would be identical.

A, filled with lampblack between the inner and outer tubes. B, annular space left empty. C, filled with silica between the tubes. A′, B′, C′, the same tubes in section.

constant when a high vacuum was reached. A confirmation of the results was generally made by noting the time required to evaporate the whole of the air from each tube. The annular vacuum space had generally a thickness of 4 to 5 mm., and was in each case completely filled up with the solid. In reality, however, the absolute

fraction of the space filled by the solid did not exceed one-half. The effect of any considerable inequality in the thickness of the non-conducting powders was ascertained by comparing two vacuum tubes, one having double the thickness of vacuum space of the other, and each then filled with precipitated silica. Taking the unfilled vacuum tube as the unit for comparison as described above, then the single thickness of silica increased the insulation to 6 and the double thickness to 8. The following table contains the results of a number of experiments with triple sets of double-walled tubes filled with different substances, when exhausted and unexhausted. The results are expressed in the relative times required to volatilise the same small volume of liquid air from each tube. This is most readily done, after filling each tube with the same volume of liquid air, by noting the time required to fill a given vessel standing over the pneumatic trough with the gaseous air distilling off.

In each triple set the unit taken for comparison is the time value of the free vacuum spaced tube.

	Vacuum.	Atmospheric Pressure.
Empty Tube	1	1·0
Charcoal	5	0·7
Magnesia	2	0·6

	Vacuum.	Air.
Empty Tube	1	1·0
Lampblack	5	0 7
Silica	4	0·7

	Vacuum.		Vacuum.
Empty Tube	1	Empty Tube	1
Graphite	1·3	Lampblack	4
Alumina	3·3	Lycopodium	2·5

	Vacuum.		Vacuum.
Empty Tube	1	Empty Tube	1
Calcium carbonate	2·5	Barium carbonate	1·3
" fluoride	1·25	Calcium phosphate	2·7

	Vacuum.		Vacuum.
Empty Tube	1	Empty Tube	1
Phosphorus (amorphous) ..	1	Lead oxide	2
Mercuric iodide	1·5	Bismuth oxide	6

From these experiments it will be seen that silica, charcoal, lampblack and oxide of bismuth all increase the insulation to 4, 5 and 6 times that of the empty vacuum space. In tubes generally which did not reach such a high vacuum the relative insulating effect of these powders could be raised as much as 1 to 8 or 1 to 10. In this case the influx of heat per unit of time in the vacuum tube which did not contain any finely divided powder was necessarily much greater. As the chief communication of heat is by molecular bombardment the fine powders must shorten the free path of the gaseous molecules, and the slow conduction of heat through the porous mass must make the conveyance of heat energy more difficult than when the gas molecules could directly impinge upon the out

glass surface maintained at a higher temperature. To separate the true conduction from the radiation and the gas motion would require far more elaborate experiments, but these are sufficient to prove that the presence of certain finely divided solids in the high vacuum space

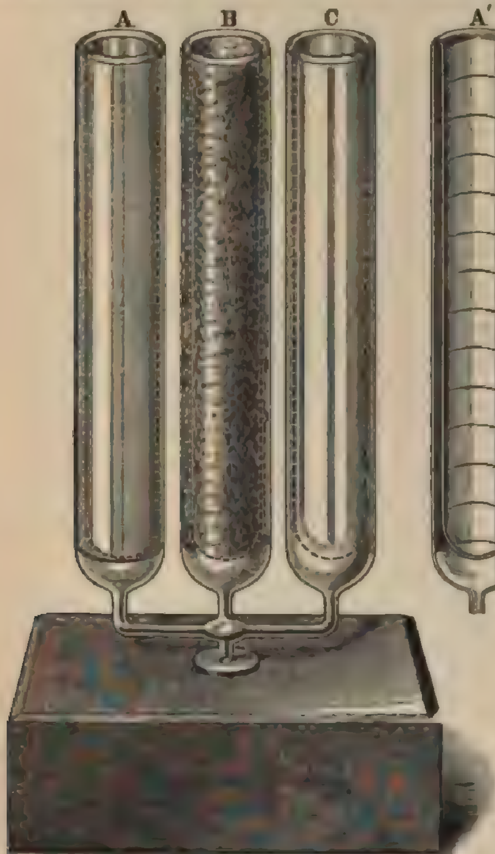


FIG. 4.—Three tubes blown on to one stem, similar to Fig. 3.

A, vacuum space having three turns of gold paper, gold outside. B, vacuum space having some pieces of gold leaf put in so as to make contact between walls of vacuum tube. C, vacuum space empty. A′, B′, C′, the same tubes in section.

of the vessels used in low temperature research improves the heat insulation, while in the presence of air the same bodies facilitate the transference of heat. This is the explanation of Pictet's apparently extraordinary results.

In no case was the diminution of the influx of heat, in the case of

the use of finely divided solids, ever so effective as a high vacuum, in an empty tube, the glass surfaces being silvered. This is seen by reference to results recorded in Tables Nos. 1, 2 and 3, where the insulation is increased in the proportion of more than 1 to 7, which is decidedly better than anything reached by the use of powders.

It will be noted that the use of silica and charcoal to fill up the annular spaces between the walls of these silver-coated vacuum vessels has produced very different results from those recorded in the former experiments with plane glass surfaces. Instead of the heat insulation being increased from 4 to 5 times by the use of such powders, it is now only very slightly benefited. This suggests that the finely divided solid affects chiefly the combined radiation and conduction factors.

A further set of experiments was made with similar vacuum tubes, replacing the powders by metallic and other septa (Fig. 4). Various papers coated with metallic powders in imitation of gold and silver which are in common use, were compared with black paper and a comparison made between the use of sheet lead and aluminium, all under similar conditions.

The following tables express the comparative results of the different experiments.

(1)

Vacuum space empty, not silvered	1
Same space unexhausted	0·25
Vacuum space empty, silvered on both surfaces	7·4

(2)

Vacuum space empty, silvered on inside surfaces	1
Silica in silvered vacuum space	1·1

(3)

Empty silvered vacuum	1	
Charcoal in silvered vacuum	1·25	
Vacuum space unsilvered		1
" silvered inside		5
" in annular space with glass test-tube silvered ..		6

(4)

Vacuum space empty	1
Three turns silver paper, bright surface inside	4
Three turns silver paper, bright surface outside	4

(5)

Vacuum space empty	
Three turns black paper, black outside	
Three turns black paper, black inside	

(6)

Vacuum space empty	1
Three turns gold paper, gold outside	4
Some pieces of gold leaf, put in so as to make contact between walls of vacuum tube	0·3

(7)

Vacuum space empty	
Three turns, not touching, of sheet lead	
Three turns not touching, of sheet aluminium	

The experiments show that liquid air can be conveniently us to study many important problems of heat transmission.

Photographic Action at the Temperature of Liquid Air.

In a former lecture on Phosphorescence and Photographic Action, it was shown that photographic action was reduced by 80 per cent. at the temperature of $-182°$ C. It was further proved that a sensitive film was still comparatively active at the temperature of $-210°$ C. Experiments in this direction have been continued at different times.

In these new experiments the source of light was respectively a 16 candle-power lamp, a magnesium and cadmium spark discharge, and a Röntgen bulb. Small dark slides were prepared having a circular hole. One was placed in liquid air, and another simultaneously exposed for the same time at the ordinary temperature (Fig. 7). They were developed together, and the density of the image observed (Fig. 5).

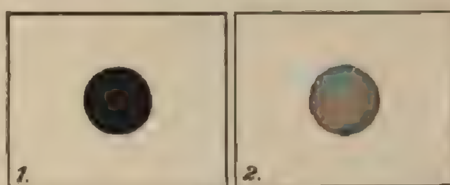


Fig. 5.

1, photographic film exposed at ordinary temperature. 2, photographic film cooled in liquid air during exposure.

Both were exposed for the same length of time, and both were developed together.

Fig. 6.—Ultra-violet spectrum of spark discharge.

1, on film at ordinary temperature. 2, on film cooled in liquid air.
Both exposed for the same length of time and then developed together.

Distance of Plates from Source of Light giving the same Photographic Intensity.

Source of Light.	Cooled Plate.	Uncooled Plate.	Ratio of Intensities at Balance.
	in.	in.	
16 candle-power lamp	20	50	1 to 6
Ultra-violet spark magnesium and cadmium	22½	90	1 to 16
Röntgen bulb	10	24½	1 to 6

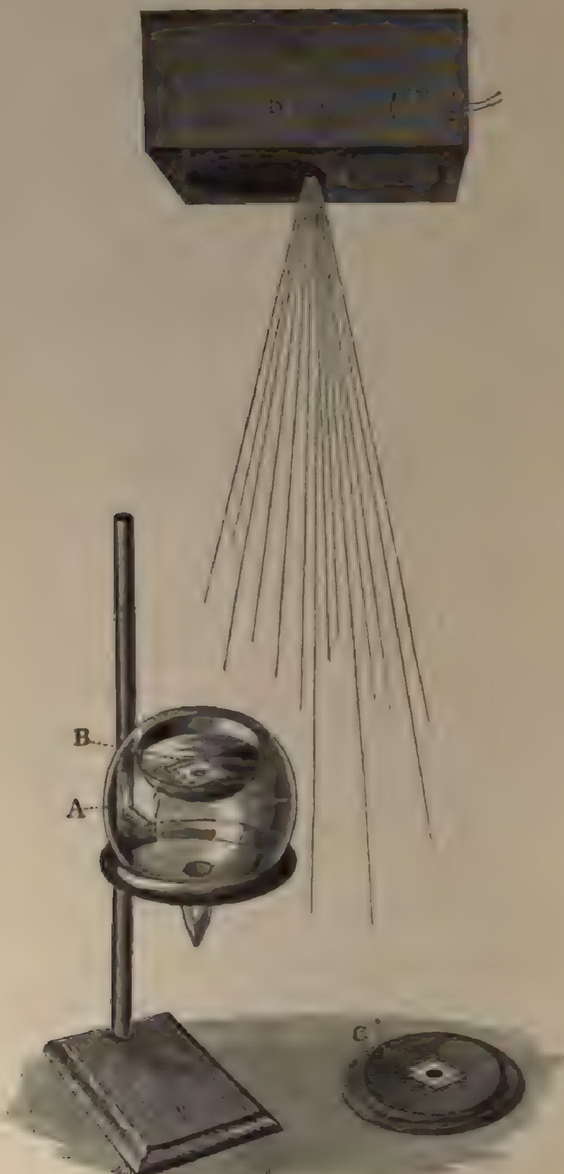


FIG. 7.

A, vacuum cup with liquid air, into which is placed a photographic film in a small metallic slide having a hole in the centre. C, a metallic slide, holding a photographic film, which is exposed at ordinary temperature.

Both of these are exposed to the light from a 16 candle-power lamp D, contained in a box. The light is diminished or increased by the diaphragm at E.

Further trials were made by bringing the cooled plate nearer to the source of light until finally a position was found where the very feeble photographic impression that appeared on both plates had the same density. In this position the relative distances of the plates from the source of light were measured. This mode of conducting the photographic comparison of the hot and cold plates gets over the difficulty of variation in the intensity of the source of light. From these experiments it would appear that when cooled to the temperature of liquid air both the incandescent lamp and the Röntgen radiation were reduced to 17 per cent. of their photographic action at the ordinary temperature; whereas the ultra-violet radiation was reduced to about 6 per cent. This marked increase in the inertia of the photographic plate at low temperatures for the short wave-lengths cannot be explained by the absorption of liquid air, for such radiation as this is small for a thickness of 10 to 20 mm. of the liquid. It is possible that the ultra-violet radiation is dissipated by the photographic film at low temperatures to a greater extent than with ordinary light, through absorption and subsequent emission as a phosphorescent glow. It would seem probable that if the plate could be developed at these low temperatures no action would be apparent, and that it is during the heating up after the low temperature exposure that the photographic action on the film takes place through an internal phosphorescence. This possibility must make us cautious in drawing inferences as to possible chemical action at low temperatures.

A more elaborate study of photographic and phosphorescent effects at low temperatures would add much to our knowledge of the chemical and physical actions of light.

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